The Journal

OF THE

Royal Aeronautical Society

A Monthly Illustrated Magazine devoted to all subjects connected with the Navigation of the Air.

EDITED FOR THE

COUNCIL OF THE ROYAL AËRONAUTICAL SOCIETY

BY

J. LAURENCE PRITCHARD, Hon. Fellow.

VOL. XXVII.

1923.

Printed by the Lewes Press, Ltd., High Street, Lewes, and published by the Royal Aëronautical Society, 7, Albemarle Street, Piccadilly, London, W.1.

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The Journal of the Royal Aeronautical Society

(Founded 1897 in succession to the Annual Reports)

Edited for the Council by J. LAURENCE PRITCHARD, Fellow

No. 145 JANUARY 1923 Vol. XXVII

NOTICES

Journal

Members will notice that several changes have been made in the present issue of the Journal. In the first place the Council feel that the title, "Aeronautical Journal," did not sufficiently indicate that it is the official publication of the Society, and it has therefore been decided to alter it to "The Journal of the Royal Aeronautical Society," which has the advantage of retaining the characteristic of the old title. There have been at the same time certain changes made in the type and *format* of the Journal to bring it more into line with other similar scientific and technical journals.

Election of Members

The following members were elected at a meeting of the Council held on December 19th :—

Associate Fellow.—A.A.S.El. Kirdany.
Students.—H. J. Penrose and H. Sommer.

International Air Congress, London, 1923

An International Air Congress will be held in London from June 25th to 30th, under the auspices of the Air Ministry, the Royal Aeronautical Society, the Royal Aero Club, the Air League and the Society of British Aircraft Constructors. H.R.H. the Duke of York has agreed to become President and Lord Weir Vice-President of the Congress and the Duke of Sutherland (Under-Secretary of State for Air) is Chairman of the Main Committee. The Society's representatives on the Committee are Lieut.-Colonel O'Gorman (who is one of the Vice-Chairmen), Professor Bairstow, Mr. Griffith Brewer and Lieut.-Colonel A. Ogilvie. The Secretary of the Society has been appointed General Secretary, and the Society has been entrusted with the organisation and administration of the Congress, which has its offices at 7, Albemarle Street.

Students' Section

The following programme of papers for Students' Meetings has been arranged for the remainder of the Session :—

1923.

Jan. 25.—"Discussion on English and German Methods of Estimating Aeroplane Performance," F. Radcliffe, B.Sc. Chairman, Mr. Harris Booth.

Feb. 8.—(Title to be announced later) W. L. Le Page.

,, 22.—"The High Lift Wing," T. A. Kirkup. Chairman, Major Gnosspelius.

Mar. 8.—"Air Transport," J. D. Campbell.

,, 22.—(Title to be announced later) S. H. Evans.

Library

The following books have been received and placed in the Library:—"Practical Applications of X-Rays," by C. W. C. Kaye; "Les Hélicoptères," by W. Margoulis; "Mechanical Testing," by Batson and Hyde; "Discoveries and Inventions of the Twentieth Century," by E. Cressy; "Technological Papers of the Bureau of Standards," No. 211, by the Department of Commerce, Washington, D.C.; "Fatigue of Metals," by Professor C. E. Stromeyer; "A Bundle of Meteorological Paradoxes," by W. J. Humphries (Smithsonian Institution); "Rhythm in Nature," by F. W. Flattely (Smithsonian Institution); and "Who's Who in Engineering."

Arrangements for the Month

Jan. 4, 5.30 p.m.—Royal Society of Arts. Professor Junkers, "Metal Aeroplanes."

,, 10, 3.0 p.m.—Society's Library. Adjourned Technical Discussion.

,, 11, 3.0 p.m.—Royal Society of Arts. *Juvenile Lecture.* Mr. R. A. Frazer, "Model Aircraft."

,, 18, 5.30 p.m.—Royal Society of Arts. Major J. D. Rennie, "Flying Boats."

,, 25, 7.0 p.m.—*Students' Section.* Society's Library. F. Radcliffe, "English and German Methods of Estimating Aeroplane Performance."

W. Lockwood Marsh, *Secretary.*

PROCEEDINGS

Second Meeting, 59th Session

An Ordinary General Meeting was held at the Royal United Service Institution on Thursday, October 19th, Prof. Bairstow, Chairman, in the chair.

The Chairman, in introducing the lecturer, Mr. J. D. North, said he was connected with one of the leading aeroplane design firms in the country, a firm which, mainly through the activities of Mr. North and his colleagues, had established a position as leading thinkers among the designers of the country. The case that Mr. North was dealing with in his lecture was rather a difficult one, and no doubt part of the strength of the argument that would be put forward came from Mr. North's belief in the subject. At the same time, Mr. North hoped to put before the meeting reasons for believing that in the immediate future metal construction could be a really serious competitor to the older wood construction or a composite construction of wood and metal. There was a good deal of material in the paper, and he hoped that many people would take part in the discussion and bring out the points of what was an extremely interesting subject.

THE CASE FOR METAL CONSTRUCTION

BY JOHN D. NORTH, FELLOW.

Although I have been requested by the Council of this Society to read a paper on the metal construction of aeroplanes, there are two phases of the subject with which I do not feel prepared to deal. In the first instance, any paper dealing historically with this matter is likely to give rise to controversy which is of neither scientific nor engineering interest, and I propose to leave this aspect severely alone. Secondly, I do not pretend to expound a process of design. The technique of this new branch of the engineering art is still too fluid; while so far as experimental results are concerned it is probable that more have been published than have been digested.

The aeroplane engineer, designer, constructor or user, not unnaturally, is inclined to pin his faith to the system of composite construction, which, brought to a state of high perfection, he has found to serve him well in the past. All the history of engineering relates the gradual displacement of timber by lighter and more durable structures of steel, but such a transition in aeroplanes he feels is difficult, if not impossible, to realise with advantage. Of the three separate metal aeroplane movements in Great Britain, Germany and France, that in this country, at least, received its principal impulse, not from a realisation of the great engineering advantages attending it, but from the pressure of a world shortage of the limited supplies of that class of timber most suitable for light structural purposes.

The cessation of the demand for the mass production of aircraft, coincident with the termination of hostilities, deprived the movement of its principal motive power, but not before it had been realised in some quarters that metal, and particularly steel, construction, could succeed on its own merits even in competition with a normal timber supply.

It may be helpful first to note some general criticisms of metal construction.

The principal argument which has been advanced is that metal construction has not demonstrated its advantages over the usual composite form of

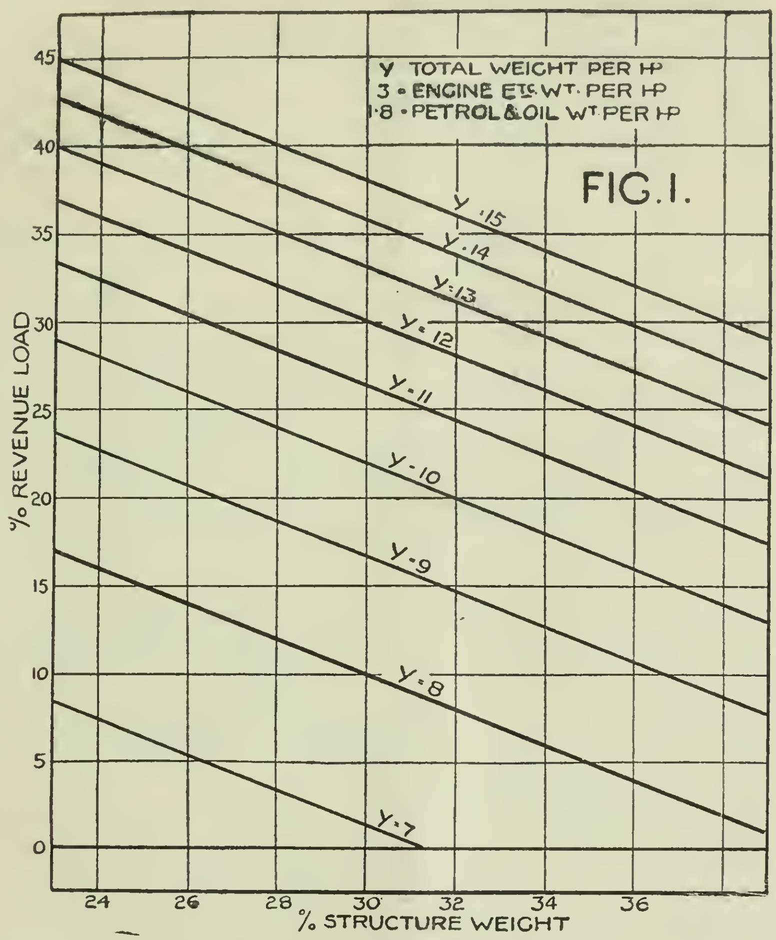
structure. This is not, strictly speaking, a sound criticism, being in the nature of an *argumentum ad ignorantiam,* and though it is quite true that in most cases the users of this argument have not had the necessary experience of metal construction to enable them to appreciate it, such an argument cannot be considered as conclusive. It may be inferred from various remarks that in the opinion of critics metal construction is expensive, perhaps heavier and in temperate climates at least not likely to be more durable than wood. It has further been criticised by analogy on the grounds that small motor boats are invariably, or at least most successfully, constructed of timber, and in some cases it has been further put forward that metal construction must be associated with large aircraft just as it is associated with large ships. The weakness of this argument is immediately apparent when we consider that the first object of a boat is to keep out water, and the adverse experience of constructors of steel motor boats is almost entirely due to the fact that it is difficult to make them water-tight. These conditions are not analogous to those obtaining in aircraft, and it hardly seems necessary to pursue the argument further. With regard to the other criticisms, I hope to indicate in these notes that the following advantages may be obtained by the use of metal construction :—

(1) Retrenchment of weight.
(2) Improvement in the reliability of materials.
(3) A longer life, particularly under conditions of storage.
(4) A better resistance to adverse climatical conditions.
(5) An improvement in bulk manufacturing facilities.
(6) A reduction in some risks and effects of fire.

It is, I think, not always sufficiently realised what a very important effect comparatively small differences in the structure weight of an aeroplane have upon its general characteristics; particularly is this the case when the aeroplane is designed for a high performance or a very long range of flight. There has been some little tendency, and designers of equipment seem to be special offenders in this respect, to imagine that a few pounds here and a few there are negligible in regard to the gross weight of the aeroplane, and that special efforts to economise weight are therefore not necessary. If it is realised that the margin available for military load per unit h.p. is only the remainder after deducting the structure, the engine, installation and fuel weights, it will be appreciated that a saving in weight in an individual part is reflected in an economy on the gross weight, greater by the ratio of the gross weight to the military or revenue load. This is illustrated in Fig. 1, which shows the variation of revenue load —the revenue load in this instance includes pilot, instruments, equipment, etc., so that the influence of structure weight on revenue load is enhanced, particularly in the case of small aeroplanes—or military load with different percentages of structure weight for aeroplanes having various classes of performance. The variations in performance have been indicated by the power loading in lbs. per h.p., since this figure conveys in a simple manner the typical performance, to the aeronautical engineer. It will be noticed that even with a power loading of 15, which is representative of most modern commercial aeroplanes, a reduction of structure weight from 34 per cent. to 26 per cent. increases the revenue load from 34 per cent. to 42 per cent., an increase of nearly 25 per cent. in the utility of the aeroplane. In the case of the high performance aeroplane, the increase will be seen to be vastly greater, and in many cases makes possible a type of aeroplane which will be placed out of court with the heavier structure weight. I have every reason to believe that the structure weight of aircraft can be reduced from an average of 33 per cent. to an average of 25 per cent. to 27 per cent. by the use of metal construction, an advantage which while extremely beneficial for commercial aeroplanes, is absolutely imperative in the case of military machines. I do not suggest that the whole of this gain can be obtained at once, but theoretical considerations and practical experience both indicate that

there is a reasonable probability of arriving at such a figure at no very distant date, provided experiment on broad lines is continued unhampered by the necessity of obtaining immediate results from experimental expenditure.

Let us consider how this retrenchment of weight is to be accomplished. It can be shown from first principles that it is possible to make structural members of metal considerably lighter than of timber, and this has been borne out by practical experience. There are two physical properties of structural materials which are of first order of importance in considering the weight of the structure manufactured from them. Firstly, the ratio of the modulus of elasticity to the specific gravity, and secondly, the manner in which this ratio falls off with increase of stress intensity. This latter property is generally considered by focussing the attention upon the yield point of the material at which



in the ordinary normalised carbon steels, a critical change in the value of E occurs. In the case of timber, which is a vegetable growth itself of complex structure, it is only possible to discuss the change of elasticity with stress in reference to a similar critical case, and indeed since the cause of failure in light compression members of wood and metal is somewhat dissimilar, the difficulty of an exact comparison is enhanced. The following table summarises the elastic moduli of a number of materials, either most commonly used in aircraft construction or by reason of their properties most suitable. In the second column of the table these values have been made specific by dividing them by the specific gravity of the material, and from this it will be seen that in the case of these materials there is very little to choose between them though metal is at some advantage.

If we were to imagine that the structure of an aeroplane were composed entirely of "Euler" struts, in which the modulus of elasticity would be the only

The tables mentioned above and following paragraph will appear in February issue.

property of the material of importance, it would be apparent that that form of construction, which would give us members with the greatest radii of gyration, would give us the lightest structure. A second table* furnishes the critical stress which will be obtained in the outer compression fibres of a strut or a beam, at failure. This stress is again made specific by dividing by the specific gravity of the material, and in this case it is found that there are marked differences between the various types of material showing an advantage for certain of the light alloys over timber, and of certain classes of steels over both. If an aeroplane were constructed entirely of short struts failing in pure compression and of ties failing in pure tension, it is obvious that, supposing the other physical properties of the material to be suitable, the greatest advantage could be obtained by using the material having the highest critical stress, so far as compression failures were concerned, and the highest ultimate stress so far as the tension members were concerned. These maxima are usually to be found in the same material. In making these statements, it is postulated that the design of the member is such as to enable the critical stress to be realised. In other words, for the purpose of our argument, this stress is the critical stress by definition, and the circumstances under which it can be realised will be discussed later. We can now consider the properties of the materials in a more general way.

Fig. 2 shows the ratio of the intensity of load to the free length/radius of gyration of struts manufactured from materials of different critical stress. This figure will be familiar to most as representative of the characteristic curves of Mr. Southwell and Prof. Robertson. The first inspection of this figure will show that in order to realise the advantage of materials having a high critical stress, it is necessary that the ratio of l/k should be kept as low as possible.

It is at this point that the principal structural characteristic of the aeroplane exercises an important influence. The aeroplane is a very large structure for its weight, which means that struts with a low l/k must have very thin walls where they are made of dense material such as steel, or in a lesser degree, aluminium and its alloys.

It is a well-known experimental fact that, where the thickness of the wall is small compared with the radius of curvature and the length of the arc, the intensity of load indicated in Fig. 2 is not realised owing to the crinkling or buckling of the shell at a stress less than that which has been assumed as the critical stress. The lightest strut, therefore, will be that one in which the compromise between low l/k and high critical stress is best effected.

An attack on this problem of local instability by rigid analytical methods is outside the scope of mathematical processes to-day, at least so far as the complex forms demanded for engineering reasons and indicated by experimental results are concerned. At the same time the elastic properties of those materials (*e.g.*, cold worked steels and hardened and tempered alloy steels) which are most suitable, are far removed from the simple proportionality necessarily assumed in elasticity problems. In spite of this we can obtain valuable guidance by reference to two simple cases for which mathematical solutions have been obtained. I do not mean that because the cases are relatively simple compared with those of the actual structural members which we are discussing that the problems are simple ones. Any one can disabuse his mind of this idea by looking up the references.

Case (*a*).—A flat rectangular plate loaded uniformly in its plane and supported at its edges.

Case (*b*).—A uniformly loaded circular tube.

(*a*) A method of obtaining this result, first discovered by Prof. G. H. Brian, is given in "The Mathematical Theory of Elasticity," by Prof. A. E. H. Love, on p. 540 of the third edition. A flat rectangular plate of thickness $2h$, and of

* Will be printed in the February issue.

sides $2a$ and $2b$, is assumed supported freely at its edges against motion perpendicular to its plane, and subject to uniform compressive loads P_1 and P_2 per unit length of edge.

Assuming perfect elasticity, in a state of neutral equilibrium the plate can distort by displacements perpendicular to its plane into a series of double sine waves, provided the following relationship holds:—

$$P_1 (m^2/a^2) + P_2 (n^2/b^2) = 1/4 D\pi^2 (m^2/a^2 + n^2/b^2)^2$$

In the above m and n are integers expressing the number of half waves into which the lengths $2a$ and $2b$ are divided respectively, and are those which give the lowest values for P_1 and P_2. D is the "flexural rigidity" of the plate and is equal to $2Eh^3/3 (1 - \sigma^2)$, where E is Young's modulus and σ is Poisson's ratio for the material.

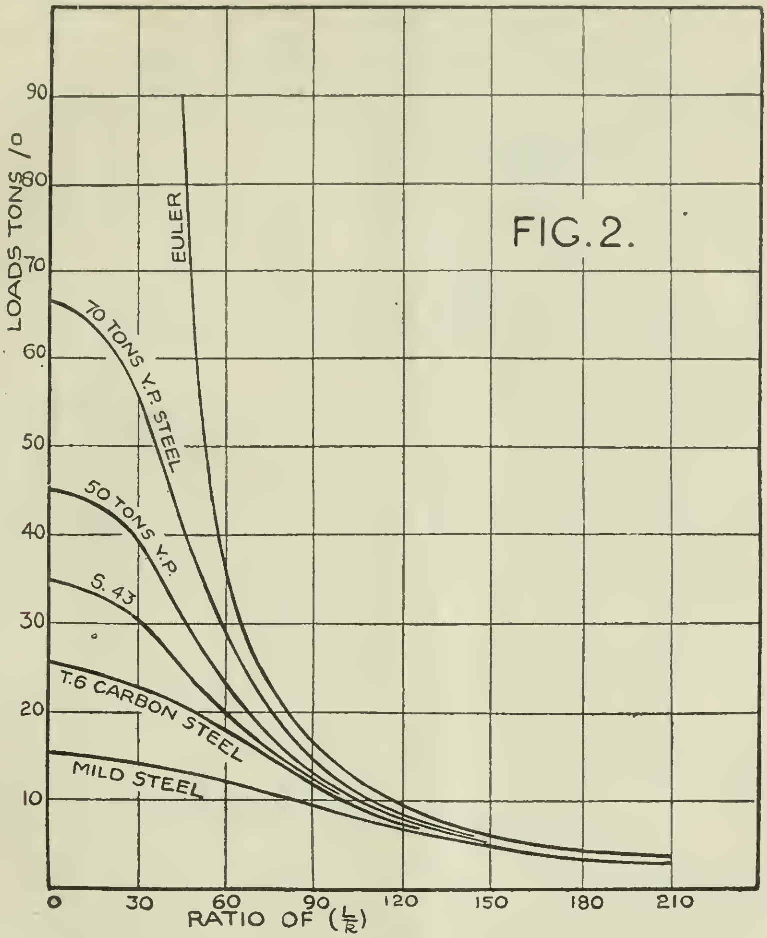


FIG. 2.

For our purpose we consider an infinite flat strip of thickness t, and of breadth B, freely supported along its edges and loaded parallel to its length only by a uniform compressive stress of intensity p.

Let the half wave length in the longitudinal direction be l. Then in the above equation :—

$$\begin{aligned} 2a/m &= l \\ 2b &= B \\ P_1 &= p \,.\, t \\ P_2 &= O \\ 2h &= t \\ D &= Et^3/12 (1 - \sigma^2) \end{aligned}$$

and $p \,.\, t \times 4/l^2 = Et^3\pi^2/48 (1 - \sigma^2) (4/l^2 + 4n^2/B^2)^2$

For a minimum $n = l$ and $l = B$, giving :—$p = \pi^2/3 (1 - \sigma^2) \times Et^2/B^2$.

The constant term in this expression is of limited practical value, but the result is important as indicating the manner in which the variables involved are to be taken into account.

(*b*) This case is examined fully by Mr. R. V. Southwell in his paper on "The General Theory of Elastic Stability" (Phil. Trans. Roy. Soc. Ser. A., Vol. 213). It is sufficient for our purpose to note that the critical stress in a tube of radius R for all forms of failure is proportional to $E\,t/R$.

These results and a consideration of the general theory of thin shells indicate the factors governing the intensity of stress at which instability will occur.

Firstly this stress is proportional to the value of E. In those classes of steels where there is a well-defined yield point shortly after the limit of proportionality is reached the meaning of this is very simple. At the yield point E "collapses" and so does the shell. Provided thickness/(radius of curvature) is above a certain figure its value has practically no influence on the stress at which instability occurs. But with cold worked and hardened and tempered materials the

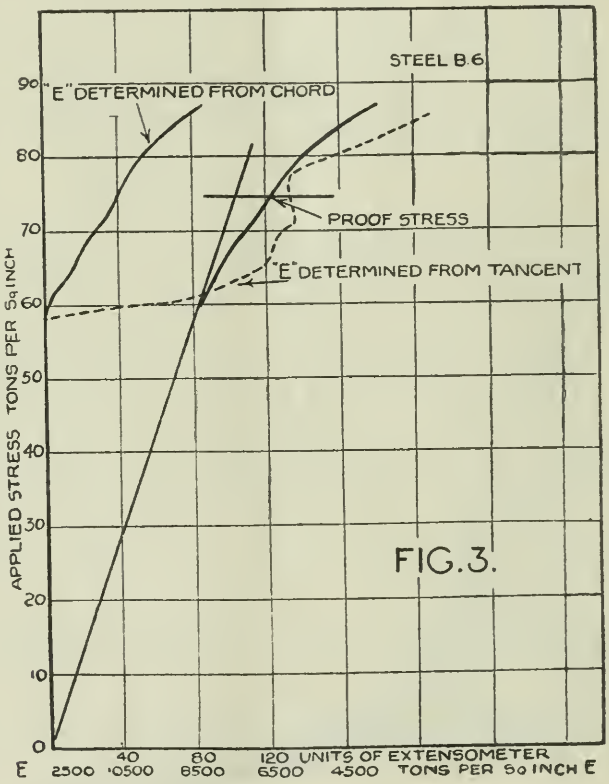


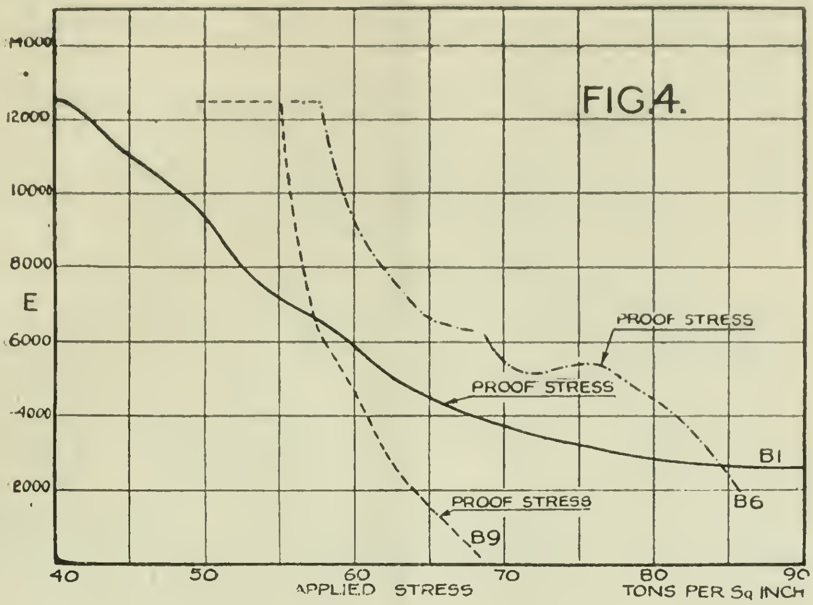
FIG.3.

case is different. The limit of proportionality may be very low, and the rate of change of curvature of the stress strain curve is very slow. An endeavour has been made to define some structurally important stress to meet this difficulty, and the result of these efforts is incorporated in the B.E.S.A. specifications S40 and S43 under the term "proof" stress, which is defined as follows.

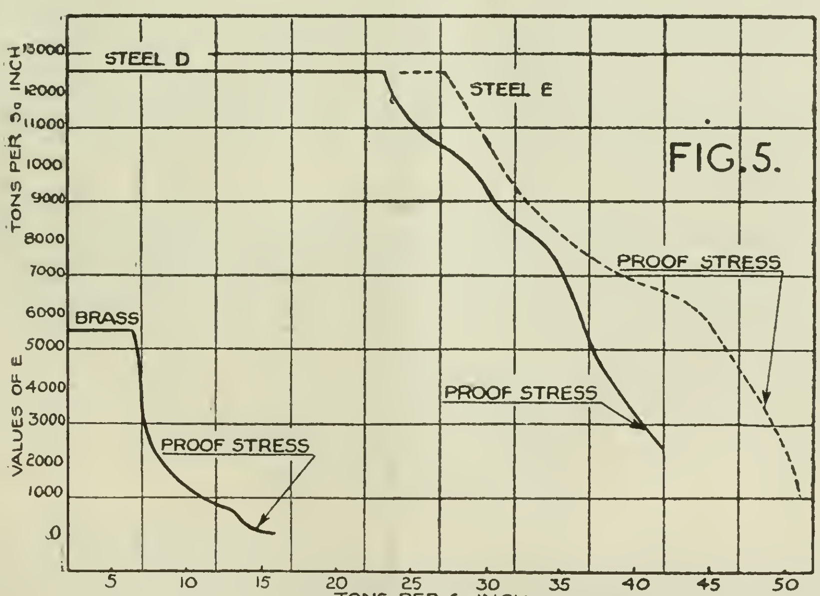
The proof stress is the greatest load per square inch which, when applied to the test piece for 15 seconds and removed, produces a permanent extension of not more than 0.1 per cent. of the gauge length. The proof stress shall be determined upon a test piece having a gauge length of exactly 4in. When a stress of 40 tons per sq. in. is applied to the specimen, and maintained, the total temporary extension of the gauge length must not be greater than 0.017in., and when a stress of 50 tons per sq. in. is applied to the specimen and maintained, the total extension of the gauge length while under load must not be less than 0.020in.

It has been customary for some time to identify this stress with the maximum realisable in a thin walled structure, however small the curvature might be with respect to the thickness of the shell and the "free" length of the arc. From considerations already indicated this is not correct, and indeed "proof" stress

can only have a significant bearing on the stress at which failure by crinkling will occur if the slope of the stress-strain curve of a given material is always constant at the proof stress. This opens up what is, I believe, a new method of examining the structural properties of a material, namely, by a consideration of its elastic properties over the whole of its stress range. The term "elastic"



is used here in the same loose sense that "E" has been used for the slope of the stress-strain curve beyond the limits of proportionality and into the region of plastic deformation. So far as the immediate problem is concerned, although it is of some importance whether the extension is elastic or plastic, there are good grounds for believing that the limit of proportionality is not, in all classes of steel, necessarily the dividing line between these two states. The alternative



to the loose use of these terms is either unnecessary verbosity or a "specially invented terminology," and I feel that laxity is the least of the three evils, and that there is little danger of being misunderstood.

I suggest that the physical properties of a material, *so far as the "critical" stress in a thin-walled member is concerned,* are best represented by the first differential of the stress strain curve with respect to stress. Fig. 3 shows such a curve, together with the stress-strain curve from which it is derived, for a

nickel-chrome steel hardened and tempered at 300 C. There are obvious difficulties in preparing such a curve where the mechanical errors of graphical differentiation are superimposed on the inevitable inaccuracies of strain measurement, but the broad significance of such a method of presentation may be appreciated by a reference to Fig. 4, where the "elasticity-stress" curves are given on a more convenient scale for three nickel-chrome steel specimens (A) fully hardened, (B) hardened and tempered at 300 C., (C) hardened and tempered at 500 C. (B) is taken from Fig. 3.

Fig. 5 gives the same curves for the following material:—

Steel D—Coldworked steel blued at 100 deg. C.
Steel E—Coldworked stainless iron (not blued).
Brass—Muntz metal (cold-drawn).

PROOF STRESS.

Steel	B.9	65.75	tons per sq. in.			E.	1,300	tons	per sq.	in.
,,	B.6	76.5	,,	,,		E.	5,400	,,	,,	,,
,,	B.1	66.25	,,	,,		E.	4,300	,,	,,	,,
,,	E.	48.5	,,	,,		E.	3,600	,,	,,	,,
,,	D.	40.75	,,	,,		E.	2,900	,,	,,	,,
Brass		14.75	,,	,,		E.	100	,,	,,	,,

From this it will be seen that there is no simply defined critical stress, and that by varying the curvature to the thickness and the *free* arc length we can push further along the curve with lower values of E and higher "critical stress."

It is clear that there would be serious difficulties in verifying commercially these elastic properties of a metal, but this is an aspect of the case to which I will return later, when discussing inspection and testing.

As regards the influence of the form of the wall or shell on the critical stress, the significant features are the curvature of the arc, its *free* length and the thickness. Experimental results within my experience indicate that other things being equal (including the value of E), the critical stress is proportional to the thickness as foreshadowed by a general consideration of the theory of thin shells, but in dealing with the question of form, the difficulty lies in the *free* length of the arc. The problem is analogous to that of a redundant structure in which the members are not pin-jointed, and is hopelessly complicated by sectional deformations and the variations of E.

Apart from these difficulties we have those relating the fibre stresses to the external forces on the member. It is convenient to represent the fibre stresses, as deduced from experimental evidence of load and deflection, as $P/A + Pd/Z$ for direct compression loads, and as $P/A + Pd/Z + BM/Z$ where there are external shear loads, where:—

P = end load.
A = area.
BM = bending moment.
Z = section modulus.
d = deflection.

The usefulness of these expressions in ordinary engineering should not blind us to the limitations imposed by the assumptions on which they are based, all of which are violated in the structures we are considering. It is perhaps not too much to say that they have no *real* meaning and that the "stress" calculated can only justifiably be used for estimating the loads which members of similar form will sustain within limits of the ratio of shear to end load permitted by the extent of the experimental evidence. To interpret this stress as representing the *actual conditions* governing the local failure of the shell is dangerous and misleading.

In determining the deflection, and in consequence the stress, in a laterally loaded strut in which the stress induced is beyond the limit of proportionality, the

variation of "E" must be considered differently. In this case the actual stress for an assumed given strain instead of the increase of stress for a hypothetical small increase of strain, is required, and therefore E is determined by the slope of the chord from the zero of the stress strain diagram to the appropriate point of the curve instead of by that of the tangent at the appropriate point. In the calculation a trial and error method must be used with an assumed value of E, and a further uncertainty is introduced by the fact that all points of the member are not equally stressed. Experiment has verified the obvious conclusion that the effective E for calculating the deflection lies somewhere between the "chord" value for the maximum stress and the maximum value, but it is not possible at present to give any more definite information, since the experimental "mean" value of E deduced from measurements of deflection is influenced by distortions not taken account of by the simple beam theory. The "chord" value of E for the steel B.6 is plotted against stress in Fig. 3.

All this may seem rather hopeless, from the scientific point of view perhaps it is, but let us consider the whole process by which a structural member of an aeroplane is designed.

Firstly, the conditions of flight for which the aeroplane is to be designed. These are fixed by an Order in Council and are interpreted by a series of good round numbers delivered after much labour by a committee. Over the manner of arriving at these numbers a decent veil is cast which it would be unwise to disturb. But the Committee seems to know its job in so far as the aircraft are not abnormally heavy nor do they collapse in the air. These numbers are converted to external forces in a particular manner founded on precedent, supported by very doubtful aerodynamic data and also fortified by the same Order in Council. The loads in the members are then estimated by ignoring those members to take account of whose presence would seriously complicate the calculations and by making what are often the wildest assumptions as to the nature of the joints between the members. This brings us to that very data which we were regretting our inability to interpret in an accurate scientific manner. But the whole process works after a fashion because aeroplanes are not designed by science, but by art in spite of some pretence and humbug to the contrary. I do not mean to suggest for one moment that engineering can do without science, on the contrary, it stands on scientific foundations, but there is a big gap between scientific research and the engineering product which has to be bridged by the art of the engineer.

Nevertheless, the technique of the design of thin metal members is progressing rapidly. To arrive analytically by measuring the frictional coefficient of the cloth, the resilience of the balls and cushions, at the precise way to play a complicated stroke at billiards is practically hopeless, but practice and a knowledge of principles deduced from scientific analysis of simple cases will produce surprising results. So it is that, over a wide range, light metal members can be designed with every confidence that they will fulfil their designer's expectations and realise the advantages in weight economy foreshadowed by the properties of the materials.

Inspection and Manufacture

It would be perhaps too much to say of light metal construction that the material is half the battle, but there would be a strong element of truth in such an assertion. I am not suggesting that there is one particular class of steel or light alloy which will be found to be ideal for all purposes. On the contrary, I believe that the types of material used in a single metal aeroplane will increase as time goes on. What we require are materials that can be produced with the necessary degree of uniformity both as regards their elastic properties in the sense these have already been discussed and as regards their plasticity, both absolute as ductility, and in relation to the *true* elastic properties. This last is required to be reasonably uniform in the state in which the strip is to be worked

(sections formed from strip and riveted together are the most characteristic forms of light metal construction). The strip in forming through rolls or dies springs back from the profile of the tool by an amount dependent on its resilience. Reasonable limits are thus required if tools are to produce a satisfactorily uniform article. Control at present is limited to an upper and lower " proof " stress and a bend test. These tests, comparatively simple as they appear, seem somewhat to strain the capacities of commercial testing and inspection without giving as much information as is required.

What is wanted is a stress/strain diagram from which the properties of the materials can be deduced, and it is unfortunate that there is no machine capable of producing them commercially and reliably. For that matter there is not even a standard extensometer which is commercially practical. It is possible that this class of testing apparatus may be improved in the near future. There is also the alternative that research may show a connection between these elastic properties and some more easily measured feature of the material. It is a matter well worth the attention of metallurgists, testing engineers and physicists generally.

A further mechanical uniformity in strip is required in thickness, flatness and straightness. The tolerances laid down in the B.E.S.A. specifications seem satisfactory and more than a slight departure from them often leads to manufacturing troubles.

The problem of manufacture from the strip onwards has two phases which are novel and of special importance. Firstly, forming the strip. Secondly, riveting it.

There is a simple formula for designing tools for forming strip. It is " live and learn." Direct attack on the resilience problem is probably hopeless, but one soon arrives at a few general principles of design, though when marked departure from experience comes along it often brings rude shocks in its train. There are broadly three methods of forming strip—rolling, drawing and pressing—and all have their uses and limitations; but whatever method is used (all are really required), it is necessary to get the best results, to have machines of special design, and my own experience is that it is most satisfactory to design and make them on the spot.

The commonest process in aircraft work is to form the strip in its final state of treatment, but although this has great advantages in assisting production by eliminating heat treatment processes, I believe that a certain amount of soft forming with subsequent heat treatment or even hot forming will be in regular use, by reason of the wider scope of materials and forms opened up. With satisfactory tools and uniform material the production of formed sections makes little demand on skilled labour and can easily be brought to a large production standard.

Riveting is a problem less easily tackled, it is so indefinite. The number of rivets in a steel aeroplane runs into many tens of thousands, the cost of riveting and all it implies is probably the critical factor in the cost of production, and the profit and loss scale will be turned by the drawing office and the planning department. This phase of technique is being slowly and painfully developed, and it is here to a large extent that the high cost of development appears. Metal construction is not inherently expensive. All our experience of the manufacture of details where the technique is in an advanced state confirms this. Money is spent on mistakes and errors of judgment, trial and error the foundation stones of experience.

Corrosion

The question " What about corrosion?" is such a common one that I may perhaps be excused if I introduce a little resumé of present-day opinions on corrosion generally. I hope to show that the susceptibilities of steel aeroplane parts to corrosion are due to faulty shop practice rather than any other cause and to

suggest that with proper precautions there is little to fear from oxidation. Further, the aeroplane which is virtually incorrodible is at last a practical proposition and will make its appearance in the near future. A survey of the large volume of experimental data which has been accumulated on the subject of the corrosion of iron and steel leads us, at an early point in the inquiry, to the conclusion that the chemical reactions involved are by no means simple. Something more than a direct oxidation of iron atoms is involved. This is not in the least surprising. Of almost any apparently simple chemical reaction it can be said that the more closely we look into the mechanism of such change the more complex it grows.

It will not be disputed that the more that is known of the onset and course of development of a disease the greater hope there is, not only of effecting a cure, but of preventing a recrudescence.

Three theories which have been put forward to account for the corrosion of iron and steel will be noticed briefly :—

1. The acid theory.
2. The electro-chemical theory.
3. The recent colloid theory of Dr. Friend.

It may be said at once that no one of them above will suffice to explain all the well-established experimental facts.

1 *The Acid Theory*

This requires the necessary presence of some acid in the water in contact with the metal before iron will rust at all. The carbonic acid present in water in all ordinary cases suffices. It is probably not a fact that iron will not rust in the absence of an acid. Be that as it may, in all cases that interest us the acid, carbonic, is inevitably present, and the presence of any acid, even so weak a one as carbonic, undoubtedly accelerates the corrosion of iron.

2 *The Electro-Chemical Theory*

In its broadest interpretation, includes the acid theory. This theory requires that, for corrosion, the iron shall be in contact with an electrolyte and oxygen. It is impossible and, for our purposes, unnecessary to say whether chemically pure water is an electrolyte; impossible because it is inconceivable that chemical methods have reached the ultimate in refinement, the chemically purest water yet prepared has a definitely measurable electrical conductivity and is an electrolyte in consequence; unnecessary, because the only water that interests us, that which condenses when the temperature falls below the dew point, that which falls as rain, is an undoubted electrolyte, and contains a measurable, though small, concentration of hydrogen and hydroxyl ions. The chemist associates what he calls acid properties with the presence, in large or small concentration, of the hydrogen ion. So water is an acid, a weak acid, perhaps the weakest known.

We have, in materials like iron and steel, which are neither pure nor homogeneous, a very fertile field for the establishment, in contact with an electrolyte, of local voltaic couples, and if the iron, as must sometimes occur, be anodic in such a couple, into solution it will tend to go as the iron ion, with a driving electromotive force behind it, which depends on the nature of the electrolyte in contact with it and on the nature of the cathode. Now this state of affairs, with iron the anode in a voltaic couple, will be found only in places on the surface of any given specimen. Corrosion due to this cause will then only develop at certain well-marked points of attack, accounting thus for the familiar phenomenon of pitting.

But this voltaic action, due to the heterogeneous character of the material and resulting in pitting, is not all. There has to be considered, in addition, the action between the metal itself and the electrolyte with which it is in contact. A

metal in contact with a liquid ionises to some extent, with a definite electromotive force which can be measured tending to ionise it. In the absence of disturbing factors a state of equilibrium is set up when further ionisation ceases and a definite difference of potential between metal and solvent exists. This is the ionic theory of solutions universally accepted by physical chemists as giving the best expression yet to the whole of their knowledge of the mechanism of solution. The disturbing factor that matters to us is the presence of other ions in the liquid, hydrogen ions. The electro-chemical theory of corrosion then attributes the corrosion of the metal to two causes, operating together or singly.

(1) Local voltaic actions.
(2) Straightforward solution of the metal.

The non-corrodible steel will be of such a character that neither of these causes can operate.

Dealing with local voltaic actions first. The presence or absence of the conditions necessary for setting up such actions will depend upon:—

First and foremost, the presence of impurities or, what is equally harmful, local segregations. As an example, it has been established that in the presence of black scale the iron becomes anodic and consequently passes into solution.

(2) *The micro-structure* of the material and anything which has happened to the material in its life history which is known to affect the micro-structure—*e.g.*, heat treatment—is very important.

(3) *Chemical composition.*

(4) *The physical condition of the material,* due to heat treatment or mechanical working. Strained and unstrained portions of a homogeneous material constitute an active voltaic couple.

There is nothing here that proper care in the selection, manufacture and handling of the material cannot successfully deal with.

Straightforward Solution of the Metal.—Whether or not a metal goes into solution in a liquid, it has been said above, depends upon what potential difference exists in the particular case under investigation. All these quantities are susceptible of accurate measurement. Hadfield and Newberry, working on these lines, in acid solutions, have shown that before solution of a given metal can take place the following condition must be satisfied:—

Electrode potential of the metal + the over-voltage must be greater than the hydrogen potential for the solvent.

The electrode potential or solution pressure is a matter of chemical composition; and it should not be impossible to produce an alloy to fulfil this condition, in which corrosion by straightforward solution would be impossible.

Of the two causes of corrosion, according to the electro-chemical theory, the localised formation of voltaic couples, resulting in pitting, appears to be responsible for most of the damage. Recent experiments carried out at the United States Bureau of Standards on the corrosion of high chromium steels by air and water have shown that the rusting of such steels was always local—a surface film of rust was not observed in any instance.

Having regard to both causes of the corrosion of steels, it is clear that a non-corrodible steel is not a mere figment of the imagination, and practical knowledge of the properties of 12-15 per cent. chromium steels and irons indicates the material has already arrived.

3 The Colloidal Theory

There remains to be noticed the recent colloidal theory of Dr. Friend. This theory claims that ferrous hydroxide in a colloidal form appears at an early stage in the course of corrosion; this is oxidised to ferric hydroxide, still in a colloidal

form, as oxygen from the air gains access. And this colloidal ferric hydroxide catalytically accelerates the corrosion of further iron, undergoing alternate reduction by new metal and re-oxidation by fresh oxygen. In other words, the colloidal ferric hydroxide acts as a catalytic carrier of oxygen from air to metal, a type of catalytic action which is perhaps better understood of chemists than any other.

It would appear that this theory would serve to supplement rather than supplant the electro-chemical theory. The iron is brought into an attackable condition by electro-chemical forces, and the course it then follows to its ultimate destination, more or less hydrated ferric oxide, may very well be via the colloidal processes of Dr. Friend.

The above only indicates rather sketchily the manner in which the addition of a high percentage of chromium to ferrous metals has gone far to make an end of the corrosion trouble root and branch, but also furnishes the clue to the precautions to be taken with ordinary steels to fight corrosion. Firstly, see that the material is free from black scale. This is best accomplished by electrolytic cleansing. Before painting see that the steel is dry and chemically clean as possible.

Whatever flux is used for soldering in the shops chlorides will be found on all soldered fittings and many unsoldered ones (resin is not a practical flux) and the parts must be properly cleaned, the wash waters being controlled by analysis. If this is carried out effectively and a suitable paint used there is little fear of corrosion where the protecting coating remains undamaged.

It is not only in the protection of steels against corrosion that advances are being made. Recent experiments have shown that the successful treatment of light alloys is practically an accomplished fact.

Reliability

Apart from this question of corrosion it is necessary to consider in what other respects an all metal aeroplane might be structurally unreliable. I think the following will cover the ground :—

(*a*) Material not up to specifications, due either to faulty inspection or the failure of the inspection to reveal the quality of the bulk under examination.
(*b*) Subsequent mistreatment either by heat treatment or cold work.
(*c*) Undetected errors of workmanship.
(*d*) Damage to members in assembly, delivery or service.
(*e*) Failure of riveting under service conditions.
(*f*) Failure under alternating stress, *i.e.*, " fatigue."

So far as (*a*) is concerned I think that there can hardly be any doubt that metal, particularly wrought metal, is at a great advantage compared with timber. The test samples represent materials of the same composition which have gone through the same processes in the mill and the same heat treatment. The production of carbon steel bars has reached such a high standard that this material *as used* is taken as the standard reliability material by the Load Factor Sub-Committee in the section of its report dealing with reliability. In the thin strip ingot, faults, such as segregation, slag inclusion, etc., are generally noticeable and are in any case revealed in rolling or drawing together with most other material faults should they escape detection before testing. Timber is, however, only representative to the extent indicated by the theory of probability; while its inspection depends on deductions from experience concerning which there is not complete agreement among timber experts.

(*b*) Can be prevented by proper works organisation and is practically confined to those metal parts common to both metal and composite aeroplanes.

(*c*) Are far more difficult to conceal than in wood, particularly as riveting replaces welding. Contrary to popular opinion, defective riveting in properly designed members has very little influence on their strength, within the extreme limits of error experience has shown as likely to occur.

(*d*) Is also not the weak point one might imagine from the thin materials used. The very qualities which enable the members to develop a high stress render them fairly immune from the effects of rough handling, but this point has to be considered in design. That glaring damage is not fatal to the structure has been shown by reloading members broken in test. The most serious danger is fatigue failure from the repeated application of a blow on a particular point; when, for example, a portion of the shell is repeatedly bumped, say, on the tail-board of a lorry, an alternating stress failure in the form of a crack may appear; it is however very obvious and easily repaired, while with similar bad packing a wood member might be irreparably injured.

So far as (*e*) is concerned, I have already mentioned that a certain amount of rivet failure does not seriously deteriorate the structure, and there is with steel at least no evidence of the failure of riveting nor any logical reason why it should fail. In properly designed members the rivets are merely shearing pins expanded to fill the rivet holes while the heads prevent any possibility of falling out. The very remote possibility of an alternating shear stress, sufficient to cause a fatigue failure, can be guarded against in design. The rivets are in fact in much the same category as most of the bolts on an aeroplane with the difference that in the case of the rivets individual failure is of negligible importance.

On a larger scale there is extensive experience of riveting in all branches of engineering and everything in this experience tends to show that riveting is a satisfactory joining process.

(*f*) Considerable strides have been made in the last few years in understanding alternating stress failures, particularly by a better comprehension ot stress distribution. Under the auspices of the A.R.C. several notable papers on this subject have been published, a study of which would remove a great deal of misapprehension. An alternating stress failure may occur after a large number of cycles in which the stress has fluctuated over a range in excess of certain values which appear to be associated with the ultimate tensile rather than the elastic properties of the material. That stress fluctuations will take place in prime movers, vehicles and similar machines is obvious from a study of their mechanism, but in a structure such as an aeroplane they are mostly due to fluctuating loads from the engine direct, varying loads on aerofoil surfaces due to the irregular wake velocity of the slipstream or resonant vibrations originating from either of these. The streamline wire is a notable exception since it has an unstable yawing derivative Nr from which the well-known sound-producing oscillation of these wires is derived.

The only direct fluctuating loads from the motor which I have known to produce failure by fatigue are the rotating couples from fixed radial engines, where these have not been allowed for in design. The maximum amplitude of resonant vibrations is generally small and the mean stress fluctuation is small, too; but the difficulty arises in the complex distribution of stress during small deformations in other than simple members. Reference to the work of Taylor, Griffiths, Coker and others, will show how complex this is and what a marked influence small but sharp changes of section have on it. Generally speaking, the avoidance of rapid changes of section (*e.g.*, sharp corners) in conjunction with material of adequate ductility, is sufficient insurance against high localised stress; metal construction falls in easily with this requirement. We can hardly go further into the subject, but it is safe to say that fatigue failures, if they occur, must be traced to the drawing office, not to the use of steel as a structural material.

Metal Covering

The desirability or otherwise of metallic covering seems to me to be largely a matter of policy. I see in the JOURNAL that Mr. O. Short puts the weight of metal covering at 0.2lbs. per sq. ft. presumably of covering. If this is the case metallic covering means an addition of 5 to 6 per cent. to the gross weight of the aeroplane—an addition which I should imagine could only be tolerated in very exceptional cases. I do not believe that it is imperative that aircraft should be *all metal*, quite a few parts—not structural—are probably better of wood except under special climatical conditions. Fabric covering, especially when it can be re-doped, really gives long service and re-covering encourages general overhaul.

Conclusion

I believe that anyone who examines the possibilities of metal construction on the lines I have indicated will appreciate the possibilities of reducing the structure weight of aeroplanes by the use of metal construction, and where they are able to supplement their deductions by experience they will be convinced of the great future in front of it. The fact that metal aeroplanes will stand adverse climatical conditions, long storage and, I believe, the wear and tear of every-day use in temperate climates better than aeroplanes largely built of timber and joined with glue is further argument in its favour.

The development of technique will undoubtedly tend more and more to eliminate the necessity for skilled labour in course of construction, and this opinion is fortified by experience. It is probable also that load factors will be able to be lowered without increasing the risk of structural failure or alternatively increase of reliability may be obtained.

I have said nothing so far on the subject of fire risks, though some improvement may be attained in this direction. Those fires in the air, resulting in a large quantity of petrol burning in a comparatively confined space, would hardly be affected if the aeroplane were made of asbestos; but since fires after a crash, probably now more to be feared, are often caused by splinters of wood coming into contact with hot exhaust pipes, this latter class of fire risk should certainly be minimised.

On looking through these notes I cannot feel that I have presented the case for metal construction as completely and convincingly as it deserves to be presented. I have been so closely occupied during the last few years with the thousand and one details of this new development that I have found it somewhat difficult to put this subject before the Society in a general way. If I had once started entering into details I should hardly know where to stop, and details as they arise will be more interesting to those who are taking up this work for themselves.

In conclusion, I should like to thank those gentlemen who have assisted me in the preparation of this paper, particularly Dr. Leslie Aitchison, who has rendered invaluable help on metallurgical matters and who kindly supplied the curves illustrating the variations of " E "; also Mr. A. E. Odgers, M.A., for the resumé on corrosion, and several other members of my experimental staff. I have also to thank Messrs. Boulton and Paul, Ltd., for permitting me, for the purpose of this paper, to make use of information and experience gained in their aeronautical department.

Discussion

The Chairman said that before throwing this extremely interesting lecture open to discussion there were two letters which he would read. One was from Mr. O. Short and the other from Major Green.

October 18th, 1922.

Chairman of Mr. North's Lecture on October 19th,
Royal United Service Institution,
Whitehall, London.

Dear Sir,

I much regret that I am prevented from attending the lecture to-night. I am in agreement with practically the whole of Mr. North's admirable paper, and I feel with the same confidence that he does that aeroplanes of metal construction will eventually show substantial advantages as against the present construction.

I should like to emphasise the need of building up the technique of design and construction, as I feel sure that once the general principles are known it is only by the continual development and improvement of detail design that metal construction can achieve the results that we anticipate. This development work calls for great patience, considerable engineering knowledge and adequate resources. It is chiefly by our difficulties that we learn, and unless we have the courage of our convictions that we are doing work which will ultimately advance aviation, we are unlikely to devote the time and energy necessary to bring the work to success.

Yours faithfully,
Fred. M. Green.

October 18th, 1922.

To the Chairman, Royal Aeronautical Society,
7, Albemarle Street, W.1.

Dear Sir,

It is a matter of regret to me that I shall not be able to attend Mr. North's lecture on "The Case for Metal Construction" as I am deeply interested in the subject.

I have, however, read the advance copy of the lecture, and I should like to congratulate the lecturer on the very able and scientific manner in which he has dealt with the subject.

I should like to point out that there is a typographical error in the advance copy of the lecture, in which I am quoted as having said that the increased weight of metal covering as against fabric covering is 2 lbs. per sq. ft.; this should be two-tenths of a pound per sq. ft. of lifting surface, or plane area, as the term is used.

With regard to this increased weight, however, it should be borne in mind that it occurs only when fabric is simply replaced by metal in the usual wing structure formed of spars, ribs and struts, and in which these members bear the main portion of the flying stresses. In future developments it will, perhaps, be possible to utilise the metal plane covering to bear the whole of the flying stresses, and from experiments carried out by my firm we are led to the conclusion that such planes, whilst giving all the advantages of the rigid metal surface, will at the same time be lighter than a steel girder structure covered with fabric.

It is evident that we are at present only touching the fringe of very important developments in the metal construction of aircraft, and I submit that it behoves those who seek the most rapid advance in aircraft design to keep an open mind on the subject and not to allow purely theoretical considerations to confine this development in one direction or another.

In this connection I particularly refer to the choice of metals without taking sides for or against the use of steel or aluminium alloys. I have used these materials in conjunction and when they appear to fit in best with our requirements, and the numerous experiments which my firm has carried out during the last three years strengthen my conviction that such a combination may be used with the utmost safety and reliability.

Quite recently we carried out a destruction test on the tail plane of a machine which was constructed with tubular steel spars and duralumin ribs, and has been in use over two and a half years. It withstood a loading of 40 lbs. per sq. ft. for 65 hours, and was then loaded to 60 lbs. per sq. ft., at which figure it failed. On opening up the plane for inspection purposes we discovered that the methods we had adopted for the prevention of corrosion and rusting were so effective that not the slightest signs of either of these disabilities were visible. This tail plane may be seen by anyone who is sufficiently interested in the subject to visit our works at Rochester.

Yours sincerely,

OSWALD SHORT.

Air Ministry, Kingsway, W.C.2,
October 24th, 1922.

Dear Sir,

I was unfortunately prevented by work at the Air Ministry from attending Mr. North's very interesting lecture on metal construction.

I have always been a great supporter of metal construction in aircraft, and I was delighted to see the conclusion at which Mr. North had arrived. If his claims are valid, as I believe them to be, the introduction of metal construction will be the biggest step in progress towards making air transport self-supporting which has been achieved since the end of the war. I only hope that designers, in spite of their present financial difficulties, will push on towards effecting designs in metal with all possible energy.

Yours very truly,

W. M. BRANCKER.

The Secretary, Royal Aeronautical Society,
7, Albemarle Street, W.1.

I am pleased to hear from General Brancker that he is in favour of making every effort to utilise the commercial advantages to be obtained by the incorporation of steel with its lighter structure into aeroplane design.

Unless, however, some direct assistance is available I am afraid it will be some time before these conditions will be available to users of commercial aircraft.

Continuing, the CHAIRMAN said he noticed among the audience several people who were specially entitled to speak on this subject, and he would ask them to keep their remarks within a definite time limit. He did not wish to fix the limit too rigidly at the moment, but he hoped those who spoke would bear the point in mind.

Major WYLIE said he considered Mr. North had presented a very big subject in a very comprehensive and able manner. He agreed with almost everything that had been said, and with some things he agreed enthusiastically. For instance,

Mr. North had said that the best way to master the art of forming strip to make spars was to live and learn, and he agreed with that enthusiastically. Mr. North indicated that the behaviour of the strip in springing back from the dies was governed by the elasticity of the strip, *i.e.*, by the elastic limit or the proof stress. He himself, however, had found that the ultimate strength was the deciding factor, because in bending the strip was bent to such a curvature that it was stressed very much beyond the elastic limit and even beyond the yield. He would like Mr. North's opinion on that point. The next point that he agreed enthusiastically with was that in order to design a steel spar properly, skill and judgment must be used. He thought Mr. North's illustration of the prowess of the billiards player was very illuminating, particularly in one respect, viz., that the success of the billiards player could be judged immediately by whether the ball went into the pocket or not. He would like to know how Mr. North judged the success of the designer; whether it was by applying some formula by which the strength of the design could be calculated or whether it was only by a method of testing. If it was by a formula, he would like more information as to what formula was applied and how it was applied. There was one point upon which he did not agree with Mr. North. It was stated in the lecture that it has been customary for some time to identify the proof stress with the maximum stress realisable in thin-walled structures, however small the curvature might be with respect to the thickness of the shell. He did not know whose custom this had been, but it certainly had not been his (the speaker's) custom. About seven years ago he had used it to test a great many tubes in compression, and he found that tubes with a ratio of radius to thickness of 40 to 1 would fail completely and crush up when the stress was about equal to the yield stress, whereas tubes that were much thicker than this and with the same diameter would stand up to a stress which was in excess of the ultimate strength of the material, and when very thin tubes were tested the stress developed might not reach even the yield point strength of the material. When he came to the very much thinner members that were used for spars, he found that, if these were of high tensile steel, he never got up to the yield point of the material. He got a number of spars which were very similar and which appeared to be of fairly stable section, as they would take a set before they failed, *i.e.*, they would not fail when the material was stressed to the elastic limit, but neither did they stand up to a stress equal to the ultimate strength of the material. He found that they usually failed when the permanent set in the strip at the point of failure was equal to about 0.1 per cent., and the proof stress had been defined, as stated by Mr. North, in order that it might be a criterion for the strength of the spar in which it was used. He believed that if members were designed so that the elastic stability of them was sufficient, it would be found that the member would fail at approximately the proof stress as determined in a tensile test. It had been his practice to design spars so that if he expected them to stand a stress of 70 tons per square inch, with material having a proof stress of 70 tons, the critical stress, *i.e.*, the compressive stress at which the member would fail if the material were elastic, as high as the critical stress, and if the section were ideally perfect, would be in the vicinity of 120 tons to the square inch. The other day he had to test an interesting section in the form of a large corrugated tube which was made of steel specified to have a proof stress of 40 tons per sq. in. He calculated that if the section were absolutely true, and if the material were elastic up to 77 tons per square inch, the critical elastic stress would be about 77 tons per square inch. The member failed at a stress of 46 tons, and the proof stress, as determined by Dr. Aitchison, was 45 tons. Such close correspondence between failing stress and proof stress was got fairly regularly, and therefore he felt that the proof stress, as at present defined, was not so unsatisfactory as Mr. North made it appear. At the same time, he agreed that the tangent to the stress-strain curve was an important factor.

Mr. W. D. Douglas said that he also agreed with Mr. North in most of

what he said. With regard to riveted joints, he had had the opportunity of testing a fair number of samples of metal construction which utilised rivets for jointing purposes, and there had been very few cases in which the riveting had been defective. He was referring to small defects in riveting and not those which were very easily observable. That confirmed Mr. North's statement that commercially defective riveting was not frightfully important. Of course, it all depended on the nature of the member. In certain cases, every rivet counted, but in the majority of cases one rivet would not affect the strength of the structure seriously. Also, in subjecting these structures to vibration tests, he had had extremely few cases of failure of the rivets in the steel members. Where they had failed they had generally been extremely defective in workmanship. He was referring here to such defects as should not pass any reasonably efficient inspector. Much more information was needed in connection with the design of riveted joints for aircraft. The case of bridge construction was not entirely parallel because some alternating stresses were obtained in metal aircraft that were not usually present in bridge construction, and it might be that the design of riveted joints to withstand alternating stresses should be founded on quite a different basis to that which controlled the design of joints for static loads. For instance, in joints designed for static loads it was generally assumed that if the drilling and riveting had not been done with micrometrical accuracy, the ultimate "give" in the material would distribute the load among the various rivets and make each of them do its fair share. Also, according to the work of Prof. Batho, the friction in the joint played a very important part in determining its strength. In vibration, or under alternating stress, it was not safe to count on the ductility of the material distributing the load. There was a parallel case in the effect of alternating stress on other ductile materials. Ductile materials in general, under alternating stress, behaved apparently as if they were extremely hard and brittle and without any ductility. For instance, a piece of new plasticene, which was almost infinitely ductile—its reduction of area was 100 per cent. in tension—if subjected to alternating stress, behaved like a piece of sulphur. If there was a small scratch on the surface there developed a crack extending from the scratch and there was a granular appearance in the fracture. It was not safe, therefore, in riveted joints to depend upon ductility for making up small errors in manufacture. Also, it was uncertain what the frictional effect of the riveted joint would be in alternating stress, although so far there had been practically no trouble with steel construction. He could not, however, at the present moment say the same about aluminium alloys. He had had a case of failure of riveted joints in an aluminium alloy spar. It was a single sample of joint, and it had failed twice in vibration, and a similar joint which had been in flight had shown traces of movement when it had been subsequently opened up. The fact that a joint had been moving in the air did not inspire confidence, but he did not wish to put this forward as detracting from the use of aluminium alloys, but merely as an instance, showing that more information as to the design of such joints was required.

Dr. A. P. Thurston thought that everyone present would join in congratulating their old friend Major Wylie on the complete recovery from a most serious illness.

He congratulated Mr. North most heartily on an interesting paper, and like Major Wylie, he agreed enthusiastically with Mr. North in everything he had said, but regretted being unable to discuss the paper as fully as it undoubtedly deserved owing to the fact that he had not read the paper before the lecture.

One of the principal objects of present experiments in metal construction was to obtain data to enable the strength of metal aeroplanes to be calculated with accuracy. Another object was to obtain the shapes of the sections which gave the best results in practice. It was not easy to place the various sections

in order of merit without careful analysis, so much in fact depended upon the means of comparing experimental results that he would be interested to know what formula or formulæ Mr. North used for comparing the efficiency of the various sections which had been evolved.

Metal construction, in his opinion, was still in the experimental stage, and solid progress would apparently be most easily achieved by a system of experiments devoted exclusively to obtaining the data or the coefficients required in the various calculations. He would be interested to know whether Mr. North had obtained curves

(1) For failing loads on flanges of various thicknesses and curvatures, and
(2) Of the relation between the curvature of the roll and the finished curvature of the strip.

He felt that the problem of metal construction had suffered from a tendency to make all-metal construction a fetish, that is to say, there appeared to be a tendency to construct every part of the machine of metal whether metal is the most suitable material or not, in order to be able to say that the construction was all-metal. He thought that in the present state of the science a composite construction was the most suitable one, at least for small machines.

In this connection it should be remembered that the great driving force behind metal construction during the last few months of the war was the shortage of suitable seasoned wood. If by any unhappy calamity we should again be involved in war, the same problem would confront us, but magnified a hundred times. It appeared, therefore, a sound and essential policy for the authorities to concentrate their experiments on the vital members of aircraft, *i.e.*, on those parts which used a lot of wood and carried main loads. When these experiments were finished, then, the problem of the minor parts could be solved. This was the way he had endeavoured to tackle the problem during the last six months of the war. The experiments were chiefly concerned with

(1) Obtaining suitable sections for the vital members.
(2) Determining the best materials for particular purposes, and
(3) The scientific properties of the materials and the sections to enable calculations to be accurately carried out.

In this latter connection it was necessary, for instance, to discover and define the scientific properties of steel which caused local collapse due to local bending. Was this local collapse dependent on a limit of proportionality, the yield point, or upon some other property? After much experiment it appeared that neither the yield point nor the limit of proportionality could be taken as giving a just measure of the strength of thin steel sections. A limit of proportionality, *i.e.*, an elastic limit, of 15 tons per square inch and a yield point of 90 tons per square inch were not uncommon in the early experiments. It was not until the discovery that the ultimate strength of a section did not strictly depend upon either of these properties, but upon some intermediate property, that suitable materials were evolved for metal construction.

It was found from experiments that the strength of a section depended upon an arbitrary figure which has been defined as the "proof stress" or "effective strength" of the material. The "effective strength" is defined as stress which causes a permanent set of one-thousandth of the length of the material. Doubtless a more accurate definition will be evolved as experiments proceed, but this arbitrary definition appears to fit in best with the experiments already conducted.

The stability of a section does not depend upon its scale provided that all the dimensions are maintained in the same proportion. It follows, therefore, that the calculation of the strength of various spars or the like is a simple matter if ample experimental data is available. He would, therefore, advocate an organised series of experiments in connection particularly with the vital members,

as this would provide the nation with a fund of information which would be immediately available should necessity arise. In any case, this information would be of more immediate practical value than a series of curves giving only some theoretical aspect of the subject.

In conclusion, he wished to convey his personal thanks to Mr. North for once again bringing this important subject forward, and congratulated him on his interesting and valuable paper.

Mr. NORTH, replying to the discussion, said he would first deal with one or two points made by Dr. Aitchison in which he did not agree with the paper as regards corrosion. In the section dealing with the electro-chemical theory, the paper said "(4) The physical condition of the material, due to heat-treatment or mechanical working. Strained or unstrained portions of a homogeneous material constitute an active voltaic couple. There is nothing here that proper care in the selection, manufacture and handling of the material cannot successfully deal with." He was afraid that the last part of this statement was too far-fetched. It was quite true that although it was a theoretical possibility, it was not something which we could actually hope to obtain. The other point mentioned by Dr. Aitchison was that it was suggested in the paper that the portion dealing with corrosion led up to the reason of the non-corrosive properties of the 14 per cent. chromium steel, but he was afraid there was a big gap and it was rather hopeless to try and bridge it within the scope of the paper. Therefore it was not quite true to say that it indicated the reason for the immunity of that class of steel from corrosion.

With regard to Mr. Short's letter, the figure of 2 lbs. instead of 0.2 lbs. was a typographical error, but it did not affect the argument because he did not use the 2 lbs. in measuring the percentage. Mr. Short now said that he meant the plane area and not the covering surface, so that the increase of weight would be something of the order of 3 per cent. Mr. Short had said that he hoped to be able to produce aluminium covered wings lighter than steel, and he himself could only reply by saying that he hoped by that time he would have produced wings which were lighter than aluminium ones.

He would very much like to emphasise what Major Green said in his letter as to people having the confidence of their convictions. Mr. Handley Page had once accused him (Mr. North) of being a fanatic. Indeed, Mr. Handley Page had said that he was a fanatic on all-metal construction and that he (Mr. Handley Page) was a fanatic on the subject of slotted wings. He did not mind being called a fanatic, however, because he did not think it was possible to get over the disheartening early stages of any work of this sort unless they made up their minds that they had confidence in their convictions, and it was his convictions which he had tried to outline in the paper. His convictions were the reason why he had confidence, and he believed that there were many other people who had had experience of all-metal construction who had the same confidence that he had.

Major Wylie had referred to the springing back from the rolls. The paper had suggested that this was due to the resilience of the material, and he had thought it was a very good method of abbreviation to use the term resilience; but Major Wylie had found him out in this. What actually governed the spring back, he presumed, was the potential strain energy of the strip as it came out of the roll. Some parts had been plastically deformed, and others elastically stretched, and the amount of spring back would depend on the strain energy of the strip as formed. He certainly did not mean to suggest that it was associated in any way with the elastic limit or the yield point or the ultimate. He simply suggested that it was, in an extremely complicated way, associated with the shape of the stress-strain diagram, not the limit of proportionality, a statement to which he thought Major Wylie would not take exception. He believed that Major Wylie associated it with the ultimate strength of the

material, and he also believed that Major Wylie's method of dealing with these calculations gave reasonably accurate results. At the same time, he did not know on what solid grounds the method of calculation rests. Both Major Wylie and Dr. Thurston asked what was the formula for testing the designer. It was a very simple test; it was the structure weight of the aeroplane which the designer built. The whole object of aircraft structural design was to make a reliable structure of a certain strength and get it as light as possible, and he could not think of any test or formula which would enable the designer to be put to the test. In fact, he thought it was purely a matter of engineering judgment. If these things could be assessed in terms of formulæ, all that would be necessary would be to have a book and the office boy could take the book out and design. But the individual circumstances had to be taken into account in every case, not only in regard to aeroplanes but in regard to all engineering design.

Major Wylie said his question was what formula was used to test the actual strength of the section when it was designed.

Mr. North said the strength of the section was defined in terms of the compression loads and the external shear loads.

Major Wylie said that perhaps he might explain himself better. The Paper gave two formulæ—one for a perfect tube and the other for a flat rectangular plate loaded uniformly in its plane and supported at its edges. What he wanted to know was whether Mr. North had a formula which could be applied to the structure he designed.

Mr. North: You are referring to a formula for knowing the elastic instability.

Major Wylie: Yes.

Mr. North said he did not suggest that he could put forward a formula based on any rational grounds at all. He only gave the formulæ in the paper because they showed what the variables are, but it was a comparatively easy matter to satisfy oneself whether a spar would stand up to a certain load or not. It really came back to the billiards table point all over again. Major Wylie, in the matter of metal construction, was a very expert performer. There was one fact in this connection, however, to which he (Mr. North) had not made any reference in the paper because he felt he had received the information in confidence, but he believed that Major Wylie had developed a method of calculating the stress at which instability would occur and one which, he believed, gave very satisfactory results. Though he was familiar with Major Wylie's formulæ, he was not aware, however, of the grounds on which they were founded, *i.e.*, how Major Wylie bridged the gap between the simple cases and the complicated cases which occurred in practice. Major Wylie had also spoken of the yield point of high tensile steel. The yield point of high tensile steel was a very vague term and it did not satisfy the proper definition of yield point. He did not mean in the paper, when he said it was customary to assume that the proof stress was the maximum realisable, that it was the maximum realisable in any material. He meant it was identified with what one could expect to gèt by the best design, using that particular material. He thought the discrepancy between Major Wylie and himself was due to the fact that he had not made himself clear.

Major Wylie said he thought that probably was so.

Mr. North, continuing his reply, said he was pleased that Mr. Douglas had said that his experience with riveting agreed with what was said in the paper, although he very properly pointed out that there were circumstances in which the riveted joints of metal aeroplanes are stressed with types of alternating stresses which were somewhat outside general engineering experience, but as Mr. Douglas had said, as far as was known at present, there was no special trouble arising

with the types of connections now used. To what extent it would be possible to simplify these joints as we gained more experience remained to be seen. Dr. Thurston had suggested that he had not put enough of his wares in the window. He did not know whether that was a reasonable thing to expect even if he had all the information that Dr. Thurston wanted, and he had not. The fact was that these things did not belong to himself, but to the firm for whom he worked and he did not know that it was a reasonable thing to ask them to allow him to publish a wholesale accumulation of data. If they were put in at all, they would have to be of a very voluminous nature, and he did not think that that was the proper way of looking at the subject at all. It was better to look at it from broad principles. If they were satisfied with the broad principles, then they could work on similar lines and obtain their own experimental results. He did not think a large amount of experimental data, although helpful in some respects, was necessary in presenting what he had been careful to call this paper, the case for all-metal construction, not the first guide to metal construction or how to design metal aeroplanes, or anything of that sort. The paper was merely the presentation of the reasons for what he believed to be the bright future for this class of construction. In actual practice, the difficulties which were arising in making metal aeroplanes were not the difficulties of designing satisfactory spars. That did not represent the trouble of designing. The designing of spars took about one-tenth of the work of one member of the staff. It was one of the least troublesome parts of the whole job. The difficulties were an accumulation of small details due to the young, weak and inexperienced drawing office technique, and not the difficulty of making a section which would stand up to a given load. The difficulty was to produce on paper a shape which could be reproduced in the shops and incorporated in a reasonable structure. It was in this direction that the greatest difficulties arose. If they were to attempt to standardise large numbers of types of sections, they would only be tending to restrict development of technique by leading people to believe there was some particular virtue in some particular shape. In the end, the design turned on the general *motif* or theme of the whole structural system. It was a thing which was very difficult to define. It was not a scientific matter at all, but underlying any engineering structure or mechanism there was a kind of theme or *motif* which was carried through the whole design, and by maintaining that theme it was possible for the engineer to produce what was called an engineering job.

The President said the lecture had been an extremely interesting one, and had taken as its main theme really the idea of live and learn. Living and learning was not restricted to engineering or to any branch of engineering, and he did not believe that anybody under-valued it when they came to think of it at all. When all was said and done, the whole object of education was living and learning in order that the younger members of the community might learn what had been the experience of their predecessors and help them to learn it quickly. It was the efforts of designers such as Mr. North, who were gaining further experience, which some day—perhaps a decade hence—would be handed down to the students of our colleges as matter which was then standardised although at the present moment, as Mr. North had indicated, it was a perpetual difficulty. This always meant expense and it needed a great deal of courage. He therefore asked those present to accord Mr. North a hearty vote of thanks for his paper.

The vote of thanks was carried with acclamation, and the meeting closed.

ESTIMATION OF WEIGHT OF AEROPLANE PARTS

BY EDWARD P. WARNER, ASSOCIATE FELLOW.

The aeroplane designer, in making up a weight schedule and calculating the centre of gravity of a projected machine, usually relies entirely upon his past experience and upon those data concerning the products of other designers which he may have been able to accumulate in his files, and it is seldom that the weight estimates and balance computations are checked, after the completion of the detail design and before the construction of the aeroplane, by carefully calculating, from its volume and the specific gravity of the material of which it is made, the weight of each separate element entering into the construction. Unfortunately, it is not often that any thorough weight analysis is carried through even after the construction is completed, or that each part going into the structure is weighed during the progress of the assembly, and the weight data available for the use of the designer are therefore much less complete than they might and should be.

Although the general practice is to depend on past experience and knowledge of the weights of similar parts in aeroplanes of types similar to that under consideration, there are times when it is very desirable to have a general formula for the weight of a part, either in order to furnish a basis for estimating with increased accuracy the weights on a new machine of a type or size differing somewhat from any in the designer's experience, or more frequently to make possible an estimate of the effect on the weight of some change in the form or dimensions of an aeroplane.

It is obvious that no such formulæ can apply accurately to all cases. The methods of construction differ so widely that it is perfectly conceivable, for example, that two aeroplanes may be exactly similar in external form and appearance and have the same total weight when loaded, yet have wing structure weights differing from each other by as much as 40 per cent. Although it is impossible for any of them to be of universal application, general formulæ may nevertheless be very useful if they are correctly derived and if they are applied with due regard for their limitations.

The fundamentals which govern the derivation of rational weight formulæ were first discussed by Normand with reference to ships, and have recently been applied by Commander J. C. Hunsaker to the detail weights of rigid airships. The same principles are equally applicable to the case of the aeroplane, but have seldom been used in that connection. There are many formulæ for the weight of aeroplane parts, but most of them are purely empirical and there is wide disagreement among them, not only as to the numerical coefficients to be used, but also as to the form assumed by the expression for the weight of any particular element and as to the exponents to be attached to the several factors.

In forming these weight equations it is necessary to treat each class of member separately, later combining the formulæ thus obtained to secure a single expression for the weight of a whole assembly such as the wing truss. In the course of this article the major assemblies which make up the aeroplane structure will be discussed in turn, and each assembly will be dissected and an analysis made of the laws which govern the weights of the components.

Wing Weights.

The most important single element of the wings in respect of weight is the spars. The spars are stressed in bending and also in compression or tension, and the variation of weight must therefore be examined from three separate standpoints.

The bending moment in a beam with any given type of loading is proportional to the product of the total load carried and the length of the beam. Since the length of a wing spar is proportional to the span of the aeroplane, the bending moment is proportional to the product of the total weight and the span. In order that the stress in the extreme fibre may be a constant, the section modulus must vary in the same ratio as the bending moment. If it be assumed that the flange thickness is a fixed fraction of the total depth of the spar, that the width can be changed independently of the depth, and that the ratio of web thickness to width of flange remains constant, the section modulus will be proportional to the width of the flange and to the square of the depth. The depth of the spar varies as the thickness of the wing, and this, if the same aerofoil section is used throughout, is proportional to the chord. The results so far obtained may then be summarised in symbolic form: —

$$M \text{ varies as } WS \qquad WS \text{ varies as } bd^2 \text{ varies as } bC^2$$

where M = max. bending moment. b = width of spar flange.
W = total weight of aeroplane. d = spar depth.
S = span.
C = chord.

The weight of the spar is proportional to the product of its maximum length, breadth and depth, or to the product of span, chord and flange width. Since, by transforming the proportionality just written,

$$bC \text{ varies as } WS/C$$

it follows that

$$W_s \text{ varies as } WS^2/C$$

where w_s is the spar weight. This may be written in the alternative form

$$W_s \text{ varies as } WSR$$

letting R represent the aspect ratio. The wing spar weight, in so far as bending stresses are concerned, is therefore proportional to aspect ratio, to total weight and to span. If the aspect ratio is varied while the area is kept constant, and neglecting the small variation in total weight, the weight varies as the three-halves power of the aspect ratio, for the span is proportional to the square root of the aspect ratio for a given area.

If the spar fails by buckling, the moment of inertia of the cross section must bear, to insure safety, a fixed proportion to the product of the square of the length and the end load. The end load is proportional to the product of the total weight and the ratio of span to gap, while the length of a bay varies as the span and the moment of inertia of the section as the product of the width of spar flange and the cube of the spar depth, or the cube of the chord. Written in symbols, assuming the gap-chord ratio constant,

$$bd^3 \text{ varies as } bC^3 \text{ varies as } WS^2\,(S/C)$$

The spar weight, as before, is proportional to the product of span, chord and width of flange, or

$$W_s \text{ varies as } bCS \text{ varies as } WS^4/C^3 \text{ varies as } WSR^3.$$

This expression is of the same general type as the other, but the aspect ratio now enters in in the third power. It will be noted in both cases that the ratio of spar weight to total weight varies as a linear dimension, and it will be found that this is true of weight formulæ in general.

If the bays are supposed to be short enough so that the spar would ultimately fail under the compressive load by straight crushing instead of by buckling, this being the common assumption in calculating stresses, the cross sectional area must be proportional to the end load and the expression for spar weight is identical with that derived in the case of bending. In general, therefore, the formula

$$W_s = KWSR$$

may be taken as representing the weight of wing spars subjected to combined stress.

An exactly similar analysis may be made for rib weights. Ribs are subject to bending stress only, and the section modulus must therefore vary directly as the product of the load, which is proportional to the product of the load per sq. ft.

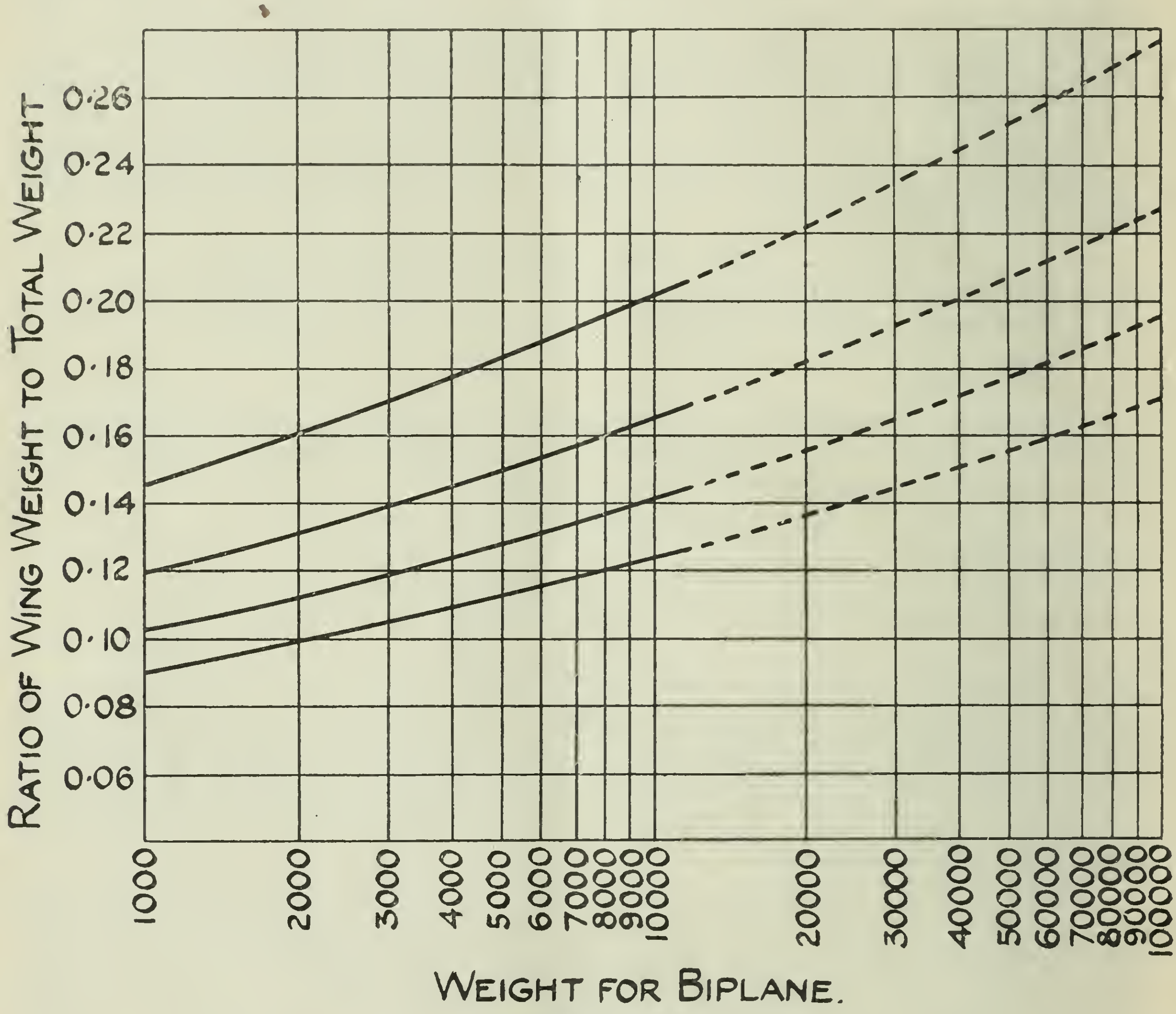


Fig. 1.

on the wing, the chord and the rib spacing, and the length of rib. The last-mentioned quantity may be represented by s. In symbolic form,

$$bd^2 \text{ varies as } bC^2 \text{ varies as } (W/SC)\,CsC \text{ varies as } WC\,(s/S)$$

and

$$w_r \text{ varies as } bC^2 \text{ varies as } WC\,(s/S)$$

$$n \text{ varies as } S/s$$

$$nw_r \text{ varies as } WC$$

where w_r is the weight of a single rib and n the number of ribs; b and d in the above equations, of course, relate to the dimensions of the rib, not of the spar. The total rib weight should therefore be independent of spacing, were the material utilised with equal efficacy in all ribs and, unlike the spar weight, it goes down as the aspect ratio goes up.

The weights of the interplane struts and of the drag struts within the wing follow the same laws. In both cases the geometrical form of the cross section

may be considered as fixed, both dimensions varying together, whereas in the spar one dimension is fixed and the other must vary alone. Since these struts are long columns failing by buckling, and since the compression in any strut is proportional to the shear in the wing truss at that point or to the total weight carried,

$$d^4/l^2 \text{ varies as } W$$

$$w_t \text{ varies as } d^2l \text{ varies as } \sqrt{d^4l^2} \text{ varies as } l^2\sqrt{W} \text{ varies as } C^2\sqrt{W}$$

l being the length of the strut and w_t the strut weight. In this case the ratio of

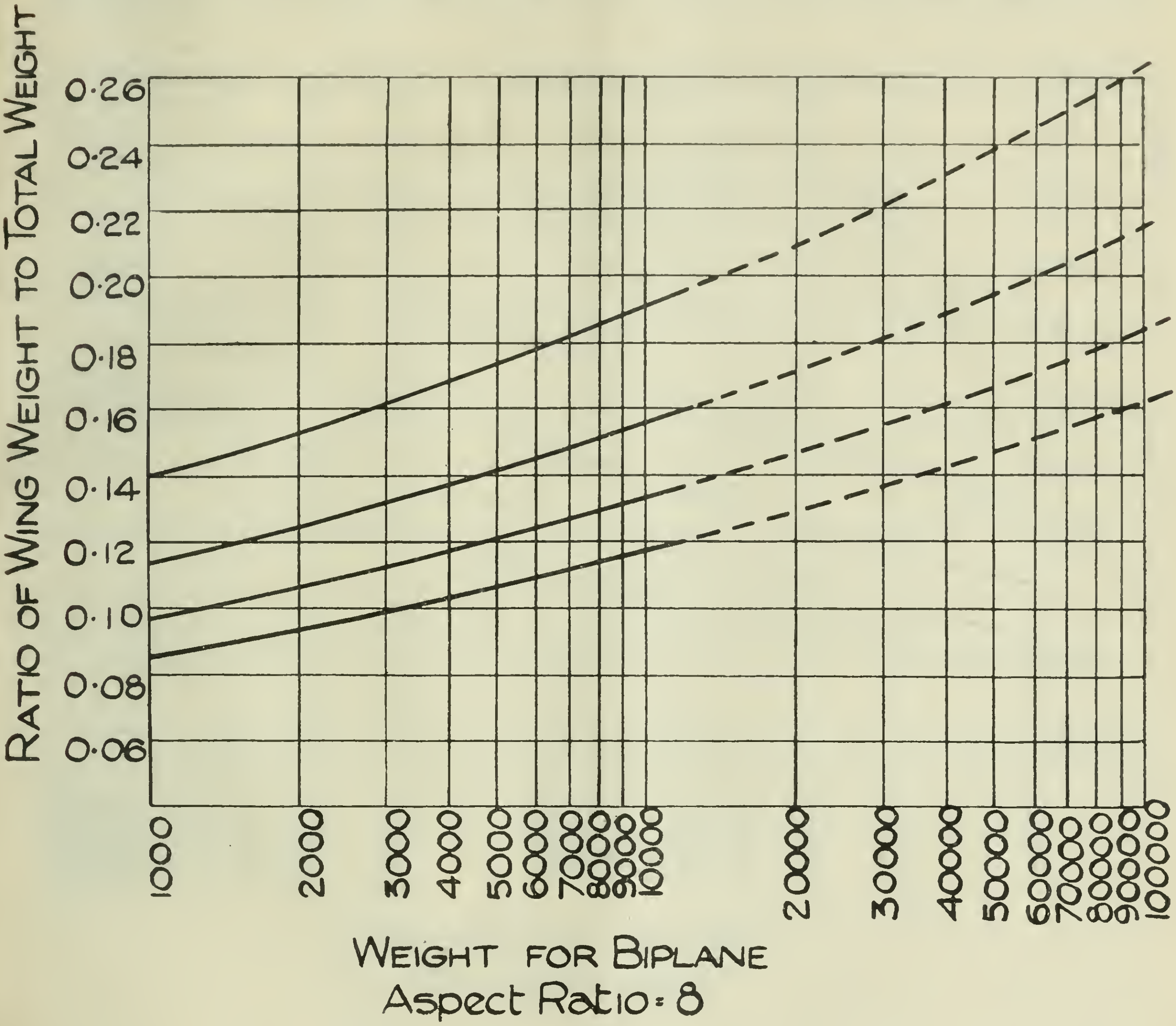


Fig. 2.

part weight to total weight cannot be taken so directly as in the others already examined, but it is clear that if the unit loading and the aspect ratio remain constant the chord must vary as the square root of the total weight. Once more, therefore, the part weight is proportional to the product of the total weight carried by a linear dimension of the structure.

The strength, and consequently the weight, of the fabric used on the wings is theoretically dependent on the rib spacing and the unit loading. In actual practice, however, the same sort of fabric is used on virtually all aeroplanes, and the weight of the fabric is therefore simply proportional to the area.

The only remaining elements of importance in the wing structure are the wires. The wires always being in tension, their sectional area must be directly

proportional to the load, and the weight varies as the product of the load and the length. If a flying wire makes the angle θ with the wing, or an internal drag wire the angle θ with the spar, the load and the wire is proportional to $W \operatorname{cosec} \theta$ and its length to $S \sec \theta$, always assuming a constant number of bays and a constant ratio of gap to chord. The weight of the wires being equal to a constant times the product of these two quantities,

$$w_w \text{ varies as } WS \sec \theta \operatorname{cosec} \theta \text{ varies as } 2WS/\sin 2\theta$$

θ, for both lift and drag trusses, almost always lies between 30° and 45° and $\sin 2\theta$ therefore lies between .87 and 1. As an approximation never involving an

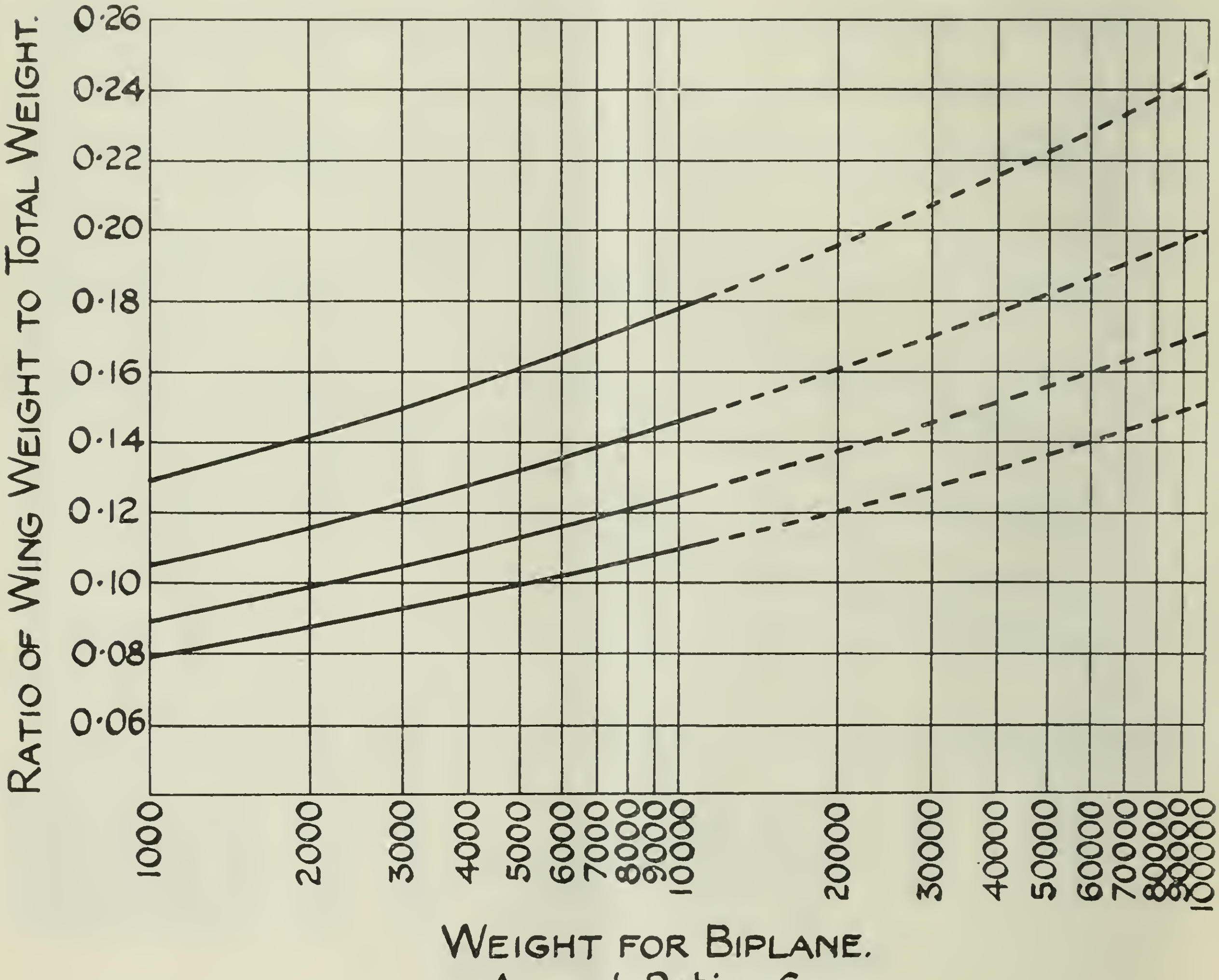


Fig. 3.

error of over 8 per cent. from the average $\sin 2\theta$ may be considered as constant and omitted from the equation of proportionality, which then becomes

$$w_w \text{ varies as } WS.$$

Since it is very seldom expedient, in making up a weight schedule, to treat separately each element such as the spars and ribs, it is desirable that all of the formulæ so far secured be combined in a single expression. This can be done, for a group of designs reasonably similar in type, by finding the average weight of each element in aeroplanes already built and then assigning to each an importance in the combined formulæ proportional to the share which it contributes to the total weight.

If W_2 and W_1 be the total weight of two assemblies of roughly similar type,

and w_{a_1} and w_{a_2}, w_{b_1} and w_{b_2}, etc., be the weights of the elements which make up the assemblies,

$$W_2/W_1 = (w_{a_2}/w_{a_1}) \times (w_{a_1}/W_1) + (w_{b_2}/w_{b_1}) \times (w_{b_1}/W_1) + (w_{c_2}/w_{c_1}) \times (w_{c_1}/W_1) + \ldots$$

If, further, $w_a = KA^xB^yC^z$, as in the elementary weight formulæ just derived, A, B and C being dimensions of the structure,

$$w_{a_2}/w_{a_1} = (A_2/A_1)^x \times (B_2/B_1)^y \times (C_2/C_1)^z.$$

If the ratios A_2/A_1, B_2/B_1, etc., are not far distant from 1, or in other words, if no very large change is made in the dimensions, the above expression is approximately equal to:—

$$w_{a_2}/w_{a_1} \approxeq [1 + X(A_2/A_1 - 1)] \times [1 + Y(B_2/B_1 - 1)] \times [1 + Z(C_2/C_1 - 1)]$$

and this, again neglecting the products of two or more fractions, is approximately:—

$$w_{a_2}/w_{a_1} \approxeq 1 + X(A_2/A_1 - 1) + Y(B_2/B_1 - 1) + Z(C_2/C_1 - 1)$$

$$\text{if } w_{b_2}/w_{b_1} = (A_2/A_1)^P \times (B_2/B_1)^Q \times (C_2/C_1)^R$$

$$\text{and } w_{c_2}/w_{c_1} = (A_2/A_1)^T \times (B_2/B_1)^U \times (C_2/C_1)^V$$

$$\begin{aligned} W_2/W_1 = 1 &+ [(Xw_{a_1} + Pw_{b_1} + Tw_{c_1} + \ldots)/(w_{a_1} + w_{b_1} + w_{c_1} + \ldots)](A_2/A_1 - 1) \\ &+ [(Yw_{a_1} + Qw_{b_1} + Uw_{c_1} + \ldots)/(w_{a_1} + w_{b_1} + w_{c_1} + \ldots)](B_2/B_1 - 1) \\ &+ [(Zw_{a_1} + Rw_{b_1} + Vw_{c_1} + \ldots)/(w_{a_1} + w_{b_1} + w_{c_1} + \ldots)](C_2/C_1 - 1) \end{aligned}$$

and this is approximately equal to:—

$$\begin{aligned} W_2/W_1 = &[1 + \{(Xw_{a_1} + Pw_{b_1} + Tw_{c_1} + \ldots)/W_1\}\{A_2/A_1 - 1\}] \\ &[1 + \{(Yw_{a_1} + Qw_{b_1} + Uw_{c_1} + \ldots)/W_1\}\{B_2/B_1 - 1\}] \\ &[1 + \{(Zw_{a_1} + Rw_{b_1} + Vw_{c_1} + \ldots)/W_1\}\{C_2/C_1 - 1\}] \end{aligned}$$

and, again approximating,

$$\begin{aligned} W_2/W_1 = &\{(A_2/A_1)^{Xw_{a_1}/W_1 + Pw_{b_1}/W_1 + Tw_{c_1}/W_1 + \ldots}\} \\ &\times \{(B_2/B_1)^{Yw_{a_1}/W_1 + Qw_{b_1}/W_1 + Uw_{c_1}/W_1 + \ldots}\} \\ &\times \{(C_2/C_1)^{Zw_{a_1}/W_1 + Rw_{b_1}/W_1 + Vw_{c_1}/W_1 + \ldots}\} \end{aligned}$$

The detail weights for a number of aeroplanes of the two-seater tractor biplane type have been examined and it has been found that the average distribution of weight among the several elements of the wing structure, neglecting minor elements which, combined, make up about 18 per cent. of the total weight, is as shown by the table below.

Element.	Per cent. of weight.
Spars	34
Ribs	16
Struts	24
Wires	10
Fabric	16

Then taking, according to the method just derived, the weighted average of the exponents with which the several factors enter into the elementary formulæ, it is found that the wing weight should vary in accordance with the formula

$$W_w = K_w W^{.72} S^{.94} C^{.46}$$

or alternatively,

$$W_w = K_w W^{.72} S^{1.40} R^{-.46} = K_w W^{.72} R^{.94} C^{1.4}$$

If the wing loading be introduced as a variable in order to eliminate all quantities directly dependent on size except the weight the equation becomes:—

$$W_w = K_w W^{.72} A^{.7} R^{.24} = K_w W^{1.42} (W/A)^{-.7} R^{.24}$$

Since the span and chord are taken for a single supporting surface the weight used in these formulæ should be that taken by a single set of wings, or

one-half the weight of a biplane, for example, when W_w becomes the weight of the single set of wings. The area denoted by A in the formula is also that for a single supporting surface. The total wing weight for a monoplane would therefore be 32 per cent. larger than for a biplane of the same total weight, total area and aspect ratio, and having the same value of K_w. The wing weight for a triplane, similarly, would be 16 per cent. less than for a similar biplane.

The value of K_w has been determined for a number of aeroplanes of service type, ranging in weight from 1,100 to over 10,000lbs., and the values found are plotted against weight in Fig. 1. It will be observed that most of the points are grouped close to a smooth curve, and where there is a large deviation from the

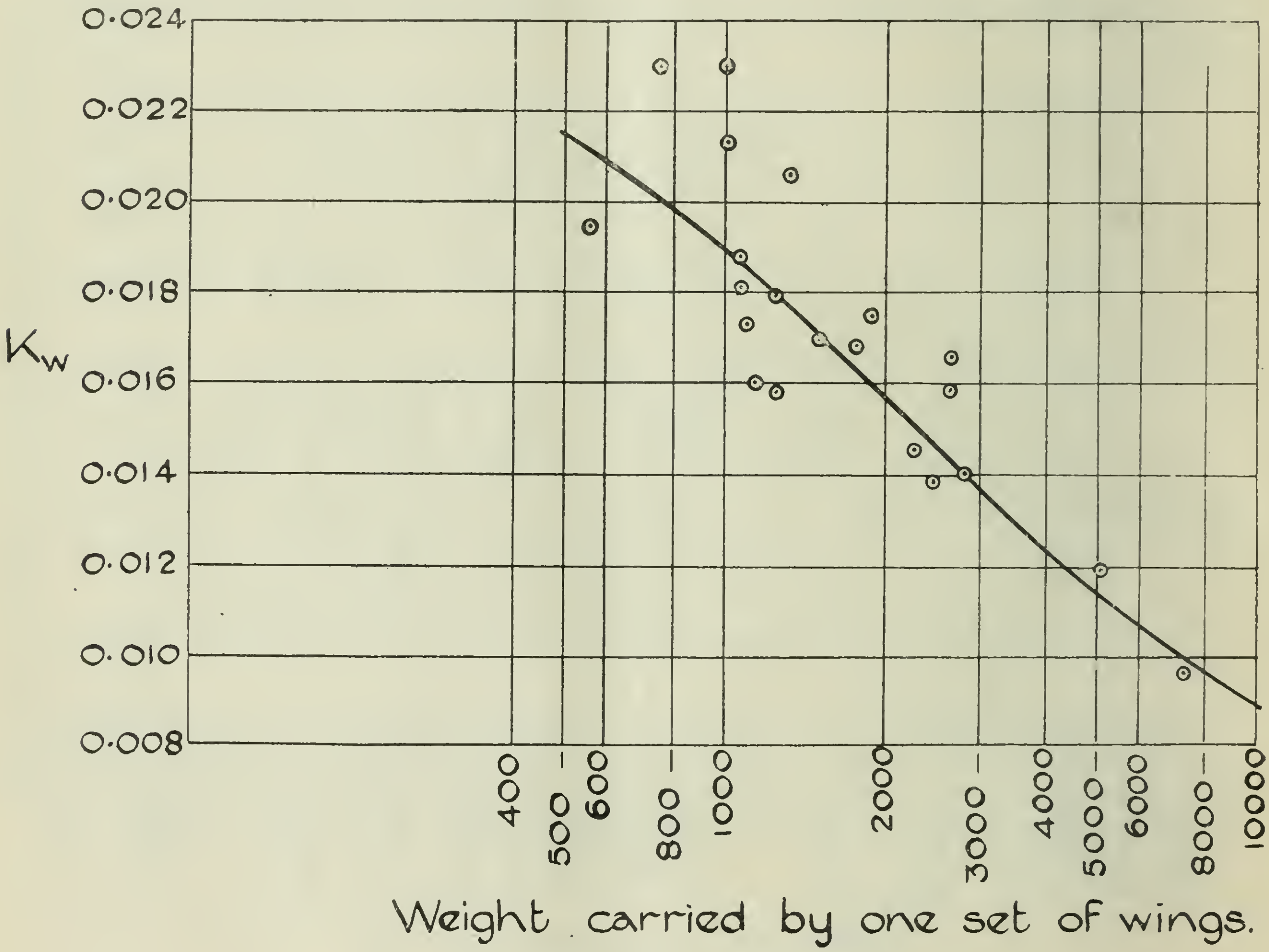


FIG. 4.

curve it can generally be accounted for by peculiarities of the design. The wing weight constant for cantilever trusses, for example, lies well above the mean curve in the cases for which data was available, while that for machines in which the interplane bracing is unusually profuse and the unsupported spar spans short lies below the curve. Aside from eccentricities in amount and arrangement of external bracing, the location of a point on the chart with respect to the mean curve gives a fair measure of the efficiency of construction of the wing cell.

The curve given for K_w can be represented with reasonable accuracy (within 5 per cent. at all points in the range shown) by the equation

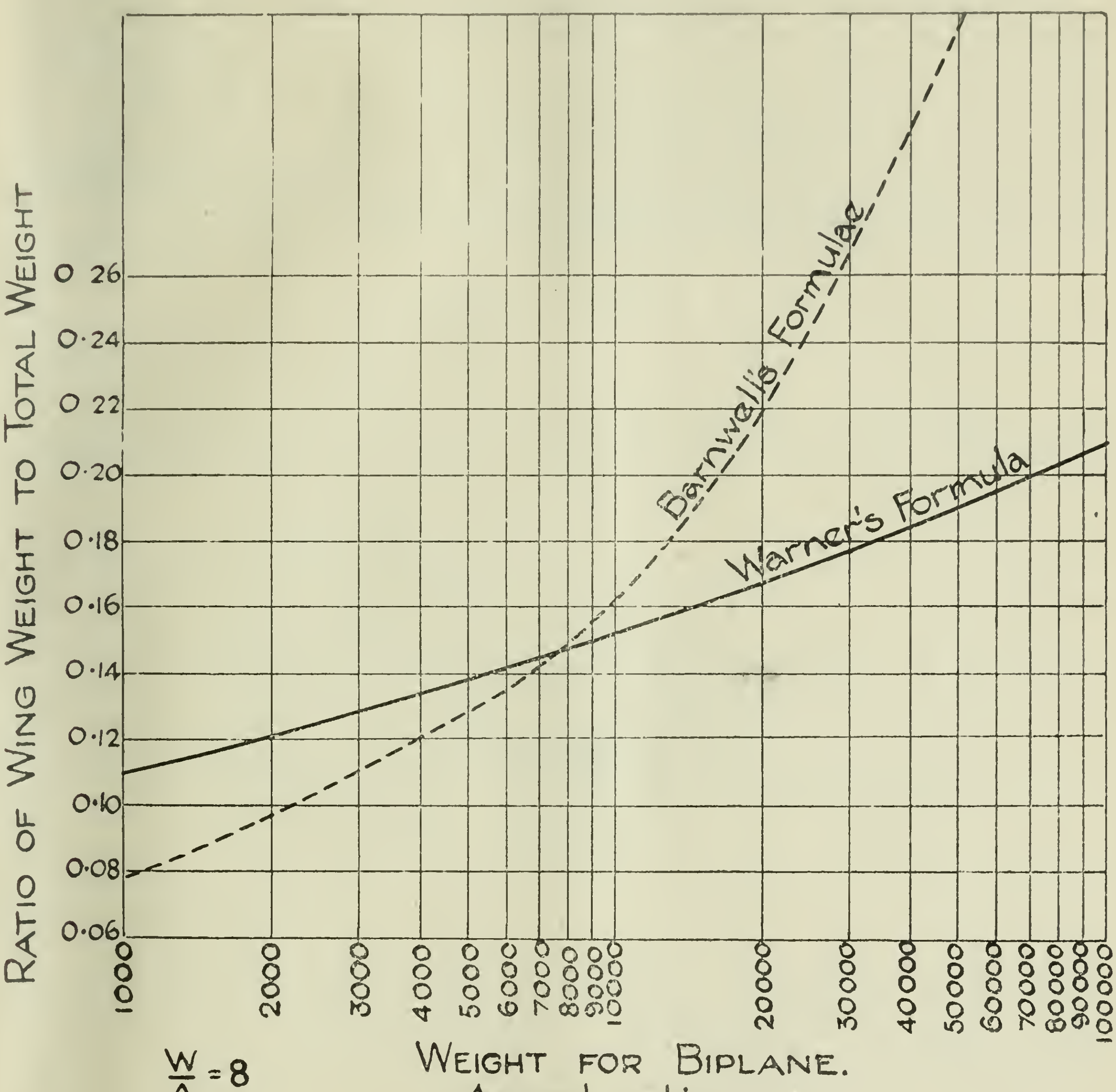
$$K_w = 1/8.11\ W^{.276}$$

If this equation be combined with the original forms they become:—

$$W_w = .123\ W^{.44} S^{.94} C^{.46}$$

$$W_w = .123\ W^{1.14} (W/A)^{-.7} R^{.24}$$

The second equation shows that, on the basis of present practice, the improvement in constructional efficiency with increasing size is insufficient to overcome the handicap inherent in the large structure, and the ratio of wing weight to total weight therefore tends to increase as the size of the structure grows. In Figs. 2 to 4 the ratio of wing weight to total weight is plotted against total weight for a number of wing loadings and for three different aspect ratios. These three sets of curves are drawn for biplanes. The corresponding figures for monoplanes are



Comparison of Wing Weight Formulae.

Fig. 5.

equal to those for biplanes of twice the weight, while the actual weight of a triplane must be multiplied by two thirds before seeking its wing weight ratio on the chart. The dotted portions of the curves are beyond the limits of present practice and the extrapolation is admittedly uncertain. The weight of the wings in very large aeroplanes is of course dependent largely on the degree to which the weight of the power plants and useful load is distributed along the wing span, and it appears from the curve of present weights that such distribution will be

essential in very large machines if the wing weight is not to become excessive, and that the scheme, so much favoured by the Germans during 1918, of concentrating all the power units in a central fuselage will be carried out only under a heavy penalty as sizes increase still further.

It is of some interest to compare the results given by these formulæ with those found by other investigators. The most complete wing weight formulæ previously published are those of Barnwell.* Barnwell gives a set of seven formulæ derived especially for an aspect ratio of 7.2 and for a loading of 8lbs. per sq. ft., but presented as substantially valid for other aspect ratios. Fig. 5, in which the wing weight ratios are plotted in accordance with Barnwell's formulæ and with the formula here derived (taking W/A as 8 and the aspect ratio as 7.2), shows that Barnwell's formulæ make less allowance for improving efficiency of construction with increasing size than does the writer's, and that they are palpably inapplicable to weights over 20,000lbs. For medium-sized machines the two sets of figures show reasonably good agreement and change with changing size in the same general manner, although at very different rates.

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* Aeroplane Design, by Captain F. S Barnwell. "Flight," January 6th, 1921, p 13.

The Journal of the Royal Aeronautical Society

(Founded 1897 in succession to the Annual Reports)

Edited for the Council by J. LAURENCE PRITCHARD, Fellow

No. 146 FEBRUARY 1923 Vol. XXVII

NOTICES

Election of Members

The following members were elected at a meeting of Council held on January 16th, 1923:—

Students.—H. L. Calder, G. S. Mills.

Air Conference Representatives

The Council, in response to an invitation from the Air Ministry, have nominated the following members to represent the Society at the Third Air Conference to be held at the Guildhall on February 6th and 7th, 1923:—

Mr. Griffith Brewer, F.R.Ae.S.
Mr. Robert Bruce, A.F.R.Ae.S.
Mr. W. D. Douglas, A.F.R.Ae.S.
Captain W. Scott Farren, A.F.R.Ae.S.
Mr. H. Glauert, M.A., A.F.R.Ae.S.
Lieutenant-Colonel S. Heckstall-Smith, F.R.Ae.S.
Captain G. T. R. Hill, M.C., A.F.R.Ae.S.
Professor C. F. Jenkin, C.B.E., F.R.Ae.S.
Mr. J. L. Pritchard, F.R.Ae.S.
Major-General Sir R. M. Ruck, K.B.E., C.B., C.M.G.
Mr. H. L. Stevens, A.F.R.Ae.S.
Dr. H. C. Watts, F.R.Ae.S.

Aeronautical Classics

It has been suggested that a further series of monographs on the work of various pioneers should be embarked upon, to provide when completed a second volume to "The Aeronautical Classics." The Publications Committee consider that this is a very desirable work if the Society's finances permit of it being undertaken. As a preliminary it was decided to invite members to indicate their willingness to subscribe to such a series in order that some idea may be gained of the probable call upon the Society's funds to meet the balance of the cost. Among those with whose work it is proposed to deal (covering the period 1883-1903) are Laurence Hargrave, Otto Lilienthal, S. P. Langley, Octave Chanute, and Sir Hiram Maxim, and it is anticipated that the price would be about 3s. 6d. to 5s. 0d. per volume, according to the number of subscriptions received. Members willing to undertake to subscribe to the series are asked to communicate their intention to the Secretary.

It may be recalled that "The Aeronautical Classics" were published during 1910, and comprised volumes on Sir George Cayley, F. H. Wenham, Thomas Walker, Francesco Lana, Percy Pilcher and F. J. Stringfellow, and G. A. Borelli.

Students' Section

Arrangements have been made to hold a smoking concert for students and their friends at the Engineers' Club, Coventry Street, London, W.1., at 6.45 for 7 p.m., on Friday, February 16th. Tickets, price 1/6 each, may be obtained from the Secretary. Early application is advisable.

R38 Memorial Research Fund

The following further donations towards this fund have been received :—

	£	s.	d.
Officers of the Staff, British Aviation Mission, Japan	20	0	0
Mrs. Little	8	18	9
Officers R.A.F., Peshawar	7	15	2
Lieutenant-Commander White, U.S.N....	5	0	0
E. G. Walker	2	5	0
Further Interest on Deposit Account	9	18	4
Previously announced	1211	2	8
Total to date	£1264	19	11

The following entries for the R.38 Memorial Prize have been received. Entrants are reminded that their papers, together with the signed declaration required by the regulations, must reach the Secretary not later than March 31st :—

Commander F. L. M. Boothby.
Captain Elbridge Colby.
A. P. Cole.
Wing Commander J. N. Fletcher.
Commander J. C. Hunsaker, C. P. Burgess, S. Truscott (*Joint Authors*).
Robert Jones.
Norman Meadowcroft.
Professor Dr. T. v. Karman.
Major G. H. Scott, Lieutenant-Colonel V. C. Richmond (*Joint Authors*).
R. H. Upson.
Ing. Rodolfo Verduzio.
Dr. Karl Wegener, Dr. Karl Schneider (*Joint Authors*).

Arrangements for the Month

Feb. 1, 5.30 p.m.	Royal Society of Arts. Mr. G. S. Baker, O.B.E., late R.C.N.C., M.I.N.A., "Ten Years' Testing of Model Seaplanes."
,, 8, 7.0 p.m.	*Students' Section*.—Society's Library. Mr. W. L. Le Page, "Experimental Methods in Aerodynamics." Mr. A. Fage, F.R.Ae.S., in the chair.
,, 15, 5.30 p.m.	Royal Society of Arts. Wing-Commander T. R. Cave-Browne-Cave, "The Practical Aspects of the Seaplane."
,, 20, 5.0 p.m.	Council Meeting.
,, 22, 7.0 p.m.	*Students' Section*.—Society's Library. Mr. T. A. Kirkup, "The High Lift Wing." Major O. T. Gnosspelius, A.F.R.Ae.S., in the chair.
Mar. 1, 5.30 p.m.	Royal Society of Arts. Major F. M. Green, F.R.Ae.S., "Helicopters."

W. Lockwood Marsh, *Secretary*.

PROCEEDINGS

Third Meeting, 59th Session

An ordinary general meeting was held at the Royal United Service Institution, on Thursday, November 2nd, Prof. Bairstow, Chairman, in the chair.

The Chairman, in opening the meeting, said that his opening remarks would be confined strictly to the chairman's preliminary business, viz., that of introducing Major Low to those present, or rather to those who did not know him. Those who did know Major Low and had an advance copy of his paper would realise that the paper was typical of the man, very interesting and very contentious. (Laughter.) That was one reason why he himself wished to keep his present remarks short, because he hoped to get a later opportunity of speaking on the subject. Major Low had been connected with aeronautics through the whole of what might be called the modern period of development, that was from a little before 1910 to the present day. In the early period of that time he was designing aircraft and applying his mathematical faculties to the general problem. In particular, he read a paper before the Society in 1913 in which he gave mathematical extensions of the Drzewiecki theory of airscrews. That paper was in the Journal and gave rise to valuable discussion at the time. In the present paper Major Low proposed to give a summary of the theories as presented by other people with his own critical faculties exerted in the selection of material and its presentation. There was one other point he (the Chairman) wished to mention now, although it really had not anything to do with Major Low's paper, except for the fact that he mentioned Prandtl's theory. Major Low had quite properly kept his remarks very general. There were very few mathematical symbols throughout his exposition and consequently the matter was easier to follow. The Council of the Society had felt for some time that opportunities might quite well be afforded to those members who were really expert in a subject, to meet and discuss points of great difficulty. On these occasions it would not be necessary that the member speaking should make himself easily intelligible and the assumption could be made that the rest of the audience had a great deal more knowledge than they were likely to possess at the moment. There might be occasions on which different speakers could attack a given problem from so many points of view that they never came into contact, and although contact might produce fireworks and a good deal of interest, that did not necessarily make any difference to the importance of discussing a new subject. Amongst the new subjects of aeronautical interest it seemed to the Council that the Prandtl theory was one of the best for discussion and Mr. Glauert, of the Royal Aircraft Factory, had undertaken to prepare a paper and initiate a discussion. Amongst the members of the Society were quite a number who could speak with first hand knowledge of the subject, and by that means the value of the Prandtl theory, its application to practice, etc., would be more widely appreciated and a more or less organised body of thought formed. In these discussions they would not be limited as to time; the sittings must not be unduly prolonged individually, but they could be adjourned as necessary. It was proposed to hold the meetings in the rooms of the Society. The Chairman then called on Major Low to read his paper.

Major Low, before reading his paper, said: I am very much touched by the way in which the Chairman has returned the soft answer in advance, but I shall wait until I have heard his second speech before I say more about it, and shall proceed with the paper at once.

REVIEW OF AIRSCREW THEORIES

BY MAJOR A. R. LOW, FELLOW.

The early ideas of a marine screw seem to have been based to some extent on the invention of Archimedes for raising water by turning a helicoidal pipe or channel about its axis inclined at a less angle than that between the helix and a plane perpendicular to the axis.

At other times the very imperfect analogy with a screw turning in a solid nut seems to prevail. The introduction of the notion of slip allowed of a fairly complete definition of the kinematical state of the screw, without attempting

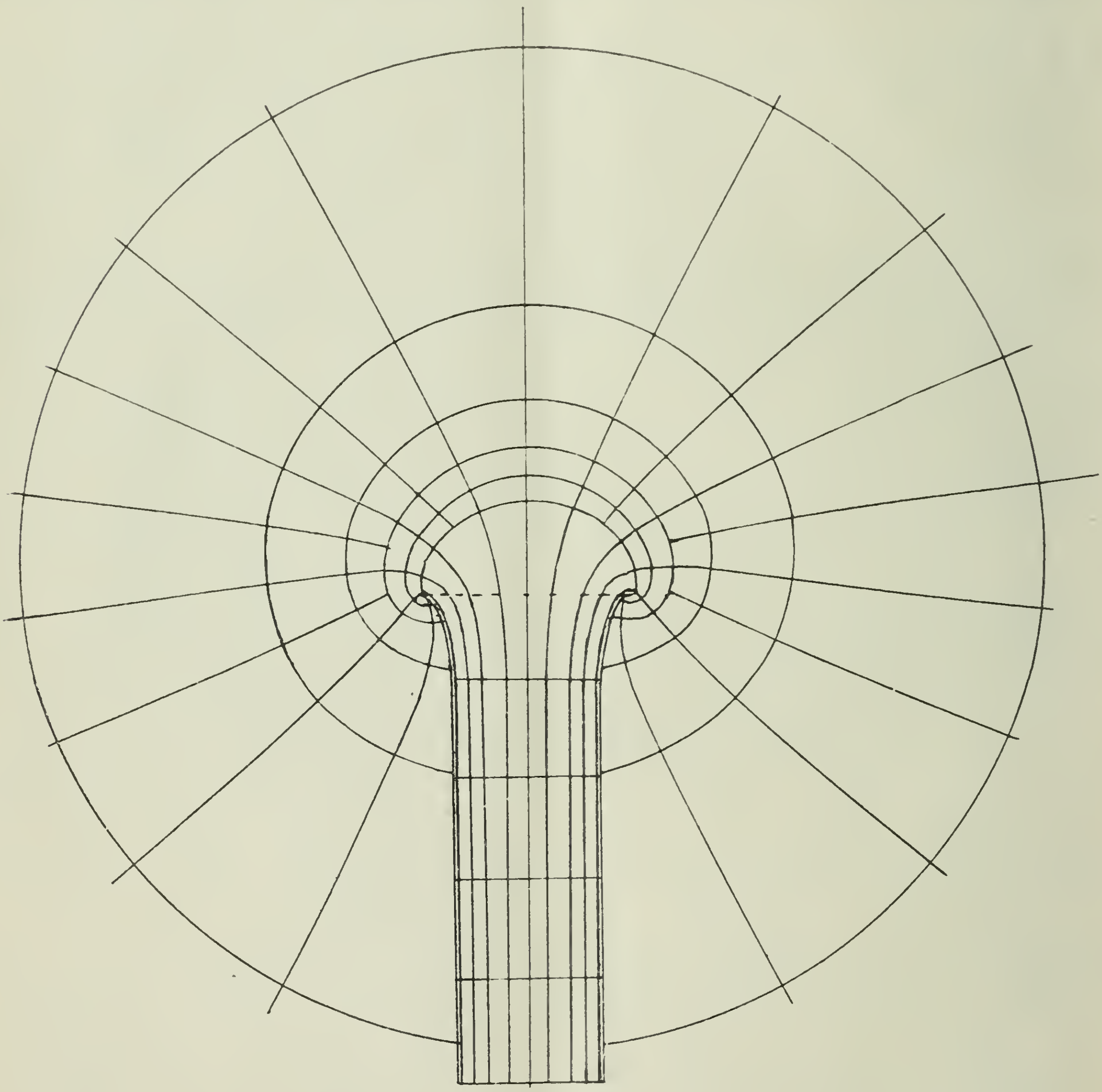

Fig: I.

to describe the motion of the surrounding fluid.* Early water screws (1810-1840) had one, two or more helicoidal surfaces of a complete turn or more, which were in fact the blades of modern practice with enormously exaggerated chords.

Passing over the claims of the Chinese, the suggestions of Hooke, and

* The French word *régime* is rendered by the word "state," qualified where necessary for clearness by appropriate adjectives.

the unsuccessful attempts of British, French and American inventors, we find one Smith with a vessel of over two hundred tons arriving, about 1840, at the decisive result that a two-bladed screw with chords one-sixth of a complete turn gave far better results than the Archimedean form.

H.M.S. "Rattler" (1843) achieved some ten knots, and two years later the "Great Britain" crossed the Atlantic for the first time under screw propulsion.

From that date there has been an enormous literature, patent and technical, on the subject. Rankine was one of the first to give a precise hydrodynamical

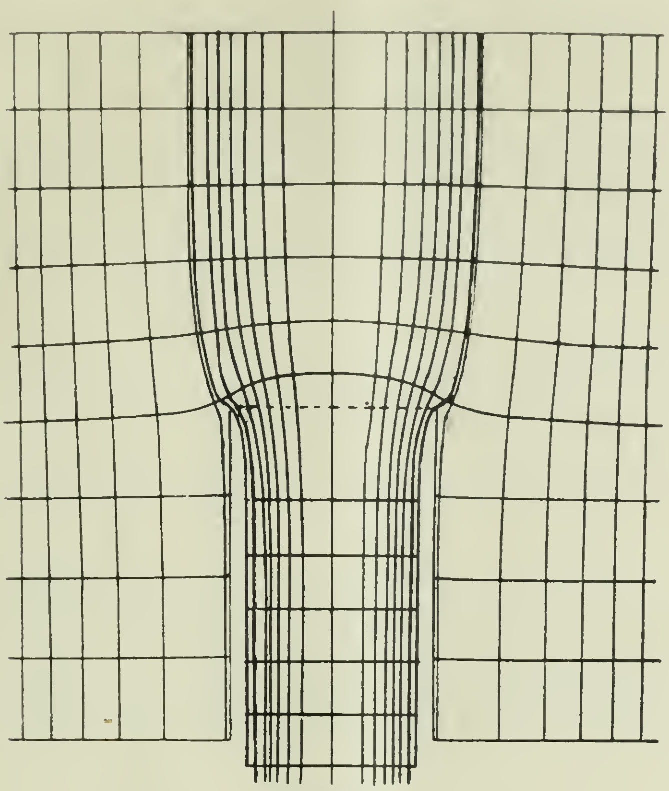

FIG: 2.

form to the theory of water screws (I.N.A., 1865). William Froude, Coriolis and others have claims to joint honours.

In Rankine's theory the screw was regarded as an actuator disc imparting momentum to a column of water drawn from the front and thrust to the rear.

Mr. R. E. Froude, son of William Froude, gave a more modern form to the theory by assuming that each element of blade put in motion the column of water standing on it axially (I.N.A., 1899).

Sir George Greenhill showed later that if rotational velocity only were imparted by the blades to the column, this would account for axial thrust on the screw. As we now know very clearly, both axial and rotational velocities are imparted to the column of water or air, the theory being incomplete without the consideration of both components.

Figs. 1-3 show conventionally the flow through the screw disc acting as an actuator, the column of water thrust back remaining a circular cylinder for an indefinite distance. The kinematic states are those of 100 per cent. slip or standstill, 50 per cent. slip and 20 per cent. slip.*

* For the nature of the actuator and for accurately drawn diagrams of the flow, see H. Kimmel, Z.f.F.u.M., 24th February, 1912, p. 53.

An interesting discussion took place (I.N.A., 1911, p. 139) between R. E. Froude and Professor Sir J. B. Henderson (representing different Admiralty Departments). The point in dispute was really the conservation of energy in the surrounding fluid. With a double source in the shape of a flat disc not in motion relatively to the undisturbed fluid at a distance there is a finite amount of kinetic energy (Lamb, p. 139, equ. 5) which is completely conserved in a perfect fluid. If the disc is in motion relatively to the fluid, if we suppose its strength unaltered the energy is unaltered. But if the strength vanishes for

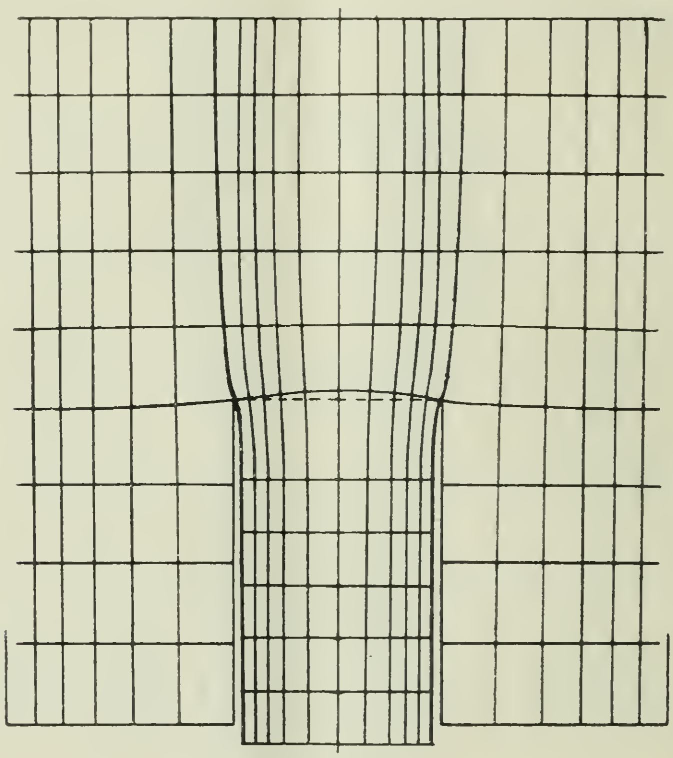

FIG: 3

a given axial velocity V_0 and if it is proportional to $V_0 - V$ for values of V not exceeding $V_0/3$, say (30 per cent. slip), then the energy is proportional to $(V_0 - V)^2$ or $V_0^2\ (1 - s)^2$ where s is the fractional slip.

In a perfect fluid, then, for any state (given strength and slip) there is complete conservation of energy once steady motion is established.

Actually there is very complete dissipation of energy, except in small enclosed basins or chambers.

If the speaker has not misapprehended the case, Henderson had the better in theoretical discussion and Froude in his conclusions.

The Rankine-Froude theory, with certain corrections for hull interference, has been of service in laying down limits for thrust energy and efficiency, but it depends for its application on empirical coefficients expressing mean velocities and momenta. In principle, it is an application of Bernoulli's theorem connecting velocities and pressures.

The columnar nature of the outflow tacitly accepted above is, in fact, well established experimentally. Its mechanism will be explained by Prandtl's theory.*

* It is of interest to refer to the R.A.E. experiments on flow about an airscrew whose tip speed exceeds that of sound. In this state the axial columnar outflow disappears and radial outflow is set up.

The Aerofoil Analogy

The next method to be described is now generally known as Drzewiecki's, and indeed this scientific engineer has overwhelming claims on the score both of priority of idea and of practical development (1892).

The main idea is now so familiar that we are apt to under-estimate the insight shown by Drzewiecki at a time when there were no precise data on lift and drag coefficients, and when the very idea was vague.

He considers that cylindrical sheets, contained by co-axial tubes of revolution, flow past blade elements in a manner kinematically nearly identical with the flow of plane sheets across aerofoil elements. Each such sheet—plane or figure of revolution—he supposed to be kept in its steady position by the adjacent sheets on each side. Thus each element had an invariant reaction, for a given kinematical state, with invariant components parallel and perpendicular to the element of the path relatively to still air. Assuming, further, that the reactions were proportional to density and path velocity square, he obtained coefficients of reaction closely analogous to lift and drag coefficients of an aerofoil. Finally, assuming a simple blade shape (constant chord) and integrating over the blade, he obtained expression for thrust and torque.

Applying the well-known theorem of the mean, he replaced the variable coefficients inside by mean values outside the sign of integration. Comparing these expressions with observed thrust and torque, he obtained experimental values for mean lift and drag coefficients. Having accumulated a sufficient number of values for different incidences, etc., he was able to design new airscrews with different geometrical pitches, blade lengths, for various working states.

Lanchester (1906) gives a blade element theory which in many respects is identical with Drzewiecki's, but the speaker always failed to establish a routine for numerical computation from his work. Only when Drzewiecki's theory came into his hands, in a brochure, " La Technique de l'Hélice Aérienne " (Gaston Camus, Paris, 1909) and when Eiffel's wing tests were published was it possible to make calculations which had a fairly close relation to practice.

The present writer's paper read to this Society was one of the first systematic accounts in English. (A paper by Bolas, dated March and June, 1912, R. and M. No. 65, may have been published earlier, and gives a useful summary with examples.) In the discussion Major Bramwell offered strong criticisms on the ground that there was no experimental evidence ! He stated that it was possible to design a screw without any theory at all, and quoted (with reserve indeed) the astonishing statement that anything that looks like an airscrew has an efficiency of about 70 per cent.

Time sometimes brings its revenge, and half a year later Mr. Bramwell read a paper before this Society on his experimental work at the N.P.L., which put Drzewiecki's method on an independent experimental basis, so far as regards any ordinary working state occurring in flight.*

Apparently Major Bramwell offered the criticisms referred to, although they were already destroyed by his own work. Possibly the A.C.A.'s method of dating explains the matter. Open publication is the only real test of work, and this was often from one to two years after the dates borne by the papers.)

The geometry or kinematics of the airscrew is simple enough in principle, but it leads to fairly complicated algebra. The speaker showed how to express

* The following dates are interesting :—
Low, Drzewiecki's Theory, Journal, October, 1913.
Bramwell, Propellers, Journal, April, 1914.
Bramwell, A.C.A., R. and M. 82, March, 1913 (!).

Drzewiecki's integrals by means of hyperbolic functions, not only for uniform chord but for a variety of forms.

Mr. Riach has done excellent work in developing the analytical treatment (Airscrews, 1916, in "Aeronautics," and in this Journal).

Soreau's chapter on integration (l'Hélice Propulsive, Soc. des Ing. Civ., 1911, p. 149) is admirable.

Of course, the known mathematical devices of graphical and statistical integration enable all difficulties of analysis to be avoided, but it is a great advantage to have a number of forms worked out analytically and tabulated.

Flow Round Airscrew Blades

It is evident on the most cursory observation that the average flow round an airscrew is well represented by Figs. 1-3 if a rotary motion is given to the columnar wake or outflow.

There is a tacit assumption in taking coefficients from aerofoil tests and applying them directly to blade elements that the relative flow of air is not sensibly disturbed until it comes under the direct influence of the blade, just as it is undisturbed save by the aerofoil in a monoplane test. But if aerofoils are placed in tandem, the rear foil is sensibly affected by the leading foil.

If now a multiple cylindrical section be developed on a plane, the blade sections in a normal working state of the screw will appear as a multiplane with high stagger. There is, therefore, to be looked for, a mutual interference. This was suggested by Bolas, by the writer, and by numerous German authors. Lanchester gave a detailed statement (Aerodynamics) many years earlier.

The claim made on behalf of Messrs. Wood and Glauert of the R.A.E. that they were the first to point this out is based on ignorance of the literature of the subject. They have, however, the credit of having put into practice a multiplane test with a view to measuring the mutual interference, R. and M. 639.

The speaker considered this at the time as the most promising development yet put in hand. It is, however, superseded by Prandtl's theory, though it may be a highly useful check.

Another view of interference is based on the observed inflow. At the N.P.L. much work has been done on measuring the root mean square velocity round annular spaces between concentric circles.

In Froude's actuator theory, where the actuator is a thin disc, it is shown that in a perfect fluid half the acceleration takes place before the disc and half behind.

Accordingly, it has been proposed to take half the total increase in mean velocity as altering the true angle of incidence of the blade elements. The results, far from correcting the simple aerofoil analogy, make it more discordant with observed thrust and torque. This leads to the introduction of empirical coefficients to correct the correction. (Full discussion and A.C.A. references will be found in Bairstow's "Applied Aerodynamics," Chapter VI., and in Fage's "Airscrews." Their foreign references are inadequate.)

Dr. Watts has given out of his very wide test experience the view that the uncorrected Drzewiecki method gives closer results than the inflow correction (*cf.* Screw Propellers, Chapter VII., Appendix). Further light was thrown on the whole subject by Drzewiecki's crucial experiment (Theórie Générale des Hélices Aériennes, 1920, p. 128). With a single blade, measuring instantaneous values of the velocity instead of the root mean square velocity round a circle, he showed that it was pulsating, rising sharply to a peak and falling less steeply nearly to the undisturbed velocity.

The immediate inference is that by far the greater part of the inflow near a blade is due to the blade itself. This, as will be seen, is substantially confirmed by Prandtl's theory.

The stroboscopic experiment was brought forward by the speaker in a discussion on Sir Richard Glazebrook's lecture (26th May, 1920; Journal, September, 1920), Dr. Stanton's work having been quoted as the latest advance in airscrew theory. The speaker is not deterred by the voice of authority from saying frankly that the application of Froude's ideal velocity ($V_0 + V/2$) at the blade finds no justification in the actual phenomena. Dr. Stanton's work, of course, has its place quite apart from this application. But it must be emphasised that Drzewiecki's stroboscopic method is far more illuminating, just as the oscillograph reveals much that the ordinary ammeter and voltmeter fail to record. Dr. Watts has since given a more detailed account and has drawn similar conclusions (Journal, July, 1920); but some amendment is required to his method of superposing velocities.

Professor Bothezat, on the contrary, opposes these conclusions (Journal, November, 1920, p. 597). His argument is ingenious. But for the true inflow there would be a diminution of the undisturbed flow at angular distance π to compensate the increased flow near the blade. It seems to the speaker that undisturbed flow at a distance from the blade is the state that matters.

Mr. Riach (Journal, September, 1920, p. 527) evidently takes the same general view as Dr. Watts and the speaker, and develops it at length in "Aeronautics" If mild criticism may be offered, his physical ideas are rather submerged in symbols, *apparent rari nantes in gurgite*. His paper (Journal, February, 1922) works out the "cascade" correction as a logical extension of Drzewiecki's method.

The Lanchester-Prandtl Theory

All these attempts to form physical theories are brought into perspective by the brilliant developments of the last twenty years, principally in Germany.

In this country Mr. Lanchester has given remarkably complete descriptive accounts of the theory here exposed (Aerodynamics, 1907). Unfortunately, research in this country has completely failed to respond to the stimulus. This may be partly explained by the absence of any account as to the manner in which Lanchester arrived at his conclusions, and of references to the work of others, from whom he may be supposed to have drawn suggestions or experimental evidence. Further, all attempts to base a routine of calculation on his exposition have failed the speaker and such of his friends as he has consulted. This is now explained by a glance at the immense amount of work, in several highly specialised directions, required to put the new theories on a quantitative basis.

Mr. Lanchester does indeed claim to have made such computations with satisfactory results, but in view of the rudimentary state of the theory at the time their value cannot have been very great in the predetermination of an airscrew.

None the less, Lanchester's work shows remarkable insight into the physics of a problem that baffled the scientists of last century. Had our physicists followed up his ideas, this country might have shared in the work. Although not till recently honoured in his own country, Lanchester has had very full recognition in Germany, unlike Bryan, who is generally ignored. In Joukowsky's words, "Lanchester's distinguished service is the elucidation of the transition from plates of infinite span, which render the space containing the fluid multiply connected, to finite plates in simply connected space" (Z.f.F.u.M., 1920, p. 282). Compare this with Reissner's reproof to German writers, "Bryan's

highly distinguished service in first (1904) putting the problem of aeroplane stability in complete mathematical form should not be ignored in citing names " (Jahrbuch d. Wiss. Gesell. f. Luftfahrt, 1915/15, p. 141).

Kutta gave the new theory its first impulse by showing how a circle with streamlines past it and circulation round it could be conformally represented by an arc of a circle likewise with flow past it and circulation round it.

On combining the two, a reaction is obtained at right angles to the undisturbed uniform flow (Ill. Aer. Mitteilungen, 1902).

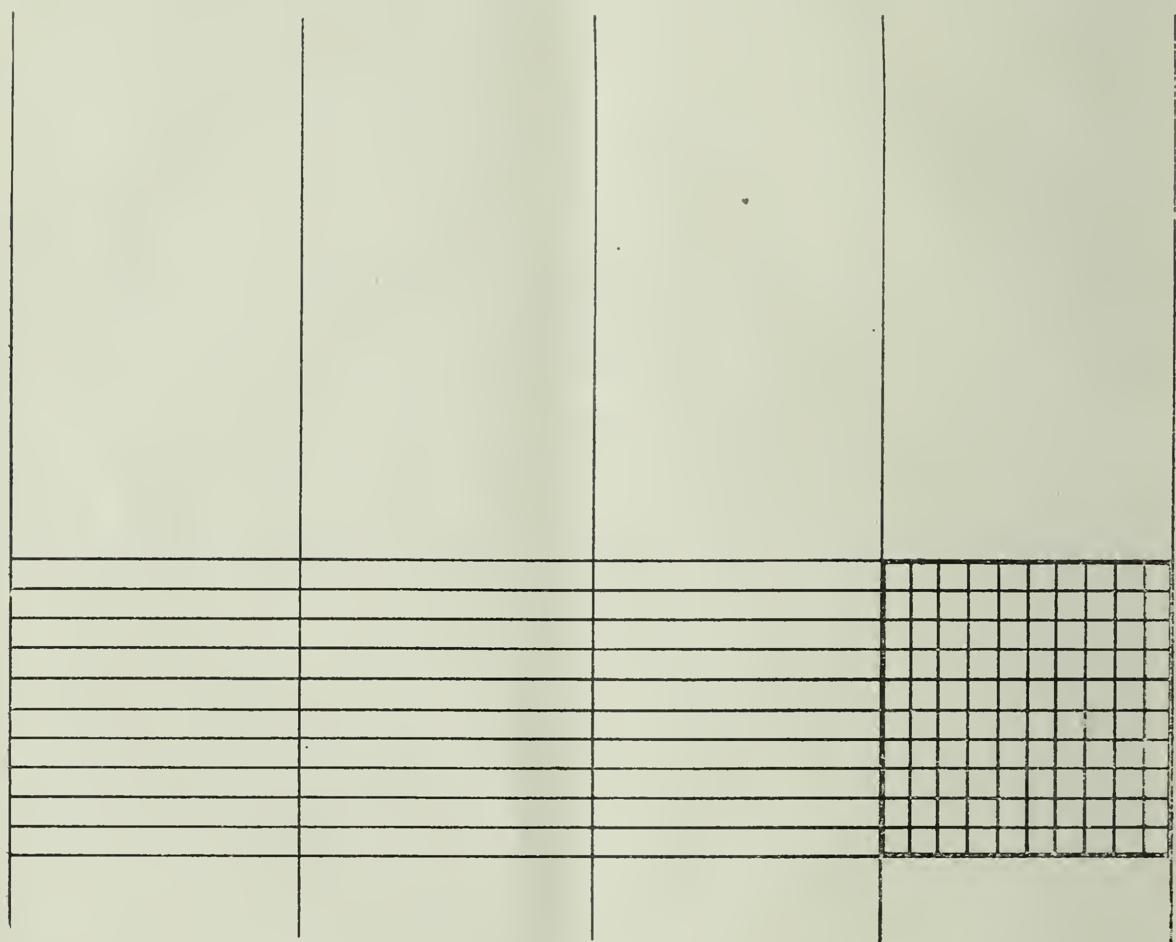

FIG. 4.

Joukowsky extended the transformation to wing-like profiles or sections, now known as Joukowsky profiles (Bull. de Koutchino, 1906; Aérodynamique, Paris, 1920).

Before describing the results obtained by Betz, Munk, Trefftz, Blasius, v. Mises, Kármán and others, as well as by Prandtl himself, the speaker proposes to give a few of the simplest cases of flow of a perfect incompressible fluid in a form which can be followed by engineers without specialised mathematical training. Those who are more fortunate will have patience for a few minutes.

The simplest case of possible motion of a perfect incompressible fluid is that in which every particle has the same uniform velocity. Fig. 4 shows the plane right section of the sheets of flow. Now consider Fig. 5; the rays from the centre are equally spaced and the radii of the circles are in geometrical progression. By choosing the ratio of the radii as exp. (θ) where θ is the angle between rays we get each small division as a square to the second order of small quantities.

Thus to each small square in Fig. 4 corresponds a small figure nearly a square in Fig. 5.*

By taking the subdivision fine enough we get the meshes as nearly square as necessary, and by mapping with the network of small squares as lines of reference we get small corresponding areas in each system, as similar figures.

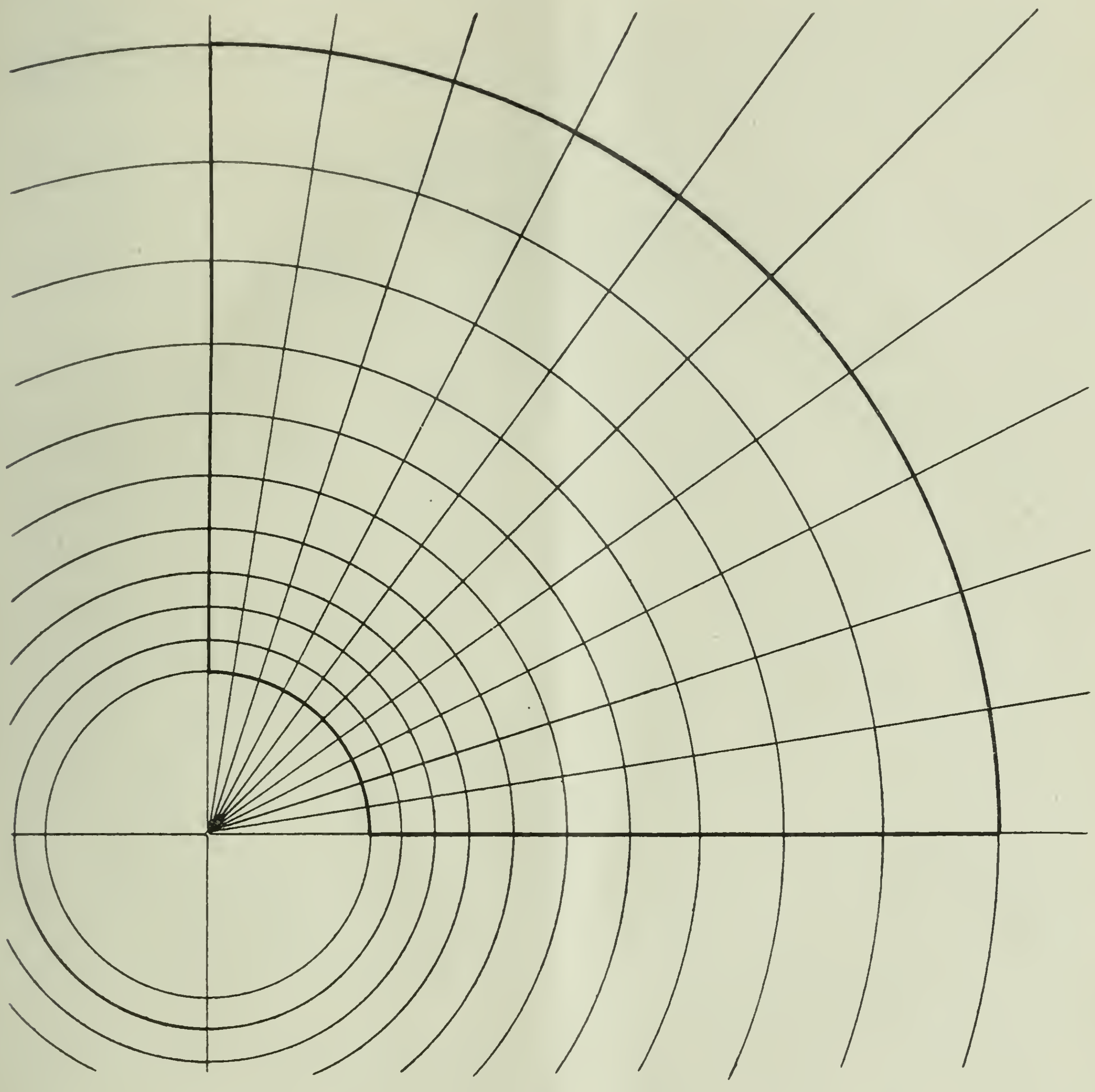

FIG: 5.

To the first quadrant in Fig. 5 corresponds the right-hand vertical strip in Fig. 4; to the whole circle corresponds the whole of Fig. 4 between the external vertical lines.

If we go round the circle many times anti-clockwise or clockwise, we get a series of strips left or right. The circle of very large radius corresponds to a straight line parallel to the horizontal at a great distance above, and the circle of very small radius to a similar straight line very far below.

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* We have exp. $(\theta)=1+\theta+\theta^2/2+ \ . \ .$ and by choosing θ small we get the ratio of the radii approximately $(1+\theta)$ while the sides are in the ratios $1 : 1+\theta/2 : 1+\theta/2 : 1+\theta$.

The simplest possible case of flow of a perfect incompressible fluid is illustrated in two ways by Fig. 4. We may either consider the vertical lines as lines of flow, and the horizontal lines of no flow, or *vice versa*.

A useful interpretation of lines of no flow in the analogous figures of electrostatics is as lines of equal potential, no work being done when a small charge is carried along an equipotential line. A similar function is obtained in this case as a velocity potential.

Let A and B be any two points (x_1, y_1) (x_2, y_2) in Fig. 4, joined by a curve s. Divide the curve into small arcs ds and project the velocity at mid arc on the tangent, and we get the product

$$v \sin\theta\, ds = v dy \} \text{ case 1.}$$
$$v \cos\theta\, ds = v dx \} \text{ case 2.}$$

Since v is constant, we get, in vector notation,

$$\int \mathrm{v}\, d\,\mathrm{s} = v\ (y_2 - y_1) \text{ in case 1, } = v\ (x_2 - x_1) \text{ in case 2.}$$

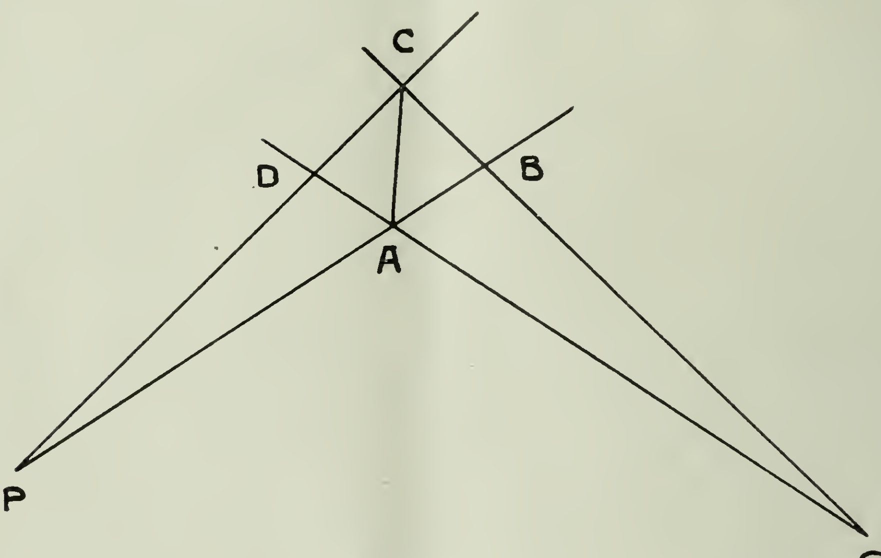


Fig. 6.

Hence the velocity potential difference between two points in either case depends only on the difference of potential between the equipotential lines they are on, and not on the path joining them. It may be denoted by $P_B - P_A$.

An advantage of knowing the potential function is that the velocity in any direction is found by evaluating dP/ds.

The lines parallel to the x and y axes are therefore lines of equal velocity potential in cases 1 and 2 respectively. It follows at once that integration round any closed path APBQA is zero, for the integral along APB is equal to the integral along AQB. Defining this integral taken round a closed path as the circulation, we find as a characteristic of this type of flow that the circulation round a closed path is zero, just as the work round an electrostatic path is zero.

Looking now at Fig. 5 there are again two interpretations, (1) the rays are lines of flow and the circles are lines of no flow. In case 1 the velocity is clearly inversely as the arc between rays, that is, inversely as the radius. Taking the circulation round any square, it is obviously zero by symmetry, the integral along one side being cancelled by the equal and opposite flow round the other.

In the other case (2) the circles are lines of flow by symmetry constant round a circle, and the rays are lines of no flow. The circulation is $v_1 \cdot r_1 \cdot d\theta$

$-v_2 \cdot r_2 \cdot d\theta$, and if this is zero $v_1 \cdot r_1 = v_2 \cdot r_2$, *i.e.*, the velocity, as in the last case, is inversely as the radius.

It can be shown that this condition is sufficient to make the circulation in any circuit zero, provided it does not include the origin. If, however, we integrate once round a circle of flow we get the potential difference $2\pi v r$ where r is any radius and v the corresponding velocity.

But $v r = v_1 r_1 = v_2 r_2 \quad . \quad . \quad . \quad = \kappa$ say, and κ is defined as the strength of the circulation. This is Helmholtz's vortex motion, and it is irrotational; in other words, a velocity potential exists, except at the centre.

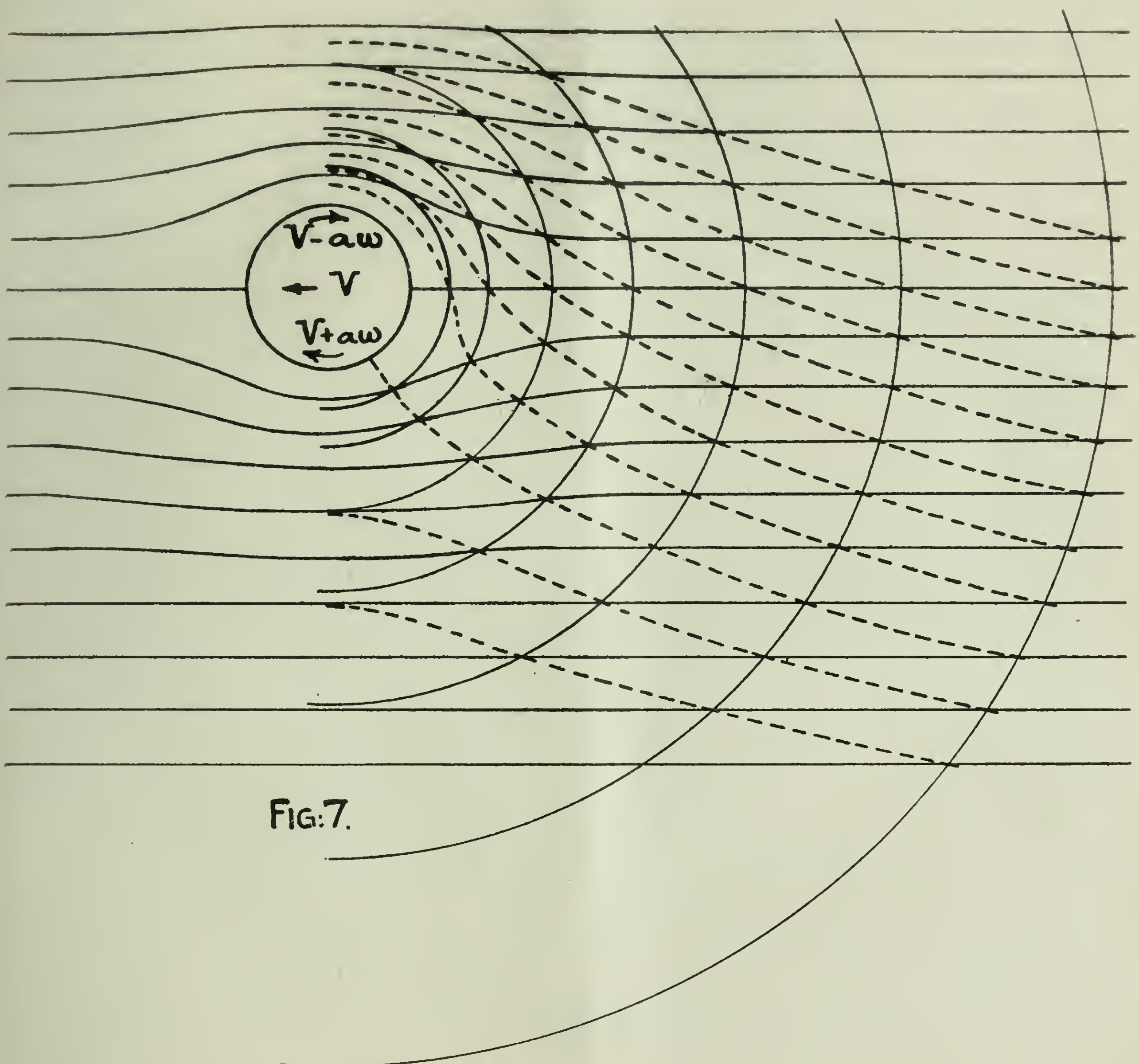
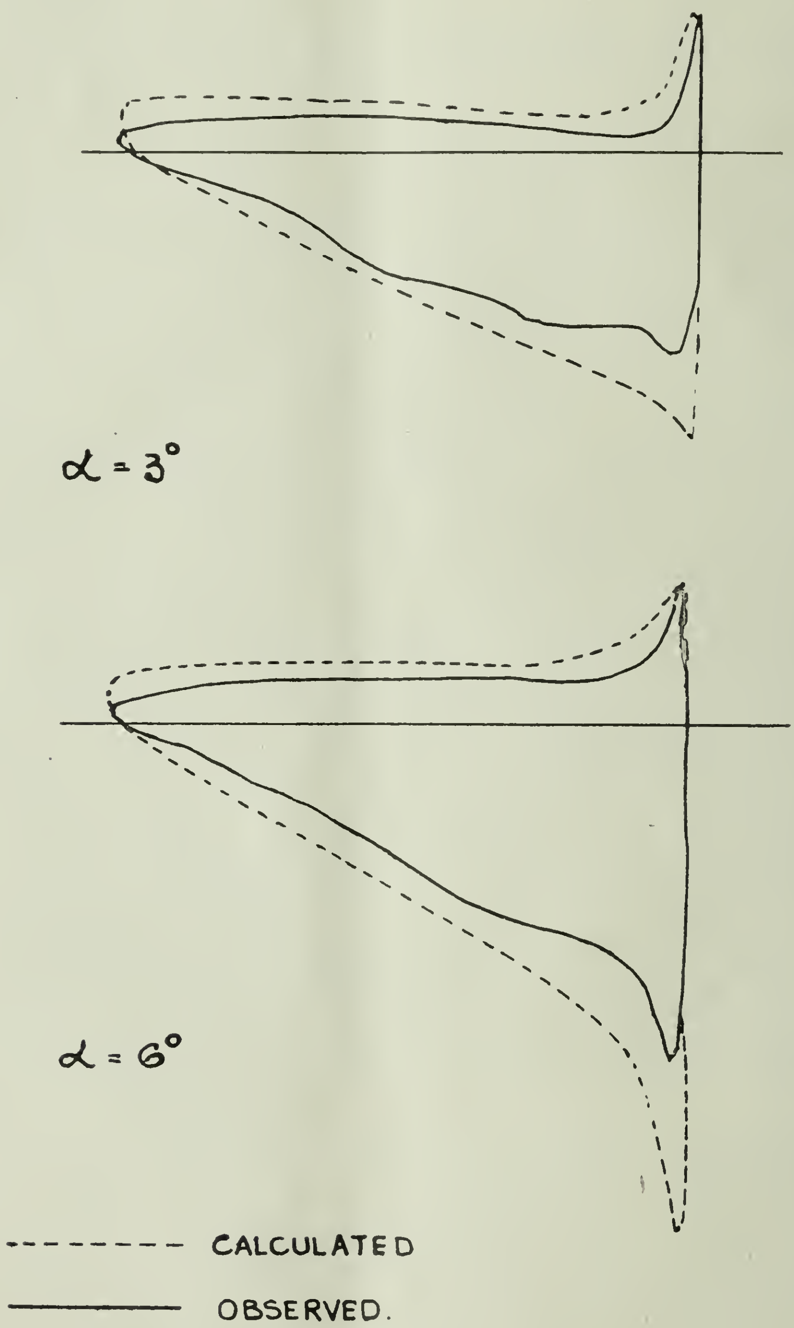


FIG:7.

There are certain restrictions in interpreting these results. At the centre of Fig. 5 the conformal nature of the representation fails completely, and in correspondence with this failure it is physically impossible to have a pipe of infinitely small diameter delivering fluid at infinite velocity and with infinite energy. It is likewise impossible for the vortex motion to continue up to the centre, where the motion would again require infinite velocity and energy. In fact, a core forms which is void or filled with air or fluid rotating uniformly or in some other physically possible way.

If two such irrotational or potential flows are superposed the resultant flow may be found graphically (Clerk Maxwell, El. and Magn., Vol. I., Chap. VIII.).

Let P Q (Fig. 6) be two sources, and let the rays in pairs enclose channels or tubes of equal flow, then the flow across A B from P is equal and opposite to the flow from Q, so that the joint flow is zero, and A B is a line of flow in the combined system. Applying this from point to point we trace out the new lines of flow. It is possible in this way to build up graphically more and more elaborate cases from simpler cases. We may also superpose



FIGS: 8 AND 9. DISTRIBUTION OF LIFT.

systems of equipotential lines, for the velocities along any path are additive; hence the differential coefficients in every direction are additive, and the potential functions themselves are additive.

As engineers, we may accept worked-out cases from any source, *e.g.*, from analysis or from Hele Shaw's experimental determinations (I.N.A., 1898).

An important case is found in Lamb's Hydrodynamics, p. 75. Superposing

the vortex motion of Fig. 5 we get Fig. 7. (It should be noted that this is not identical with the usual examples in electrostatics and electromagnetism.) The flow in Fig. 7 is the basis of the transformations of Kutta and Joukowsky. There is bunching of the lines of flow, showing increase of velocity and diminution of pressure above the circle, and thinning, showing decrease of velocity and increase of pressure below.

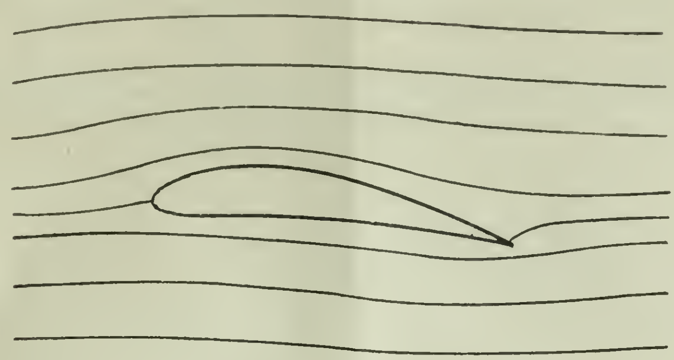


Fig: 10

This follows from Bernoulli's relation between pressure and velocity (ignoring the gravity field).

$$p_0 - p = \rho v^2/2 - \rho v_0^2/2$$

There is therefore a resultant pressure vertically upwards, the flow being horizontal at a distance. This reaction R is given by the equation $R = \rho \kappa U$ (Lamb, p. 76), where ρ is the density, κ the strength of the circulation, and U the undisturbed velocity at a distance. This holds for any boundary, as is seen by the principle that at a distance large in comparison with the boundary the lines of flow of the circulation are nearly circles with centre at the centre of position of the system.

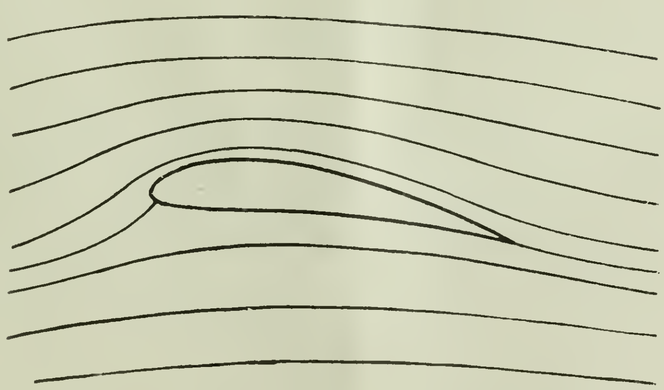


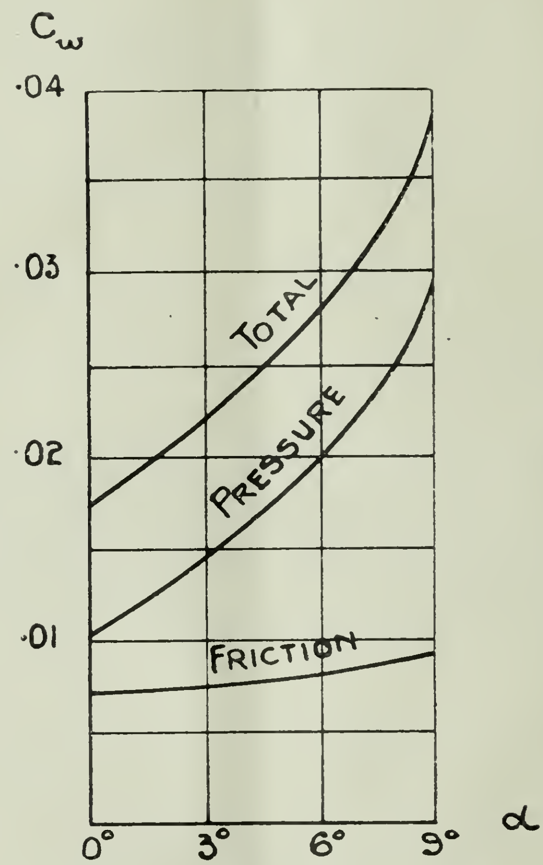
Fig: 11.

This useful principle also holds for lines of flow at a distance from any combination of sources. (See Lamb, p. 666, sect. 371-2 and notes, giving general references on the subject-matter of this paper).

Examples of circulation combined with linear flow are common in our playing fields. Rayleigh found a problem for investigation in the flight of a cut tennis ball (Mess. of Math., 1878) and Tait in the flight of a golf ball,

whose flight with under-spin, over-spin, right spin and left spin are only too familiar in the last three cases as topped, sliced and pulled drives, the first giving the easy and prolonged flight of a correct stroke.

The existence of circulation round a ball with spin and forward motion follows at once if we admit frictional reaction between the surface and the air in its neighbourhood, increasing with the relative velocity. The mechanism is almost intuitively established. We are, however, making the tacit assumption that the fluid acts as viscous near the solid boundary and as inviscid at a distance. This is, indeed, the basis of recent advances in hydrodynamics, and it has led, as will be seen, to a mechanism of flight which brings physical theory into something like accord with observed phenomena.



PRESSURE AND FRICTION COMPONENTS OF RESISTANCE.

FIG: 12.

To return to the work of Kutta already mentioned, the strength of circulation was so chosen that one of the two points of infinite velocity at nose and tail—that of the tail—was eliminated by causing the streamlines there to run parallel to the tangent.

Joukowsky extended the transformation of the circle to wing-like profiles (Fig. 10) with flow past it. Superimposing circulation he obtained (Fig. 11) with no point of infinite velocity, the strength of circulation being determined by the condition that the flow at the trailing edge is parallel to the tangent.

O. Blumenthal, working at the important laboratory at Aix, gave the distribution of pressure arising from this assumption (Z.f.M.u.F., 1913, 31st May, p. 125), and Betz (*ibid.*, 1915, pp. 175-8) has compared calculated with observed

results. Figs. 8 and 9 give the results at 3 degrees and 6 degrees incidence, and the general agreement between theory and wind tunnel measurement is very striking. The observed lift, and, it may be inferred, the true circulation, is about 75 per cent. of the value calculated on Joukowsky's supposition.

E. Trefftz (Z.f.F.u.m., 1913, p. 130) and v. Mises (*ibid.*, 1917, p. 157) develop graphical methods of carrying out the transformation from circle to Joukowsky's profile, of which Miss M. Barker, B.Sc., has given a summary and example (R. and M. 788).

A somewhat different view may be taken of the flow round an aerofoil. No one who has watched the flow of water past a barrier can have failed

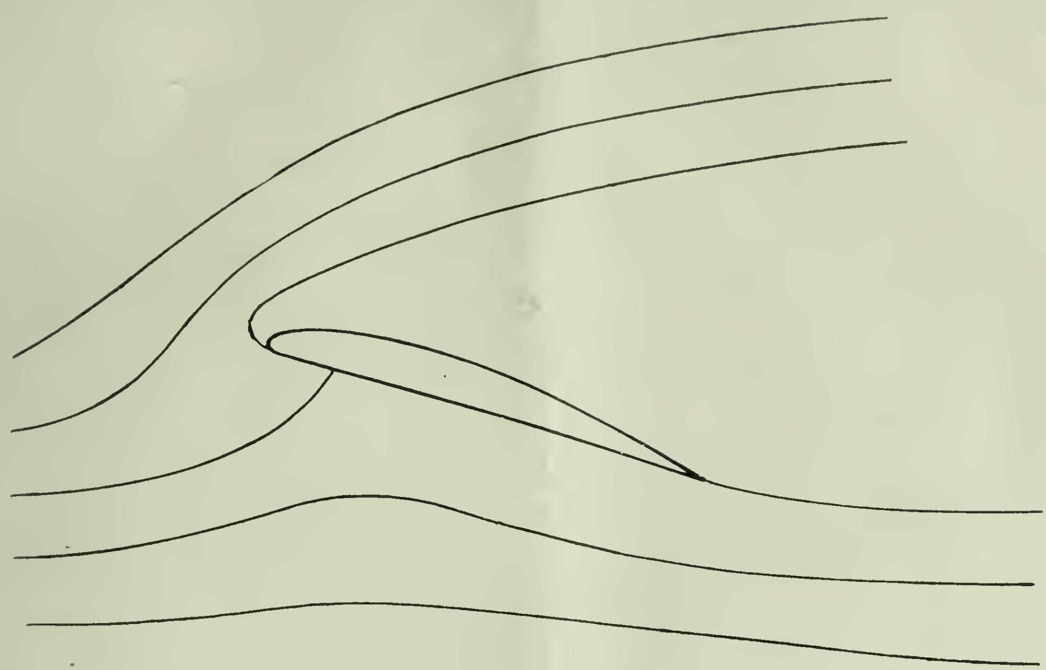


FIG: 13.

to observe the formation of a sheet of separation marked at times by air bubbles drawn in. The motion of the two surfaces past each other forms at once a vortex sheet as nearly as molecular physics will allow (Fig. 13).

The molecular mechanism involved may be easily enough pictured in a general way, but much elucidation by the methods of molecular physics is required before it becomes a subject for computations.

This sheet, as soon as formed, breaks up into a row of finite rotating cores surrounded by fluid in motion very nearly identical with that due to ideal vortex filaments concentrated at the centre as in Helmholtz's theory.

These eddies pass away at first along the line of separation between free stream and dead water, finally becoming confused, mixed together and dissipating their energy in the general body of fluid.

While they continue to exist they maintain a region of approximately dead water behind the barrier, but their breaking down reduces the pressure in the dead water.

The theory of flow with sheets of separation is due to Helmholtz, and Fig. 13 shows the general nature of the streamlines and the shape of the dead water area.

Fig. 14 shows diagrammatically the modification of the flow by viscosity.

At first it would seem that evidence for Helmholtz flow is evidence against Joukowsky's assumption. They may, however, be reconciled *for small angles of incidence,* by the consideration that the layer between the upper sheet of separation and the wing is very shallow and is rapidly set in motion approximating to Joukowsky flow. This is at least roughly confirmed by observation of flow at small incidence. (Cp. Captain Lafay, Technique Aéronautique, 1911, La Photographie du Vent.)

We have still to account for the setting up of circulation. At the face there is viscous friction between wing and adjacent air, proportional to velocity. This sets up a reduction of speed at the lower surface, which is equivalent to the effect of superposing circulation, assuming as before that at a distance the fluid acts as non-viscous (Fig. 14).

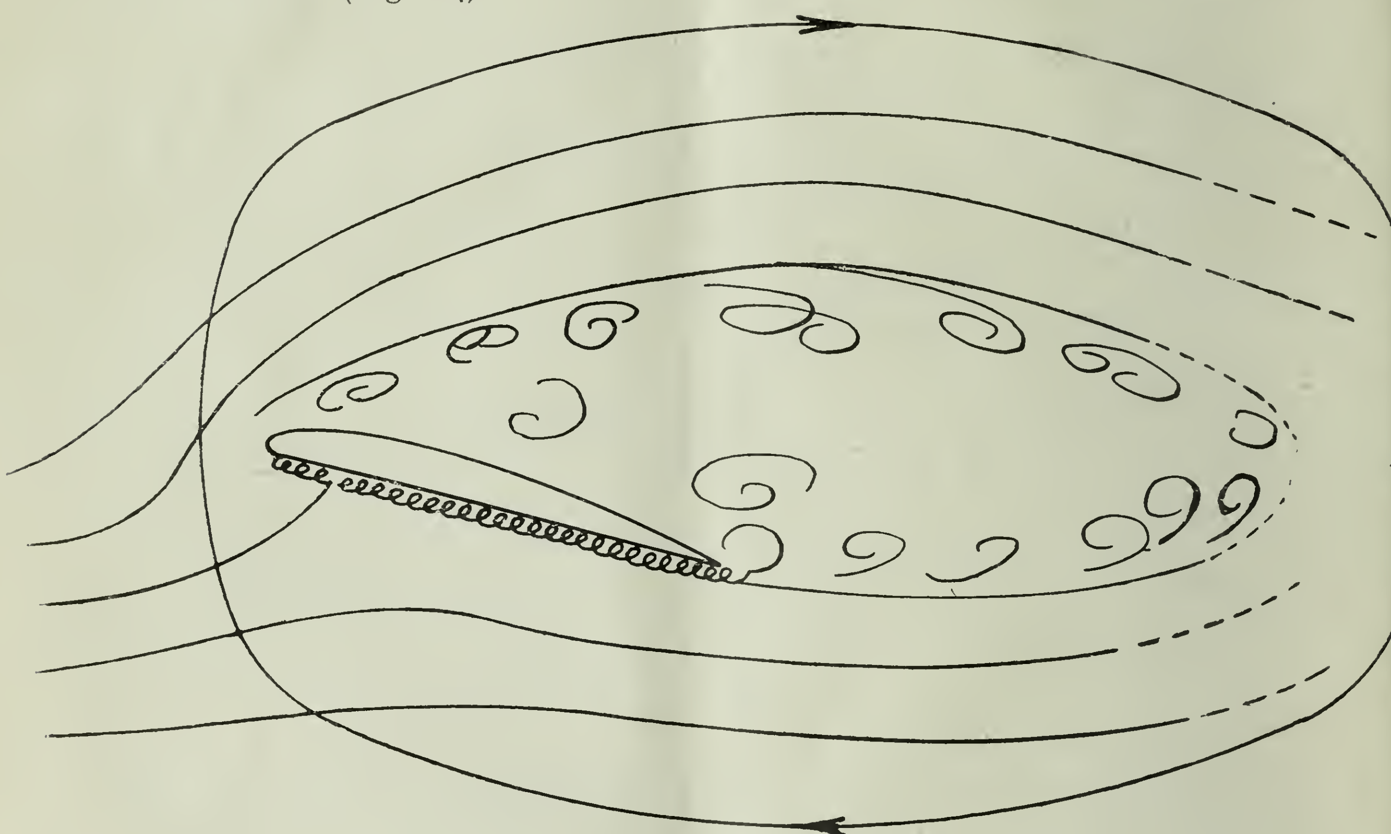


FIG: 14.

On the upper surface, the layer of air between the Helmholtz sheet of separation and the wing, acts as a lubricant and offers less retardation than the direct contact of the stream with the lower surface. But what circulation it does set up is opposed to action of the under surface.

At the same time, the two sheets of separation dissipating in eddies seem to be equivalent to a circulation which is in the same sense as that set up by the under surface.

Comparing the two expressions for the reaction $R = \rho\,\kappa\,U = k_L\,\rho\,c\,U^2$ per unit length, where c is the chord, we get $\kappa = k_L\,c\,U$.

But if we assume that the friction between the surface and the adjacent layers is viscous, the retardation will in fact be proportional to the chord c and the velocity U in agreement with the above relation. Thus k_L is the coefficient of circulation directly and of lift only indirectly.

If now we integrate round a closed path (in the form of a circle if desired for simplicity) large enough to enclose all undissipated eddies we get the approximately ideal circulation of the air at a distance (as an inviscid fluid).

Note added after the meeting:—This is incorrect as a statement of Prandtl's view. The circuit round which the circulation is taken should exclude the vortices which have become definitely detached from the trailing tip of the wing. These are carried down stream with a velocity approaching U, and their influence on the lift is to be neglected.

This very rough descriptive account of the mechanism at least takes qualitative account of the frictional part of the total resistance which separates itself from the induced or pressure resistance of Prandtl's theory, as will be seen below, and is a compromise between the Kirchoff-Rayleigh lift for Helmholtz flow, which is too small, and the Joukowsky lift, which is too large.

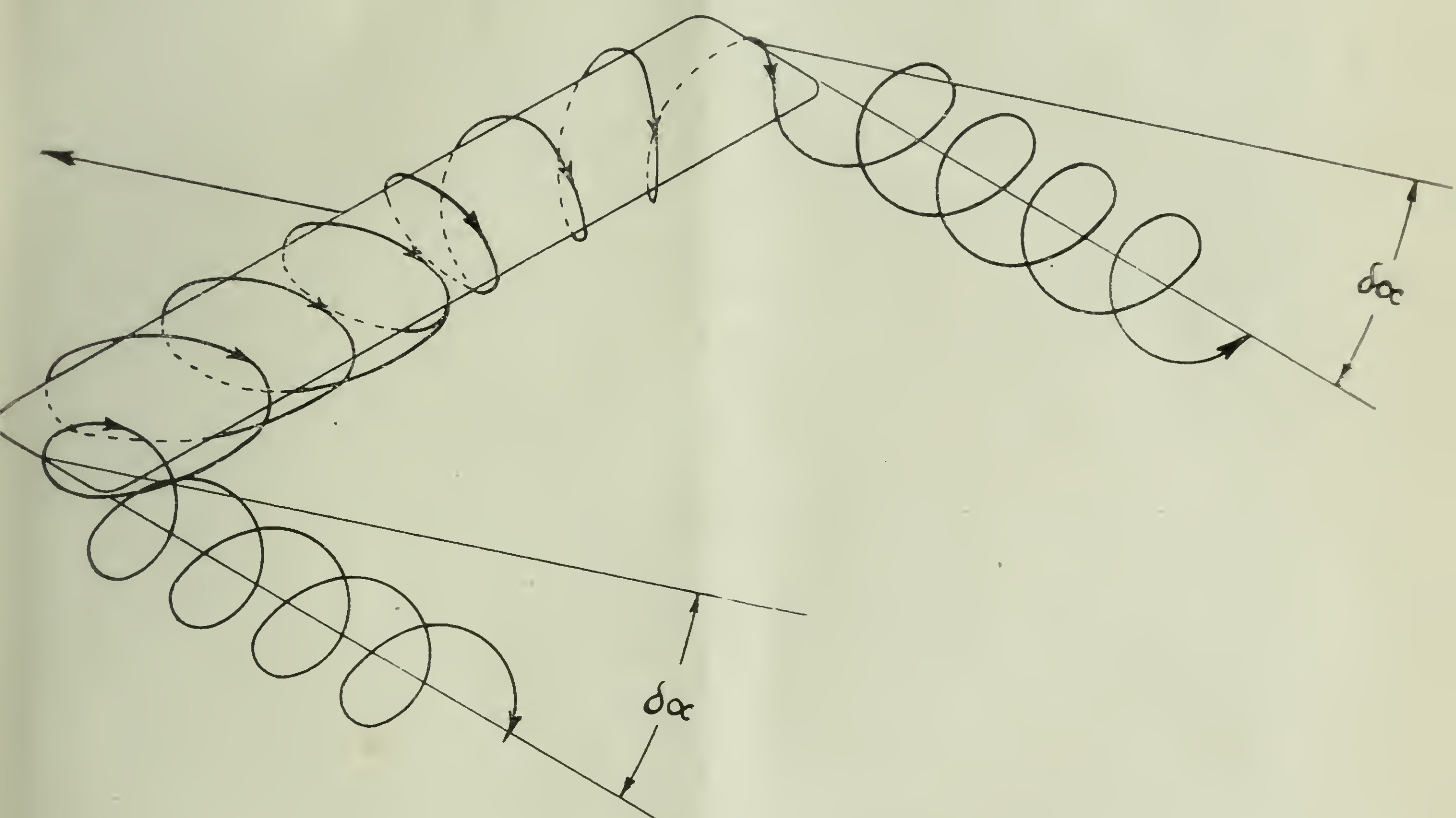


FIG. 15.

It also takes some account of the breakdown of the Joukowsky flow, or the approximation thereto, at small angles, and its transition to the Helmholtz flow modified by viscosity at larger angles. There it must be left at present, for it raises questions in the domain of the kinetic theory of fluids, and of the molecular physics of the formation and break up of eddy sheets, which have not yet been systematically examined, much less solved.

We now pass to consideration of the steps whereby Prandtl extends the Joukowsky method to wings of finite aspect ratio.

Analysing the forces on a wing in the light of these new conditions, Prandtl showed that within the flying range they can be largely accounted for. Circulation having been set up round the wing by the mechanism suggested

above, or by a modification of it, or by any other mechanism, we have vortex filaments distributed along the wing span.

As Lanchester has pointed out, such filaments cannot have their ends "in the air" in a perfect fluid, but must extend to a boundary or to infinity. In air

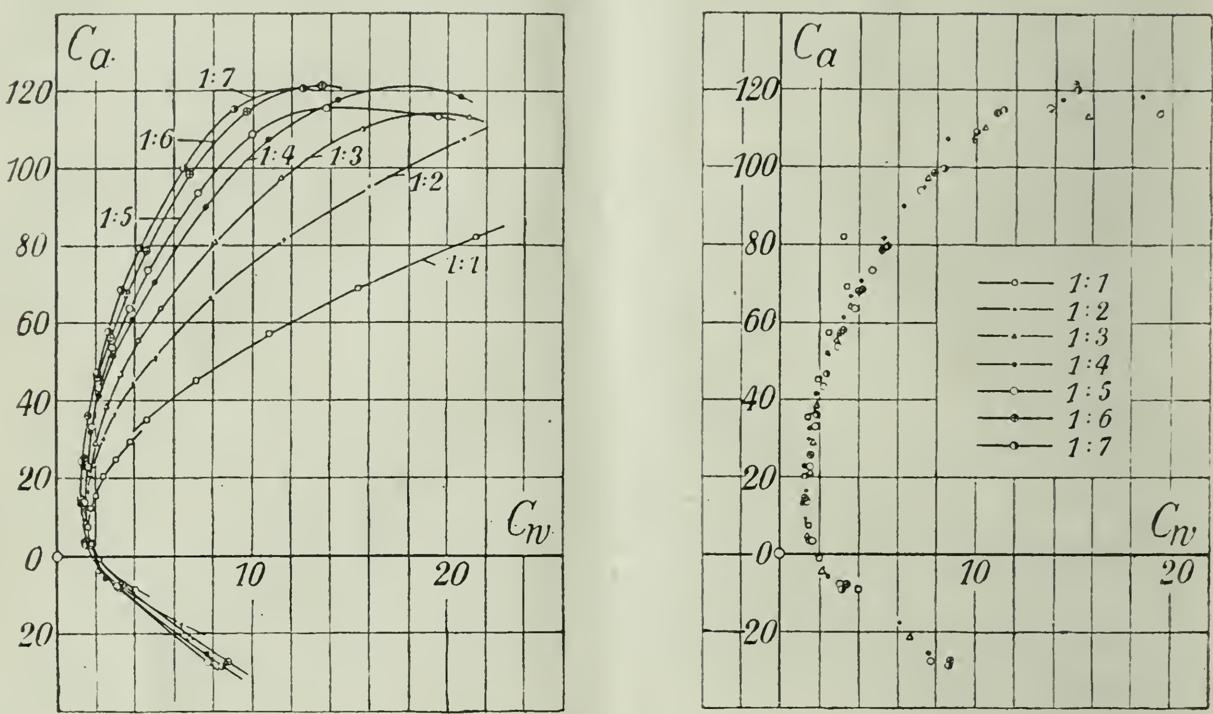


FIG. 16.

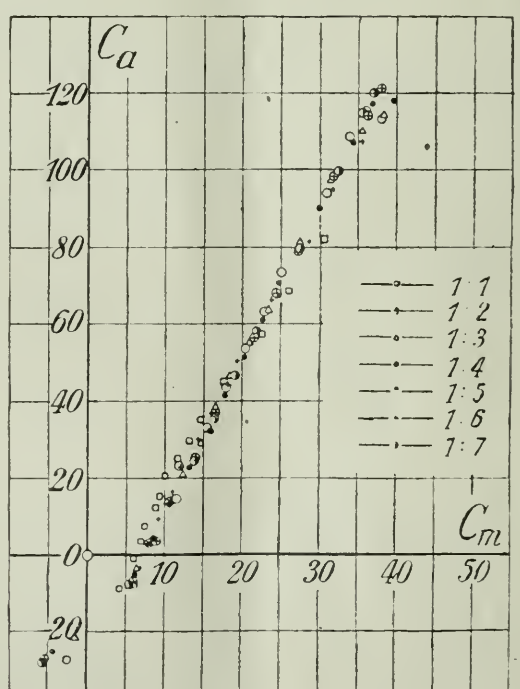


FIG. 16a.

and water the eddy filaments act approximately as in a perfect fluid, and once set up stretch out from the wing. As they leave the wing they are carried backwards by the stream. If the filaments leave the wing tip with a single concentrated core we get the so-called rectangular distribution of lift. But if they leave it in a sheet along the trailing tip we may get the distribution of least

induced drag, the so-called elliptical distribution of lift. For simplicity of discussion the former condition is assumed here. The vortices trailing from the wing tips set up a velocity at every point in surrounding space. In the first place they themselves are carried downwards at a definite angle, each by the other (Fig. 15). They set up at each point along the line of the wing a calculable downward velocity, which alters the relative wind in direction. The total reaction is considered as due partly to the non-viscous forces of reaction, and partly to the viscous forces along the surface. That part due to the non-viscous forces depends on the effective angle of incidence, and is therefore to be corrected for the alteration in the relative direction by the downward component due to the vortices. The other part is due to the surface friction and the tangential velocity near the surface, substantially unaltered at small incidences, by the deflection (Fig. 12), combined with eddy formations similar to those of a symmetrical profile. If then we know the coefficients of lift and drag for a given aspect ratio throughout the ordinary range of flying incidence, we can account for the effect of the downward component created by the trailing vortices, and calculate the lift and drag for any other aspect ratio, on the assumption that the profile resistance is independent of the aspect ratio, and depends only on the incidence and velocity. Examples of such reductions are shown in Fig. 16, and they are very conclusive. (Ergebnisse der Aero. Lab. Göttingen, I., 1921.)

Professor Bairstow (Applied Aerodynamics, 1920, p. 364) objects that Kutta's theory cannot indicate even the possibility of the well-known critical angle of an aeroplane wing, while the best he has to say for it is that the disagreement between calculated and observed values is not so great as to discredit the theory. The criticism is true of all theories of moving fluids which are assumed to be physically homogeneous. It is true even of Professor Bairstow's recent research work. It requires the methods of molecular physics to attack the problems of the molecular mechanism of moving liquids, of which the kinetic theory of gases gives glimpses.

But the theory of Lanchester and Prandtl finds an application and an independent verification in airscrew theory. If the blades act substantially as aerofoils, then circulation will be set up in exactly the same way, and trailing eddies will be generated at blade tip and root. The eddies at the root will be of the same sign, and will amalgamate into a single eddy with a single air core along the axis. The eddies at the tips will also be all of the same sign, opposite to that of the axial eddy, and their cores will, in the first place, leave the tips in the direction of the blade path through undisturbed air, thus forming a spiral core. Each spiral will impose a velocity on every point of the surrounding fluid, and the self and mutual influence of the spirals will determine their final steady position. The steady position and strength being known, the influence of each eddy may be calculated.

Lanchester (Aerodynamics, § 217, 1911 Edition) states that this phenomenon was observed by him from the after-deck of the s.s. "New York."

A glance at Flamm's photographic record of the formation of such vortices with air-filled cores, trailing spirally from the tips of a model screw blade, will bring conviction to the most cautious that the theory has an indisputable experimental basis. ("Die Schiffschraube, Berlin, 1909.) With stereographic apparatus the spiral cores of the trailing vortices, filled with air, can be seen vividly in three dimensions. The four spirals shown appear as left-handed. In Plate 10 (Fig. 3) and Plate 9 (Fig. 4) two vortex cores are seen clearly originating at different points on the same blade tip. They interlace and finally amalgamate. In Plate 9 (Fig. 3) a single core originates at each blade tip. The pitch remains very uniform for several turns, but once they begin to lose this form, complete irregularity soon follows, so that the steady position seems to be unstable.

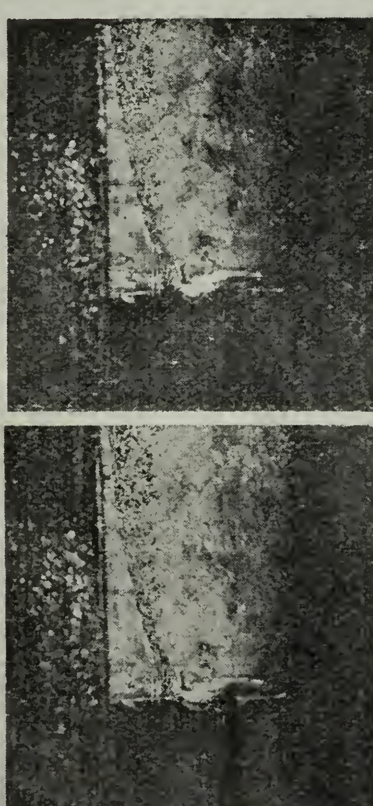


PLATE 10. FIG 4.

P.4. v = SLOW. n = 3500. g = 15 kg.

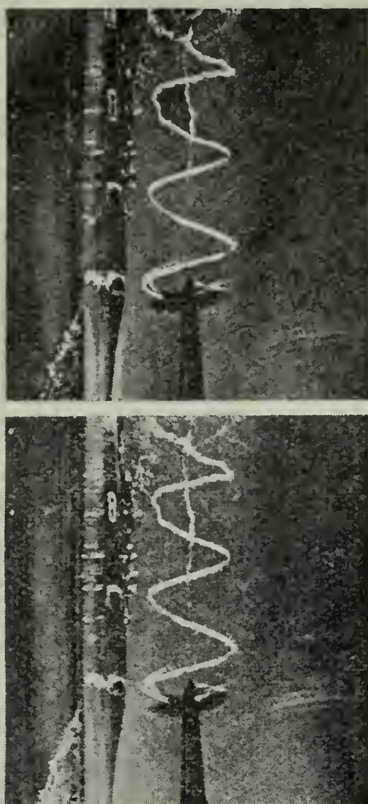


PLATE 9. FIG 4.

P.4. v = 1,9 m/s. n = 2500. g = 2,0 kg.

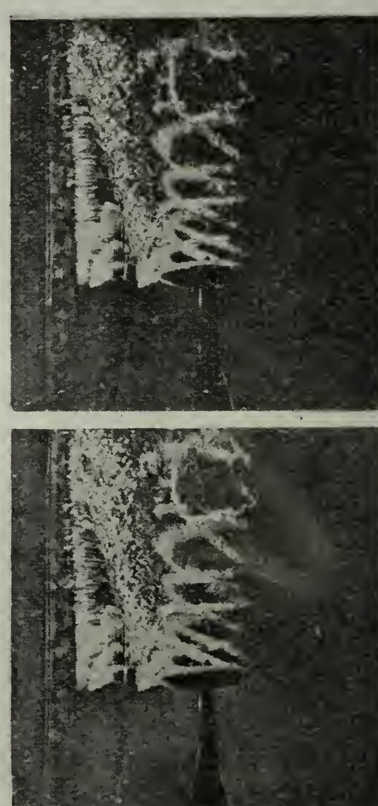


PLATE 10. FIG 3

P.4. v = SLOW. n = 3600. g = 15 kg.

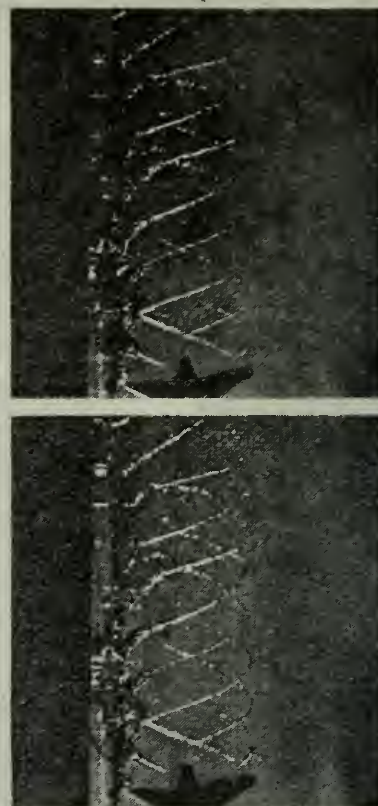


PLATE 9. FIG 3

P.3. v = 1,6 m/s. n = 900. g = 0

Adopting the hypothesis of a single core at each tip, if we know the pitch and circulation we can calculate the effect at any point in space (Fig. 17). The formulæ are given in the appendix. The speaker has not been able to find them in the literature of this subject or of the related subject of the magnetic field set up by currents in open spirals. He has made a good deal of progress in computing and tabulating the integrals, but heavy translation work has prevented their completion for this paper.

Fig. 18, which is similar to the figure in Prandtl's original paper (Abriss der Lehre v.d. Flüssigkeits Bewegung, Handwoerterbuch der Naturwissenschaften, Vol. IV., 1913), gives the components of velocity in the plane of the paper. A peripheral component also exists in the actual spiral stream lines.

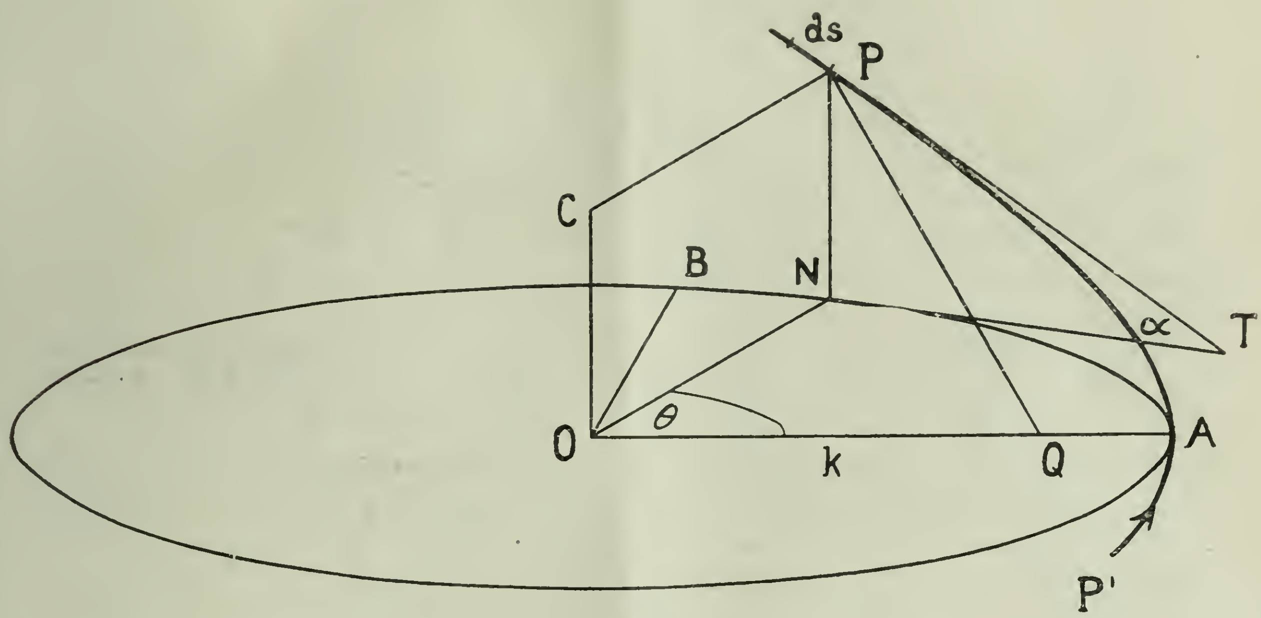


FIG: 17.

We may now find at each element of the blade the relative wind velocity corrected for the effect of the spiral and central vortices, and using the coefficients of lift and drag for infinite aspect ratio, compute the reaction on each element.

Such is the Lanchester-Prandtl theory of water and air screws, in the barest outline indeed, but, it is hoped, without the omission of any known physical phenomenon, so far elucidated.

Mr. Glauert gives an account of the theory in R. and M., 786 (which came into the speaker's hands a month ago—October 23). By taking the vorticity as distributed uniformly in a spiral belt covering completely the cylindrical surface of the outflow, the problem is reduced to one whose solution is known in electromagnetic theory. But in this way he loses one of the most characteristic effects, that of the high velocity near the blade. With a large number of narrow blades his assumption might be useful, but with a small number of blades, super-position

of the velocity due to the vortex and of the circulation round the blade on the undisturbed flow will give just such pulsations of velocity as were observed by Drzewiecki.

Again the portion of the vortex near the tip has the greater effect on the part of the blade which does most of the work. The other vortices have less effect in the order of the inverse of the distance of the blade tip from which they spring.

Thus de Bothezat's conclusion is negatived and Drzewiecki's original assumption is very largely justified, and the cascade arrangement is seen to be a rational approximation.

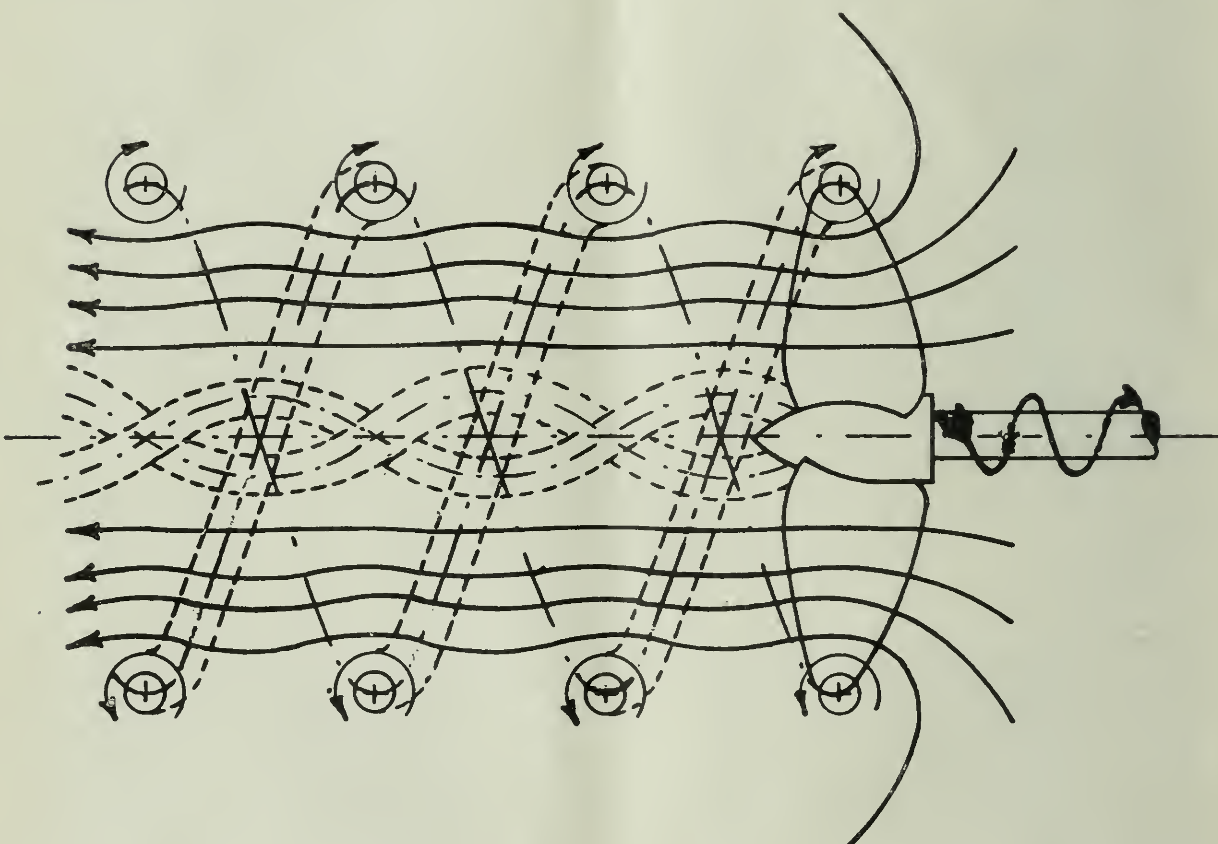


FIG. 18.

Self-interference is the most important factor in so-called inflow correction, and it is largely accounted for in the lift and drag coefficients of an aerofoil of the same profile and the same order of aspect ratio. The reduction of the inflow correction for blade incidence is almost complete, in so far as it differs from the aspect ratio correction.

Since this Paper was undertaken, M. Margoulis has published a lengthy paper on helicopters (Aérophile, Supplement), and Mr. Case is contributing articles to this journal. At the same time the development of Prandtl's theory has increased the present lecture to such length that it has become necessary to abandon any lengthy examination of helicopter theory.

Appendix I

Let P'AP (Fig. 17) be an arc of a helical vortex core; θ the angle between generators OA, CP; α the angle between any tangent PT and the plane AOB. Q is any point on the axis of a blade, cutting off a fraction k of its length, taken as unity for simplicity.

To find the components of velocity at Q due to the element ds of strength -1 (left-handed).

Projecting unit length of TP on the axes OA, OB, OC, the components are seen to be

$$-\cos\alpha\sin\theta,\ \cos\alpha\cos\theta,\ \sin\alpha.$$

The point Q is (k, o, o).

The point P is $(\cos\theta,\ \sin\theta,\ \theta\tan\alpha)$.

The components of unit length along PQ are

$$(k-\cos\theta)/r,\ -\sin\theta/r,\ -\theta\tan\alpha/r,$$

$$\text{where } r^2 = (k-\cos\theta)^2 + \sin^2\theta + \theta^2\tan^2\alpha.$$

The vector product of unit vector along ds in negative sense, and of unit vector along PQ has components given by dropping the first, second and third columns of the matrix.

$$(-\cos\alpha/r)\quad\begin{vmatrix} -\sin\theta & \cos\theta & \tan\alpha \\ k-\cos\theta & -\sin\theta & -\theta\tan\alpha \end{vmatrix}$$

These must be multiplied scalarly (numerically) by $ds/4\pi r^2$ to get the actual velocity components, hence putting $ds = d\theta/\cos\alpha$ and integrating

$$V_x = -\tan\alpha\int_0 (\sin\theta - \theta\cos\theta)\, d\theta/4\pi r^3$$

$$V_y = -\tan\alpha\int_0 (k-\cos\theta-\theta\sin\theta)\, d\theta/4\pi r^3$$

$$V_z = -\int_0 (1-k\cos\theta)\, d\theta/4\pi r^3$$

For any point Q $(k\cos\gamma,\ k\sin\gamma,\ o)$ in the plane of a generator at angle β with a blade axis, these expressions are easily generalised and become

$$V_x = -\tan\alpha\int_{-\beta} (\sin\theta - k\sin\gamma - \theta\cos\theta)\, d\theta/4\pi r^3$$

$$V_y = -\tan\alpha\int_{-\beta} (k\cos\gamma - \cos\theta - \theta\sin\theta)/4\pi r^3$$

$$V_z = -\int_{-\beta} (1-k\cos[\theta-\gamma])/4\pi r^3$$

$$r^2 = 1 + k^2 - 2k\cos[\theta-\gamma] + \theta^2\tan^2\alpha.$$

The upper limit is somewhat indeterminate, but the integral converges rapidly after the first few spiral turns.

Discussion

The Chairman said they had listened to an extremely interesting account by Major Low. Perhaps it was not so contentious as he had indicated at the beginning. Nevertheless, there was a good deal of material for discussion in the paper and there were several people present who were able to speak with knowledge on the subject.

Mr. McKinnon Wood said he did not know whether the lecturer had had the intention of giving an historical account of airscrew theory, but thought he was rather giving a logical account in placing the multiplane interference before the inflow theory. Historically, the Drzewiecki theory was followed by the application of inflow factors before, he believed, the multiplane idea was put forward, at any rate before the multiplane theory was developed. A great deal of credit must be given to Drzewiecki because his theory had formed the basis of practical airscrew design, and on that designers had worked with great success, although it suffered from the weak point theoretically that it did not define very clearly what an aerofoil element was. It said that the airscrew might be regarded as built up of aerofoil elements, but until we came to the work of Prandtl we never had a very clear definition of an aerofoil element. The actual airscrew designer simply took the data which were supplied him from wind channels which was obtained on aerofoils of aspect ratio 6, and fortunately that worked very well for some time, while the power transmitted was relatively small. At the same time, it was realised that the Drzewiecki theory might lead to calculations of efficiency which were so high as to be inconsistent with the Froude-Rankine theory. Various people had tried to work in the Froude-Rankine theory with the Drzewiecki theory, but they mostly realised that what they were doing was empirical to a large extent, and they therefore introduced empirical instead of theoretical factors and that had worked fairly well. The multiplane idea, the use of an "infinite" series of aerofoils as a means of determining the interference between the blades was an endeavour to replace the empiricism by a rational basis. It was a logical development of the Drzewiecki theory, but again, in his opinion, it suffered from the same lack of a clear conception of the "aerofoil element." If we experiment with aerofoils of aspect ratio 6 we get one answer for the interference, but if we use another aspect ratio a different answer would be obtained. The Drzewiecki theory was sound as a basis, but the interference of the aerofoil element had to be introduced into the problem. We should start with an aerofoil of infinite aspect ratio and apply corrections for interference between the blades and also for the interference which corresponds rather loosely to the aspect ratio correction for the ordinary aerofoil. At the end of the paper Major Low said:—"Self-interference is thus the most important factor in so-called inflow correction and it is largely accounted for in the lift and drag coefficients of an aerofoil of the same profile." He could hardly follow Major Low in this. He did not understand what was meant by self-interference. If by that was meant the disturbance of the air flow due to the aerofoil element itself, that did not seem to him to be interference. It was simply the flow that was present.

Major Low said it might meet the point if he used the words "with respect to infinite aspect ratio."

Mr. Wood further quoted:—"The demolition of the inflow correction or blade incidence is almost complete, in so far as it differs from aspect ratio correction." The term "aspect ratio" applied to an airscrew had always given difficulty. The aspect ratio of an aerofoil had a perfectly definite meaning, but there had always been a difficulty in assigning any meaning to "aspect ratio" for an airscrew blade.

There was a reference in the paper to Mr. Glauert's work recorded in R. and

M. 786. He did not think Major Low had given quite a fair account of it. This was based upon the work of Prandtl; but he knew of no success that had been achieved in developing an airscrew theory in Germany. It was a curious application of the Prandtl theory, because it seemed to eliminate the vortex arriving again at the two inflow factors of one half the slipstream speeds, but with the aerofoil element clearly defined as part of an aerofoil of infinite aspect ratio.

Early in the paper there was a reference to the R.A.E. experiments on flow about an airscrew whose tip speed exceeded that of sound and it was added that in this state the axial columnar outflow disappears and radial outflow is set up. In that particular experiment that is what actually happened, but he wished to make it clear that that was not necessarily due to the tip speed exceeding the speed of sound. More recent experiments had shown that the flow through an airscrew might be of the normal type when the tip speed exceeded the velocity of sound by 20 to 25 per cent. The experiment to which the lecturer referred was rather a freak experiment. The airscrew was run at a fixed point by an electric motor and was an airscrew of very low pitch with very fat sections.

Mr. R. McKinnon Wood (*communicated*): I do not follow Major Low's conclusion that the inflow correction is "almost completely demolished." May I ask space to make my difficulty clearer?

The use of the theoretical inflow factors of Froude and de Bothezat in a calculation using aspect ratio 6 aerofoil data is irrational; but Glauert's analysis based upon the Prandtl vortex theory leads to the use of just these factors with infinite aspect ratio data.

If by "self-interference" Major Low means the "induced velocity" due to the vortex system trailing from the blade under consideration, I agree that it constitutes the greater part of the interference in a two-bladed screw; but it can, I believe, be shown that in practical cases the interference is roughly proportional to the number of blades and self-interference not so very much greater than that of any one other blade and not the greater part of the whole in a multi-bladed screw.

Further, interference in an airscrew differs from the induced velocity due to the trailing vortex system of an aerofoil (aspect ratio correction), because the vortices are coiled behind the screw in fairly close helices and the induced velocity is therefore greater.

This helical form of the vortices is a consequence of the helical path of the blade and but another aspect of the idea that the blades chase their own tails by virtue of rotation in addition to the forward motion—the idea represented by the development in an infinite cascade. The vortex theory thus confirms the existence of mutual interference between blades (in which we may include the influence of the blade on itself due to its helical path).

It is interesting to note that Glauert has reached the conclusion that the performance of any anular element of the airscrew disc is independent of the performance of the other anuli, which somewhat weakens resemblance between interference in an airscrew and aspect ratio effects on an aerofoil.

Mr. Kirdany said he was not very clear on one point. The lecturer showed an aerofoil with a pointed tail and the flow coming out tangentially to the upper and lower surfaces. He happened to be working on this problem and this tail gave him a lot of trouble and it was for this reason that he would like to know exactly what happened there. Is the pointed tail essential to the mathematical transformation or is the shape of tail immaterial? Also what would happen if the cyclicity put on was more than that necessary to drive the backward stagnation point to the tail? Would the latter go underneath the wing or what?

Mr. A. Fage said that one could not help being impressed by the extensive reading necessary for the preparation of a paper such as this and he would like to congratulate Major Low on his labours. He agreed that the development of airscrew theory on the lines suggested by Prandtl and Lanchester appeared to offer very promising results, especially if the realms of empiricism were left behind. Perhaps he would be excused if he confined his remarks to the work done at the N.P.L. as that was the work with which he was most familiar. On page 2 of the paper it was stated:—"Accordingly it has been proposed to take half the total increase in mean velocity as altering the true angle of incidence of the blade elements." Unless he was mistaken, he took it that the lecturer was there referring to an empirical theory which was a combination of momentum and aerofoil theories and which was published several years ago. That was based on a quasi-rational basis but had now definitely given way to the later theories mentioned by Major Low. The shortcomings of this theory were also demonstrated quite clearly by the latest work at the N.P.L., which was published a few months ago. This research endeavoured to estimate from comparisons of the pressure distribution over an airscrew blade and over aerofoils of appropriate shape the aerodynamic performance of an element as actually functioning in the blade, and also the type of flow which must be assumed to exist if these aerodynamic data were used in the calculation of the performance of the airscrew. The research showed that the performance of the blade element was in most cases appreciably different from that measured on the rectangular aerofoil of aspect ratio 6; the magnitudes of the translational and rotational inflow velocities which should be used with the aerodynamic data of the blade elements were also estimated. The last figure of that report showed that the translational inflow velocity which should be used with the aerodynamic data of the blade elements was different from that calculated by the Froude theory. He hoped that now that a lengthy investigation on a family of airscrews had been brought to a conclusion the opportunity would be taken to examine these results in the light of the theoretical work of Prandtl and Glauert. He would like to support the remarks of Mr. Wood that Mr. Glauert's theory was a brilliant piece of work and appeared to be a logical extension of the earlier work of Froude.

The Chairman said he would speak mainly as a critic of the Prandtl theory. First of all, he wished to say with the lecturer, that he was greatly impressed by the effect of the Prandtl theory in bringing experiments on aerofoils of a given aspect ratio into agreement. He was also impressed by the fact that on one of the slides shown by the lecturer the comparison between the calculations and the experiments of Betz on pressure distribution agreed to a degree not hitherto known. There was some little difficulty in that case, however, because the calculation was made on a Joukowsky aerofoil and it was essential to the success of the method that the aerofoil should have a sharp cusp, otherwise it would be found that instead of having a very small pressure at the trailing edge of the wing the value would again come out to $\rho v^2/2$. He did not think it mattered very much, however, because probably the area of intense pressure would be very small and would not affect the resistance very much, but so far as he could see, it would have to be there. The Prandtl theory did help to connect a great number of facts and, to his mind, was a very good empirical theory. But he would ask them to be chary of scrapping all their previous work and placing sole reliance on Prandtl's theory, because in his view it was not sufficiently well established. There was nothing wrong with the old idea of the Froude and Rankine theory or the Drzewiecki theory, except their degree of approximation. Whether the Prandtl theory went one stage further or not was a matter to be discussed, and it appeared that it had a chance of doing so; on the other hand, the physical fact, stated as a consequence of Newton's laws, that thrust is equal to momentum produced per second still remained and, before dismissing the inflow theory, one must get to know a good deal more about the Prandtl theory. It was rather surprising to

him to find that the lecturer had got through the whole of his lecture without mentioning a fundamental property of air on which its motion depends, viz., its viscosity, and that was not peculiar to the lecturer; it was the defect of the "circulation" theories of lift. In the Prandtl theory viscosity had no other place than to produce circulation and one of the slides shown by the lecturer referred to a cylinder; the spin on it made the illustration a little more real, but in plain language the Prandtl theory would mean that circulation round the cylinder produced lift without drag. That brought him to ask the questions: When should a circulation be applied and when should it not; how much should be applied and so on? The Joukowsky aerofoil had to have a pointed sharp tail in order to give a precise example since circulation applied to that aerofoil did not give infinite velocity at the trailing edge. On a cylinder any circulation would do, and if the trailing edge of a wing section were rounded, again there was indeterminateness. Major Low had called attention to certain remarks in his (the Chairman's) book and which he repeated in his lectures from time to time. He had pointed out in connection with the Lanchester theory of cyclic flow that none of these theories could predict stalling, which was one of the most important things in aeronautics. A Joukowsky aerofoil 90 degrees to the stream would not stall. The lecturer then says, "The criticism—that you cannot predict stalling—is true of all theories of moving fluids which are assumed to be physically homogeneous." He (the Chairman) wished to give that the direct denial. It just was not true. The great service of Stokes in relation to the problem of solving the equations of fluid motion was to remove it from the theories of molecular structure. Stokes knew that fluids were made up of molecules and that the movement of these molecules gave rise to viscosity. But this did not alter the fact that although they did not know what the molecules were doing, yet their effect, so far as resistance was concerned, was expressed by a coefficient of viscosity, and if they assumed that the material was homogeneous and viscous they could, from the equations of motions, deduce stalling. Of that he was convinced from experimental evidence. This brought him to his fundamental objection to the Prandtl theory. They could have various theories which were good or defective in various proportions, but ultimately if they were going to deal with a real physical problem they must come back as the basis to physical ideas. They had in the equations given by Stokes, and the txperiments of Poiseulle and Stanton, very strong experimental indication that these equations were sufficient to account for the phenomena, whether it was a steady flow or an eddying flow. These equations did not appear in the Prandtl theory. Other equations were given, but so far as he knew the literature of the subject nobody had attempted to show what relation these fundamental equations had to the viscous equations. It happened that at the present moment various people at the Imperial College were working on the solution of equations of viscous fluid motion and they naturally looked for the source of the circulation of which the Prandtl theory makes use, without finding it. In the solution of Stokes' equations it appeared there was no circulation, *i.e.*, the motion of a viscous fluid around a body moving in it was free from circulation. He knew of no natural mechanism that could produce circulation in a viscous fluid and that seemed to him to make a great difference to one's appreciation of the Prandtl theory. It was not, in the sense of, Stokes' equation, a fundamental theory, and the only justification which the lecturer had given was that the results fit experiment, *i.e.*, it was one degree of further empiricism. Moreover, Prandtl himself, it seemed to him, did not claim nearly as much for his theory as other people claimed for him. Prandtl himself realised, and he believed also that Lanchester realised, the very tentative nature of the proposals and he did not think any of them should run away with the idea that the Prandtl theory was the last word in either aerofoil theory or airscrew theory. We were just beginning to find out where it might fit, but the ultimate solution must be along other lines. We must start from the Stokes' equations of viscous fluid

motion and if the Prandtl theory was going to help it must show how its hypothesis of circulation had its equivalent in what we know of the real equations of viscous fluid motion. He would again like to say that he did not wish to be a harsh critic of the Prandtl theory; he only wished to come in at a meeting of this description and to say that the case is not yet overwhelmingly proved.

Mr. Zahra said he had a difficulty in explaining the cyclic theory physically. The mathematical explanation given by the lecturer appeared to be to subtract from the actual physical fluid motion the cyclic factor and that that subtraction would give the motion of an imperfect fluid, and that when that subtraction was added again to the imperfect fluid flowing past the cylinder, one obtained the flow of motion which would give the lift. As a matter of fact, when he added that subtracted factor into the imperfect fluid motion he did not get the physical flow at all, because from what had been said by Prof. Bairstow in one of his lectures, although the cylinder was put symmetrically and the flow past it originally was along the central line, the stagnation point started from the lower part and there was no similarity of flow between top and bottom of cylinder although gravity was not taken into account, and this showed that the theory when applied to a cylinder would not give the exact line or lines of flow which ought to appear experimentally. The other point he wished to mention was the 25 per cent. mistake between the experiment and the calculated value. It appeared that this always existed in spite of the fact that the strength of the cyclic current was so chosen as to fit the experiment.

Mr. Lock congratulated the lecturer on the excellent photographs he had shown of helical vortices coming off the blades of a screw under water. That was a very striking confirmation of the application of the Prandtl theory to the airscrew, and the lecturer had stated at the end of his paper that it helped to demolish the inflow correction. But Glauert in his application of Prandtl's theory to an airscrew had shown that the helical vortices coming off from the blades gave rise to an inflow and confirmed the inflow factor $\frac{1}{2}$ for aerofoil data reduced to infinite aspect ratio. It also confirmed the standard inflow theory except that the latter made use of data from aerofoils of aspect ratio 6 and so required a different inflow correction. Of course, Glauert's theory would have to be further confirmed by experiment, but it seemed that in any case it was a convenient theory for actual computation in connection with airscrews. The lecturer mentioned that Prof. Greenhill had produced a theory in which the rotational slipstream produced a thrust, and he would like to know how that was possible; how it could be reconciled with the idea that the thrust is equivalent to momentum in the slipstream.

Major Low said he would prefer to take time in replying to the discussion, but there were one or two points he could answer now. One was about the aspect ratio in an airscrew. He did not define it in the lecture and he had tried to make that clear in his comments during the reading of the paper. He had indicated calculations by means of Prandtl vortices given the strength of the vortex for calculating the real incidence of each blade element. The fundamental aspect ratio was infinite, as was the case all through the Prandtl theory; all the corrections were based on the infinite aspect ratio. With regard to the multiplane theory, he thought it was a perfectly sound and logical development of the Drzewiecki method, but it would have to be regarded finally in the light of the Prandtl theory. With regard to the experiments on high speed blades and the tip speed exceeding the speed of sound, he had there quoted a definite and interesting experiment and accepted the comment. With regard to Mr. Kirdany's question, the cusp at the tail of the wing profile was a mathematical condition in Joukowsky's theory. He did not suggest that it did anything more than give an approximation to the actual state of flow, and in the first place his position

was based, as the Chairman very justly remarked, on the agreement between the measurements and the experiments. With regard to the Chairman's remarks on the physical theory of fluids, he noticed that Mr. Lanchester, in one of his papers, had said that if we had waited for the mathematicians we would not have made any progress since the days of Noah, a somewhat one-sided statement. He would be glad to see adequate endowments provided so that pure scientists might have the leisure and atmosphere for pursuing purely and strictly abstract studies, for such studies were the real foundation of technical developments. But as an engineer he did not intend to wait for them on this occasion. He believed that that attitude really covered all the admirable remarks of the Chairman on the very difficult subject of hydrodynamical theory. He himself was not a physicist and it would be affectation on his part to argue with the Chairman on his special branch of physics. At the same time, he thought he had followed clearly enough the outline of the question of viscosity. Prandtl's assumption that viscosity sets up circulation, and that once it is set up the fluid could be regarded as acting as a perfect fluid, had the support of no less an authority than Mr. G. I. Taylor, and he might put that against the Chairman's statement. With regard to Mr. Glauert's theory, he had brought his paper rather rapidly to a close and did not make it quite clear that the Prandtl theory squared with the Drzewiecki stroboscopic experiment; that near the blades and near the cores of the vortices which were seen coming off from the tips of the blade there were very high local velocities. The effect of a vortex coming off one blade upon the other blade was relatively quite small. This agreed very substantially with Drzewiecki's actual stroboscopic experiments. On the other hand, Mr. Glauert assumed a very large number of blades and thus used a type of Froude actuator; only instead of the flow being axial it was actuated spirally through the blade disc; he thought this was a possible point of view, to go back to Froude and consider the mean increase of velocity. And by exactly the same reasoning as Froude's, they got half the final spiral velocity at the actuator disc,* and rotational inflow velocity would also come in as a correction. Taylor (R. and M. 765) had also pointed out that in Pannel and Jones' observations (R. and M. 371) there was no trace of the rotational inflow in front of the blade, and that conclusion again squared quantitatively with this mechanism of the central axial vortex and the spiral vortex coming from the blade tips and the circulation round the blades. If they estimated the rotational velocities in front of the blade, due to the circulation and vortex system, they would find it was very small. He was quite in agreement that Glauert's version of Prandtl's theory should be called Glauert's theory if the author really so desired. He himself thought it was merely a degenerate† form of Prandtl's theory, but if Mr. Glauert thought otherwise he was perfectly entitled to argue his view. As to the question with regard to Prof. Greenhill, he must refer them to Prof. Greenhill's own argument ("The Dynamics of Mechanical Flight," 1912, p. 102). He would reply in writing to such points as had not been dealt with.

On the motion of the Chairman a hearty vote of thanks was accorded Major Low at the conclusion of the discussion.

Major Low *(contributed)*: With regard to Mr. Lanchester's letter to the Secretary putting forward his later work as his most complete statement, I have read several times his two papers on the Aerofoil and Airscrew, and agree that they are a further development of his original theories, but as they are subsequent to much more systematic work in Germany, they can scarcely claim the same importance as his earlier work, which occupies a unique position in the development of what I have called the Lanchester-Prandtl theory.

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* Note.—It is necessary to distinguish between the expressions before, at, and behind the disc.

† Note.—In the sense that the trigonometrical functions are degenerate cases of elliptical functions.

Mr. Riach also has communicated his desire that his recent paper in the Journal should be considered as most representative of his views. He has there made full use of the ideas suggested by Drzewiecki's stroboscopic experiment, and as already stated above, this along with the "cascade" experiment may offer a useful check. But while logical and rational, I think it cannot be regarded as comparable with the circulation theory as a physical statement of what is actually taking place round the blade.

Fig. 14a is added as a better representation of flow at small incidence.

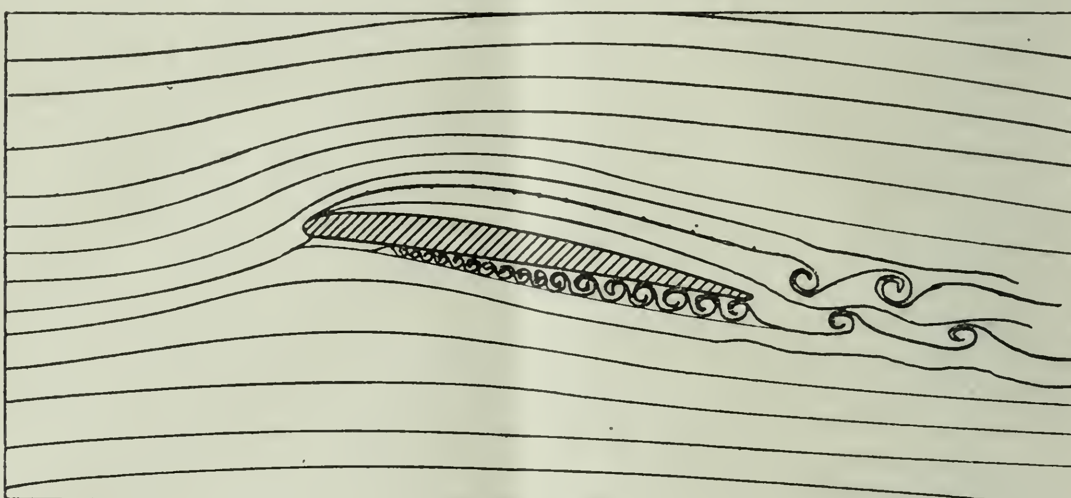


FIG: 14a

The Helmholtz sheet at the upper surface is prevented, partially at least, from breaking up by its image in the surface. It curls in on itself, thus approximating visibly to the assumed Joukowsky flow. At higher incidence it reverts to the Helmholtz flow, modified as in Fig. 14.

Since the meeting the writer has received a paper by Betz with a note by Prandtl, on the application of the vortex theory of wing lift to airscrews.*

This is referred to by Mr. Glauert, R. and M. 786, App. IV., and contains a discussion of the simplified form of the theory ascribed to him. Assuming, as in this simplified form, that there is an indefinitely large number of blades of indefinitely small section, the airscrew is reduced to Froude's ideal actuator disc as far as axial components of flow are concerned. Prandtl's remark that an inflow correction of one half the final increase in velocity must be applied at the disc is in accordance with the assumptions made. It is of course implied, if not explicitly stated, that the uncorrected coefficients are those of profiles with infinite aspect ratio. With regard to the rotary component of flow Prandtl makes the analogous assumption that at the disc a correction must be made of half the final rotary velocity. The disc is a surface of discontinuity of pressures for axial flow, but of velocities for rotary flow. It is in fact a vortex sheet with vortex filaments directed radially, and there is no rotary flow before the surface, while behind the full rotary velocity is at once set up.

To give the expression "at the disc" a physical meaning, the disc may be supposed to have a small finite thickness, and the vorticity not to vary in passing through the disc at right angles to the surface. In accordance with the electromagnetic analogy, the rotary component at mid-thickness will have one half the final value, and this is the meaning to be given to Prandtl's rule. This rotation is in the same sense as that of the screw, therefore it is to be taken as subtractive.

* Nachrichten v. d. K. Gesell. d. Wissenchaften zu Goettingen, Math-Phys. Kl., 1919, Heft 2, p. 193, "Schraubenoropeller mit geringsten Energieverluste."

In the more practically important case of airscrews of two, three or four blades, the writer's exposition of the consequences of applying the wing theory to airscrews is confirmed. The integrals in the appendix and the Fig. 17 of this paper are identical, *mutatis mutandis*, with integrals 9, 10 and 11 and Fig. 4 of Betz's paper.

It is at once disappointing to find one's work anticipated, and satisfactory to find it in agreement with the work of a recognised authority.

The contributed remarks of Mr. McKinnon Wood, Mr. Glauert and Mr. Lanchester will require careful consideration before reply. But I must refer Mr. Lanchester to p. 102 of Sir George Greenhill's "Dynamics of Mechanical Flight" for the very special condition under which he obtains the result questioned. Perhaps too much importance is given to this case, but it has been quoted by an American writer and this led to its inclusion in the references.

Mr. H. GLAUERT (*contributed*): I should like to emphasise one aspect of airscrew theory, namely, the distinction which must be made between the actual flow through the disc of the airscrew and the interference flow experienced by the blade elements. This distinction was pointed out in the multiplane interference theory and is also an essential feature of my more recent theory based on Prandtl's vortex theory of aerofoils. The actual disturbance consists of two parts, the first being due to the circulation round the blade element under consideration and the second due to the remainder of the airscrew. Only the second part acts as an interference on the blade element under consideration.

This view of the nature of the interference experienced by the blade elements leads me to differ from Major Low on certain points. I agree, of course, that the axial flow through the disc of the airscrew is strongly periodic, as shown by the stroboscopic measurements. On the other hand, it appears to me that this periodicity is due mainly to the circulation round the blade elements, and this part of the disturbance must be subtracted from the actual flow to obtain a measure of the interference flow experienced by the blade elements. The axial interference flow is the velocity field of the trailing vortices which constitute the slipstream, and since a helical sheet of vortices springs from each blade of the airscrew it is clear that the interference flow will also have a periodic character. This periodicity will, however, be less strongly marked than that of the actual flow shown by the stroboscopic experiments, and I am satisfied that it is sufficiently accurate to take a mean value in estimating the interference effect. By this method the old "inflow" factor $\frac{1}{2}$ is restored to its position and, when used in conjunction with aerofoil data for infinite aspect ratio, leads to very satisfactory agreement between calculated and observed airscrew characteristics.

Mr. F. W. LANCHESTER, M.Inst.C.E. (*contributed*): Major Low in his paper has made very generous reference to my own work in connection with the screw propeller and has given me an opening for adding a few remarks to what he has himself said. There are two points of view from which to regard the theory of any mechanical device. The one is to consider the theoretical work as an aid to the practical man in the development and perfection of his mechanism; the other is to treat the question as the more academic problem of solving a mathematical or quasi-mathematical puzzle. My own efforts have been directed to the former rather than to the latter. I note that Major Low complains of the absence of any account in my work as to the manner in which I arrived at my conclusions, and also of the absence of a reference to the work of others "from whom he may be supposed to have drawn suggestions or experimental evidence." With regard to the first of these I do not think he can have read my "Aerodynamics" seriously or he would realise that what I did in that work was a perfectly logical investigation which led step by step to formulated conclusions. While it is perfectly true that my own work lacked the precision of a real mathematical or Euclidical demonstration, there were no steps in the argument omitted and no results given which were not supported by the argument and evidence presented. There are some

39 pages constituting this investigation forming the first part of Chapter IX. of my "Aerodynamics" devoted entirely to propulsion and the theory and design of the screw propeller. The work of Rankine and Froude is discussed at some length under the title of the Newtonian Method. Then there are some six pages constituting a close investigation on the question of efficiency. Following this there is a discussion concerning the proper distribution of the load radially on the propeller disc area and of the number of blades permissible, and on interference, leading to some suggestive work on the subject of actual propeller design and the application to a hypothetical case of an aeronautical propeller. I think I am in a position to refute any statement to the effect that there is an absence of any account as to the "manner in which Lanchester arrived at his conclusions." Beyond the above Major Low omits even to mention two later papers which I have published on propulsion and the screw propeller, namely, "A Contribution to the Theory of Propulsion and the Screw Propeller," read before the Institution of Naval Architects in 1915, and a paper read a few weeks later on "The Screw Propeller," before the Institution of Automobile Engineers. The latter required to be read in conjunction with a paper read a month previously before the same Institution on "The Aerofoil." I will not attempt to abstract either of these papers here, but without undue egoism I do not think it can be pleaded that a review of airscrew theory is complete without some mention of this work.

When Major Low complains that I have not made due reference to the work of others, I must refer him to the very extensive reference to Rankine and Froude above cited. At the same time I confess ignorance of the work of Drzewiecki. In self-defence I would point out that, as stated in my "Aerodynamics," a large amount of the investigation then published (1907) actually dated from ten or twelve years earlier; a great deal of the subject matter of my investigation *re* the aerofoil and reference to its application to the screw propeller were included in a paper which I offered to the Physical Society of London in 1897. The fact that this paper was not accepted was not my fault—I did my best and a great deal of the matter in this paper has since (as Major Low says) received its principal recognition in Germany. Perhaps I ought to have sent the paper in question to a German scientific society, but I was a young man at the time and did not realise then how true it is that an Englishman has usually to go for recognition to some other country than his own. With regard to Mr. Drzewiecki's work, when I first heard of this, several years after the publication of my "Aerodynamics," I tried from more than one bookseller, also by writing direct to various addresses abroad, to obtain a copy or some particulars of the work in question and I failed. The only information I have now in my possession was furnished me by the Air Ministry three or four years ago.

As far as I can make out there is a decided difference between Mr. Drzewiecki's ideas and work and my own and behind this difference of ideas and method I believe there is precisely the difference of attitude of mind to which I have already referred. While Drzewiecki and myself are at one in dealing with the concentric annular elements of the propeller race as analogous to vertical strata of air in the case of the aerofoil, I believe I am right in saying that I differ from him in permitting or recognising the necessary inter-action between the fluid in the different annular elements in the case of the screw propeller, or the different vertical strata in the case of the aerofoil, by assigning properties to the blade depending upon its aspect ratio. In other words, the behaviour of the given element of a blade, and the value of that element as a unit of propulsion, can, according to my method, only be assessed when the aspect ratio of the whole blade is known and taken into account. The point is a rather subtle one, but in my opinion is of no little importance. Whatever the similarity or dissimilarity of my own methods and conclusions and those of Drzewiecki may be, I do not think I can be seriously blamed for having failed to make reference to work

with which I had no means of becoming acquainted and which, when I came to know of the existence, I found it so difficult to obtain information.

I have one further remark to make on this subject. There was a paper published by someone who shall be nameless, who described the method of designing airscrews in vogue at the Royal Aircraft Factory and attributed it, doubtless in good faith, to Drzewiecki. Now in this paper and the description of the method certain terms were used, such as *load grading*, *linear grading* and *thrust grading*, terms which I myself coined to deal with my own method. They are there as the headings of articles § 208, § 209 and § 213 of my "Aerodynamics." I am quite sure that these titles came from my own book; I will leave others to judge whether the method described by means of these terms was inspired from any other source. As I have said on a previous occasion, a man who steals another man's gold should take the elementary precaution of throwing away the purse. I have defended myself at some length because I have been attacked. I could say more but I have to remember the ironical notice that was said to have adorned the cage of an animal of long-suffering disposition : "Cet animal est très méchant, quand on l'attaque il se défend."

Referring now to the two papers which I have mentioned as having been lost sight of by Major Low. In my paper read before the Institution of Naval Architects I dealt with the Froude theory of propulsion to which Major Low makes reference in the early part of his paper. Parenthetically, I would point out that in dealing with the Froude conception of an "actuator" we are not dealing with the screw propeller at all; that the ultimate intention is to make use of the theory in connection with the screw propeller is fairly obvious, but the fact that the theory is not directly concerned with the screw propeller should be always borne in mind and made clear. Before the date of my own paper there had been certain doubts about the arguments presented in connection with the Froude actuator which I endeavoured, with some measure of success, to clear up. The theory was also extended to deal with possible losses of energy not contemplated in the regime but which certain writers believed might exist. At the conclusion of this paper examples were given of the application of the theory to the stationary screw propeller, the helicopter and the windmill. As is often the case, not the least important part of this paper is to be found in the discussion in which I had to reply to criticisms by Mr. R. E. Froude, the Hon. Sir Charles Parsons, Mr. Sydney W. Barnaby, Professor J. B. Henderson and others. I think if Major Low studies this paper carefully, and especially the discussion, he will have considerable doubts as to whether, as he states, Professor Henderson had the better of the theoretical discussion when he ventured to cross swords with Mr. R. E. Froude. My own conclusion, supported by careful argument, was that Professor Henderson failed to establish his objections.

Passing now to my paper of April, 1915 (Proceedings Inst. Automobile Engineers, Vol. IX., page 263), a rather intricate theoretical investigation is presented, the main results of which are summarised in graphic form, Fig. 23 opposite page 310. This diagram gives the means of calculating the optimum condition of a propeller for any given duty. This optimum condition not only requires a certain diameter but a certain pitch which, under the conditions assumed, gives pitch = 1.46 times the diameter. Further this diagram gives the solution of the best pitch to adopt to secure maximum efficiency for any restricted diameter where circumstances do not allow of the optimum condition being adopted. There is also a curve deduced giving the theoretical efficiency for any case chosen between the limits to which the diagram applies and beyond this there is further given the theoretically best number or permissible number of blades which may be employed without loss of efficiency. The above data, as given in this diagram, furnish very nearly all the information that a propeller designer requires and, where comparisons are possible, they do agree very closely with what experience has shown to be the

best. Now this agreement would not be any great achievement if it had been based on empirical or experimental determinations—to all intents and purposes, however, it is based on abstract theory.

It is quite true that the investigation in question does not determine the shape of the blade, but the rules of the determination of the shape of the blade are given in Chapter IX. of my "Aerodynamics," to which reference has already been made. Beyond this, although I do not subscribe to the view that some rash rule-of-thumb workers have expressed, namely, that a twisted bit of hoop iron is as good as any other screw propeller, it is certainly true that if the diameter and pitch are chosen as the best possible for any given service, any good-looking blade is very little worse than the best. The same fact applies to the aerofoil, if we are dealing with flight efficiency alone and not other considerations (such as slow alighting speed) if the span be properly chosen for any given aspect ratio and flight speed, the best and the worst produced by any experienced designer are very little different in performance. In my opinion *the only factor in the form of blade section* which really counts is the mean curvature, the assumption being that the mean curvature is "faired" by eye to a good streamline form. It is this mean curvature which is dwelt on as the factor of importance in any aerofoil section, both in my "Aerodynamics" and in my paper on the aerofoil read before the Institution of Automobile Engineers in March, 1915 (Proceedings, Institution of Automobile Engineers, Vol. IX., page 171). The practical limitation in the screw propeller is the need for strength of blade, and thus it is not possible in a marine propeller blade to adopt so high an aspect ratio as in an air propeller; in the marine propeller a high aspect ratio blade will carry too great a thickness of body to give streamline flow. It is in this that the designer of an air propeller has the advantage.

I note that Major Low quotes Sir George Greenhill as having showed that if rotational velocity only be imparted by the blades to the column of fluid, this alone will account for the axial thrust on the screw. This is a result that ought not to pass unchallenged. If we are dealing with the regime contemplated by Rankine and Froude it is a patent absurdity; if on the other hand it merely means that we deal with the whole volume of fluid and not merely upon the propeller race then it is quite irrelevant to the investigation. It is equally irrelevant if the meaning is that the backward momentum communicated by the screw is equal and opposite to the forward momentum communicated to the water by the ship or vessel because the combination of vessel and propeller is not under discussion.

Mr. A. S. D. Poncox (*contributed*): Major Low asked me to prepare the diagram of Fig. 7 for his paper and showed me the method of Fig. 6. I have no equipment for reading a book like Lamb's Hydrodynamics, but have no difficulty in seeing the graphical method as an application of the triangle of velocities, and the drawing of the curves in Fig. 7 was carried out by myself after a few minutes discussion of the method. It seems to me that circulation is very difficult to understand as a mathematical expression, but graphically very easy to see as the flow round the body, got by subtracting graphically the flow without lift from the flow with lift. There are always two diagonals that may be joined in each small parallelogram formed by the two sets of lines crossing; one gives the result of adding, the other the result of subtracting. If there is a circulation component of the flow, the circulation curves will be closed and will go round the body. With this rule it is easy to choose the right diagonals to join up in getting the new curves.

Major Low (*reply to written contributions*): With regard to Mr. Lanchester's protest, the supposition about failure to give reasons and references was confined to the circulation theory and was an attempt to explain the inexplicable neglect

of what the writer considers the most far-reaching advance in hydrodynamical theory since Helmholz's vortex theory. The writer cannot understand how he failed to see at once, some fourteen years ago now, that the key to the fascinating problem of flight had been found, save on the ground that it was not yet capable of yielding numerical values, which were demanded insistently by designers who had to get aeroplanes to fly. The application of the Biot-Savart law in electro-magnetic theory to the analogous problem in hydrodynamics has made Lanchester's method a numerical one, leaving only the circulation to be determined by experiment. This missing link in Lanchester's chain of reasoning has been filled in by the labours of the Prandtl school.

In the development of the blade element theory, Mr. Lanchester's independence of Drzewiecki is patent, his analysis of the problem is in many ways more complete and satisfactory, but here again there is a gap which the latter filled by experimental determination of his mean blade coefficients, while Lanchester claims to have obtained his more general coefficients by "good straight thinking," in Colonel O'Gorman's phrase, in which claim the writer is not yet prepared to follow him.

With regard to the Froude-Henderson controversy, it seems more natural to consider the wake in a finite basin as gradually opening out again and returning the energy of flow to the front of the actuator, less the amount dissipated by friction, than to maintain the column with unaltered diameter up to infinity, and there put it down a sink of unfillable capacity. The Goettingen type of tunnel was probably designed to conserve energy. In a dock the return flow is visible. In the N.P.L. type the conservation of energy is understood to be negligible. So that in the North Atlantic dissipation of energy must be very complete. Thus, Froude's assumption leads by an artificial line to the correct result, while Henderson's conclusion from a more natural line of thought is negatived by the rapid dissipation of energy.

Proceeding with the circulation theory, Mr. Glauert and Mr. McKinnon Wood seem to take as the fundamental case the limiting case of a disc covered with radial lines of vorticity, together the system of trailing vortex lines covering the surface of a cylindrical column of outflow uniformly. To this must be added the central axial core of vorticity. Prandtl, in the paper referred to, points out the need for adding half the axial inflow and subtracting half the rotary inflow, and as all corrections are applied to coefficients for infinite aspect ratio, there seems little left for Mr. Glauert to claim.

From the ideal limiting case, an attempt is made to argue back to the more practically important cases of screws with two, three or four blades. This seems to the writer to suffer from exactly the same defects as the attempt to argue from the Froude actuator disc back to bladed screws. The most direct way is obviously to calculate the effect of the open spiral of vortex lines trailing from the tips in conjunction with the complementary central core of vorticity and the circulation round the blades by evaluating the integrals in the appendix.

Self-interference is defined as the effect on a blade of the vortices streaming from its own tip and root, mutual interference as the effect on a blade of the circulation and vortex systems of the other blades, on the analogy of self and mutual induction of circuits in electro-magnetic theory.

For two blades the mutual interference is a small part of the total interference, for three blades not so small, for four blades still larger, and finally for the limiting case self-interference is negligible, and the ideal correction factor of one half the inflow component of velocity, additive for axial flow and subtractive for rotary flow, is reached, but it is to be applied to the coefficients of each element for infinite aspect ratio.

Thus, the difference of view is seen to be largely apparent and is completely reconciled by the correct application of the general theory.

Mr. Fage remarks that the reports criticised have been since superseded. This is to some extent inevitable as one can only criticise what has been openly published. It is satisfactory to know that the high standard of accuracy of measurement established at the N.P.L. is to be applied to the experimental examination of the circulation theory.

Mr. Lock's support of the general theory is most satisfactory, his criticism of detail being largely met by the reply to Mr. McKinnon Wood. The writer has qualified as "brilliant" the whole original work of a galaxy of able writers during a period of twenty years, Mr. Lock an expository pamphlet of twenty pages. Surely there is some disproportion in values.

Professor Bairstow approaches the subject from the consideration of Stoke's general equation of viscous flow. Apparently these equations have not yet yielded in his hands a demonstration of the existence of circulation, and until they do so he is cautious about accepting mere experimental evidence. The analogy he draws with elastic theory in a special problem in which the cyclic stress terms vanish, is weakened by the theory of dislocations of which a summary will be found in last year's B.A. reports. His objection to molecular theory should likewise be modified by the fact that apparent anomalies in the elastic behaviour of materials were recently explained by Professor Jenkin to this Society by means of a molecular model.

If Stoke's equations prove sufficient to account for the formation of eddies at solid boundaries then the writer's remarks will have to be modified. If not, then further coefficients will be required and molecular theory may be called in to explain their nature.

The writer is preparing for the Journal a brief account of the Kutta-Joukowsky transformations which will meet certain questions raised by Mr. Kirdany, by Mr. Zahra, and by Professor Bairstow himself. There is always a characteristic point at the tail which may be selected as point of stagnation even when the tail is rounded. The assumption is in the first place purely arbitrary, but Prandtl has offered a working hypothesis to justify it, and the close agreement in form with the observed distribution of velocity, gives it great interest and importance.

From elementary geometrical considerations it may be shown that the flow due to circulation alone is $K/2\pi a$, and that the flow due to the flow without circulation is $-2U \sin \alpha$, at any point on the circumference, α being the angle between the radius on which the point lies, and the velocity U at infinity.

Hence, having determined U and having assumed α arbitrarily, the value of the circulation is given by the relation $K/2\pi a = 2U \sin \alpha$.

In Fig. 7 α is about 60°, a value much beyond stalling point, and $K/2\pi a = \sqrt{3}\,U$. With a greater value than $K = 2\pi a U$ the stagnation point must be at a distance from the circle and from the profile, as is possible with a golf ball spinning fast enough, but not with an aerofoil.

With regard to the prediction of the stalling incidence, the writer has suggested a point of view which makes the stalling depend on the breakdown of the equilibrium of a Helmholz sheet of separation regarded as a vortex sheet and which may lead in the direction of a numerical determination. ("Engineering," 15th Dec., 1922, p. 739.)

In conclusion the criticisms called forth have been of a most stimulating nature and have greatly enhanced the value of anything that has been done in the paper itself to bring the Lanchester-Prandtl theory to the consideration of this Society and through it of the English-speaking aeronautical world.

AN ACOUSTIC TENSION METER

BY A. H. STUART, B.SC.

The demand for an instrument capable of measuring the load carried by a bracing wire has resulted in the designing of a number of tension meters. The majority of existing instruments make contact with the wire at three points and the force required to displace the middle point a definite amount is determined. Under favourable conditions the tension in the wire is a function of this force.

The chief objection to this class of instrument is that the application of the meter causes a change in length of the wire, and if the bracing wire be a short one, this change in length materially affects the tension. Much difficulty is also experienced with stiff wires.

Judgment of the tension in a wire by touch is very unreliable, and in bracing a structure without instrumental aid there is a very marked tendency to place an excessive stress on the bracing wires. There are many cases on record where the compression ribs of an aeroplane wing have failed during flight through a too great initial stress being put on the bracing wires.

The tension meter described below has enabled the writer to demonstrate this tendency to overstressing in many instances. The instrument has been used with marked success for checking the tensions in the internal bracing wires of aeroplane wings and fuselages. It has also been of much service in experimental work on a laboratory model of a Warren girder and similar structures.

The instrument depends upon the principle that the frequency of the note emitted by a wire is a function of (among other things) its tension. It consists essentially of two bridges, the distance between which can be varied from 20cms. to 40cms. These are placed in contact with the wire under test and the distance adjusted until the note emitted when the wire is plucked between the bridges is in unison with that given by a standard steel reed attached to the instrument. A suitable frequency for this reed is 276 vibrations per second; this is the "middle C" of modern concert pitch. A second reed of frequency 390 vibrations per second was added, however, to increase the range of the instrument.

Fig. 1 shows the general arrangement of the instrument. It is convenient to have a sound box between the bridges, and this may be connected by means of a rubber to some ear attachment such as a stethoscope for use when there is much external noise, as in a workshop.

The calibration of this instrument must be made experimentally. The well-known relation between the frequency n and the tension T expressed by

$$n = (1/2l)\sqrt{(T/m)}$$

is only true for what is generally called a "stretched string," that is, one in which the product of the modulus of elasticity of the material and the moment of inertia of the section is negligibly small. As this product is far from negligible in the wires used for bracing and the actual relation between n and T for stiff wires is unusually complicated, the experimental method of calibration is the only satisfactory procedure.

Figs. 2, 3 and 4 show the calibration graphs for steel wires known as "piano wire" of S.W.G. Nos. 20, 17 and 11 respectively. The following table gives particulars of these wires :—

S.W.G.	Diam. in ins.	Sectional area in sq. ins.	Weight per 100yds. in lbs.
20	0.036	0.00102	1.06
17	0.056	0.00246	2.54
11	0.116	0.0106	10.4

Fig. 5 shows the calibration graph for $\frac{1}{4}$in. B.S.F. streamline wire. This is a much heavier type of wire. It is swaged to a section of more or less streamline form and is equivalent to rod of 0.2in. diameter and weighs $32\frac{1}{2}$ lbs. per 100 yards.

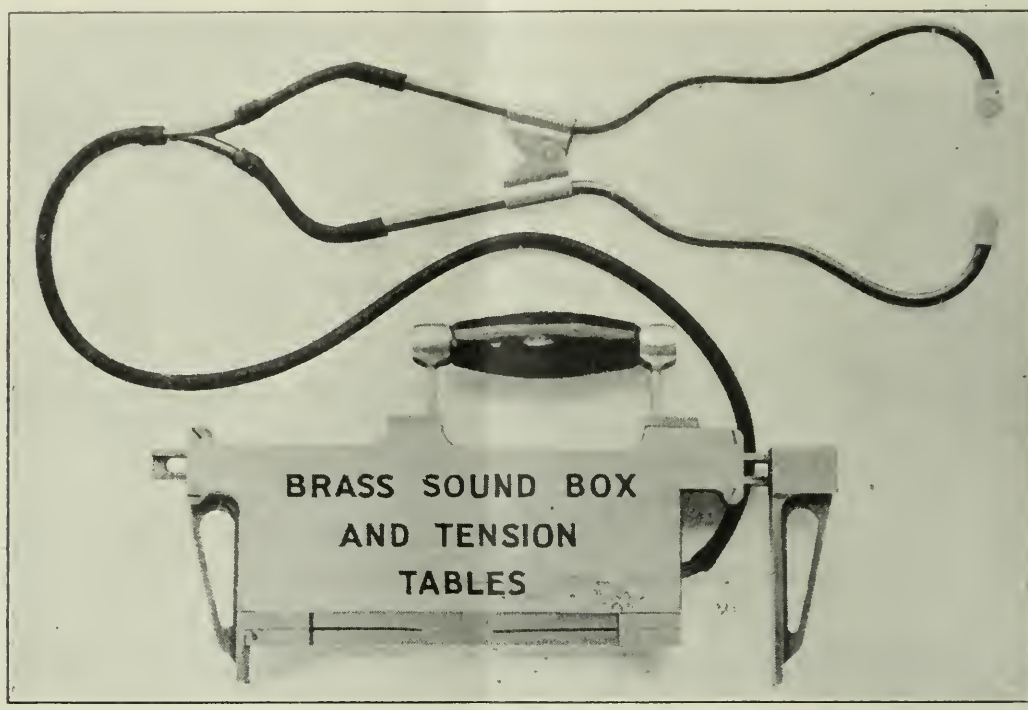


FIG. 1.—*Brass Sound Box and Tension Tables.*

An examination of these graphs shows that the readings are very consistent. It will be observed that some of the graphs are straight lines, while others are curves. It can be shown, however, that the calibration graph for any steel

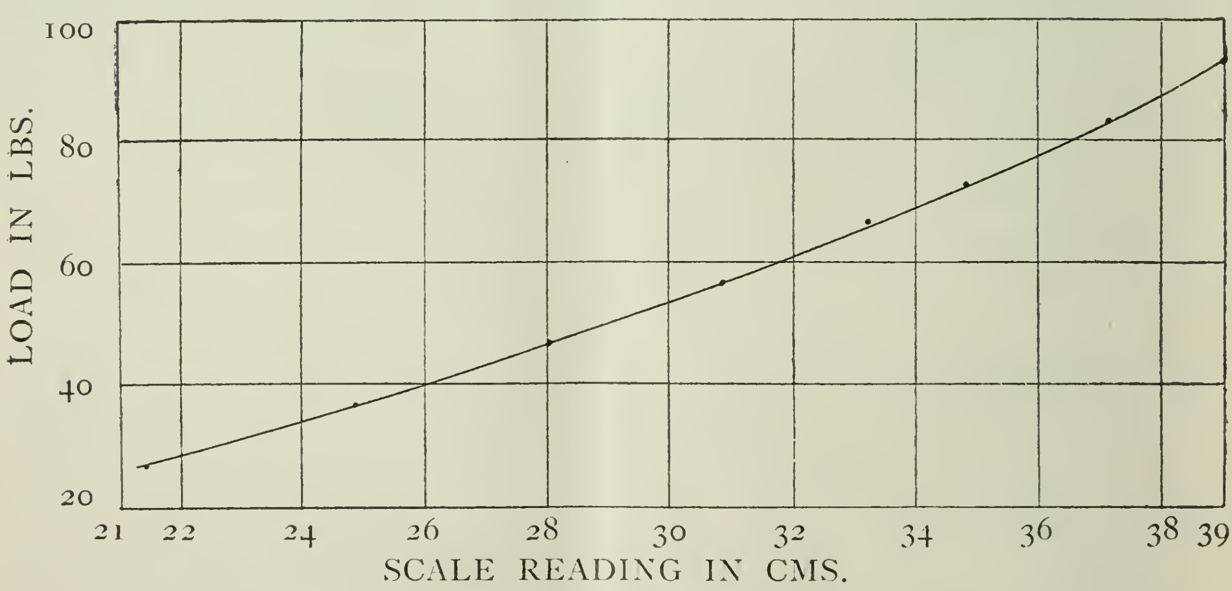


FIG. 2.—*Calibration Graph for No. 20 Piano Wire* $N=390$.

wire does not sensibly deviate from a straight line so long as the stress does not exceed 20 tons per square inch.

Figs. 3 and 4 show the advantage derived from the addition of a second reed to the instrument. When the limit of the instrument is reached on the

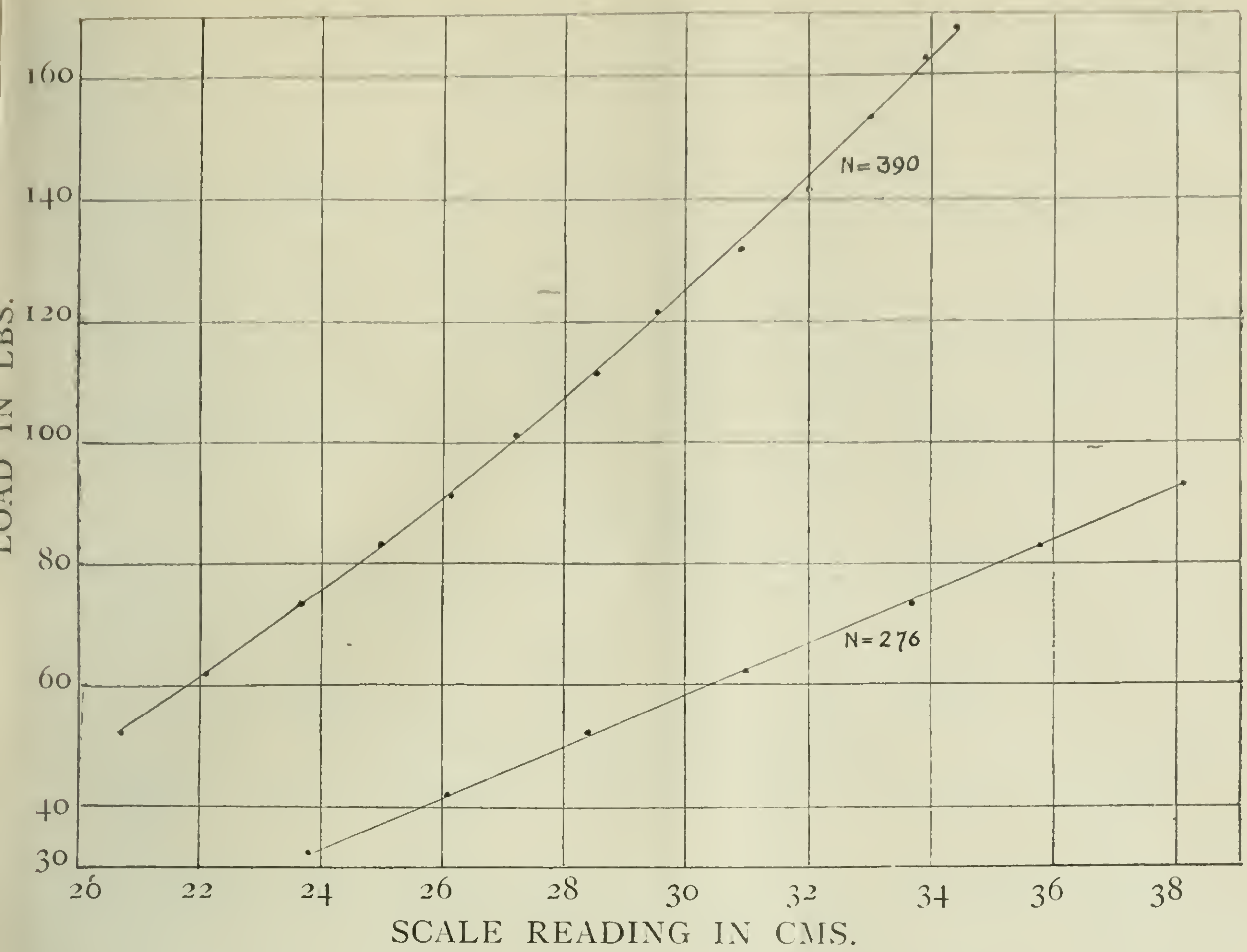


FIG. 3.—*Calibration Graph for No. 17 Piano Wire.*

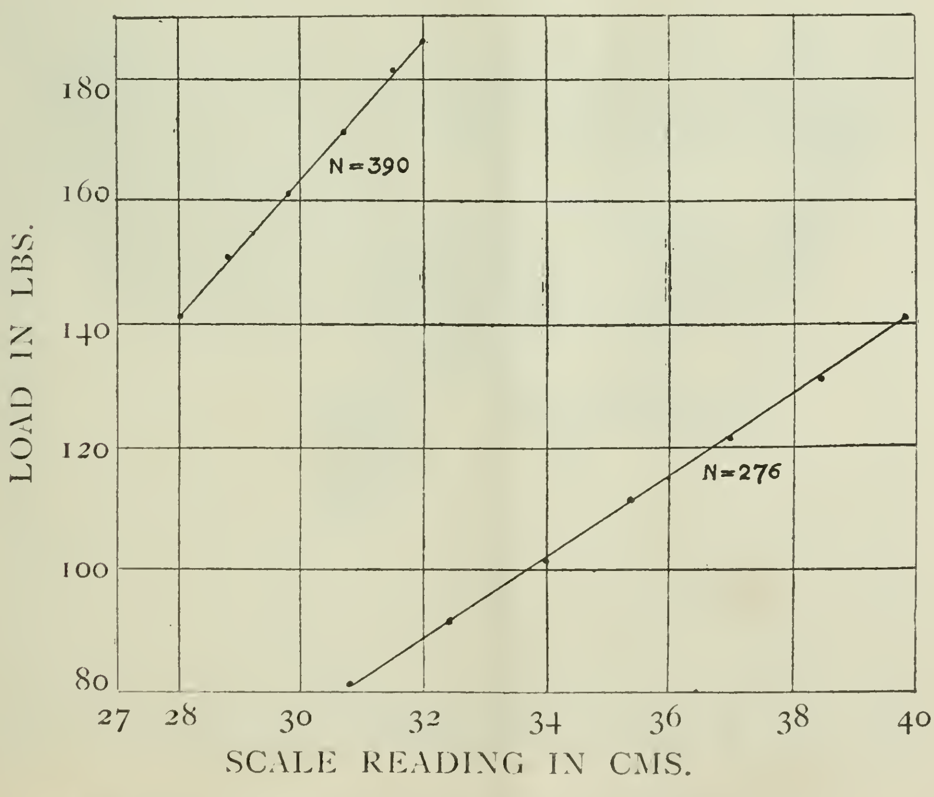


FIG. 4.—*Calibration Graph for No. 11 Piano Wire.*

reed of lower frequency, that of the higher frequency may be used. This makes the instrument available for loads approximately double those which may be determined by means of the reed of lower frequency.

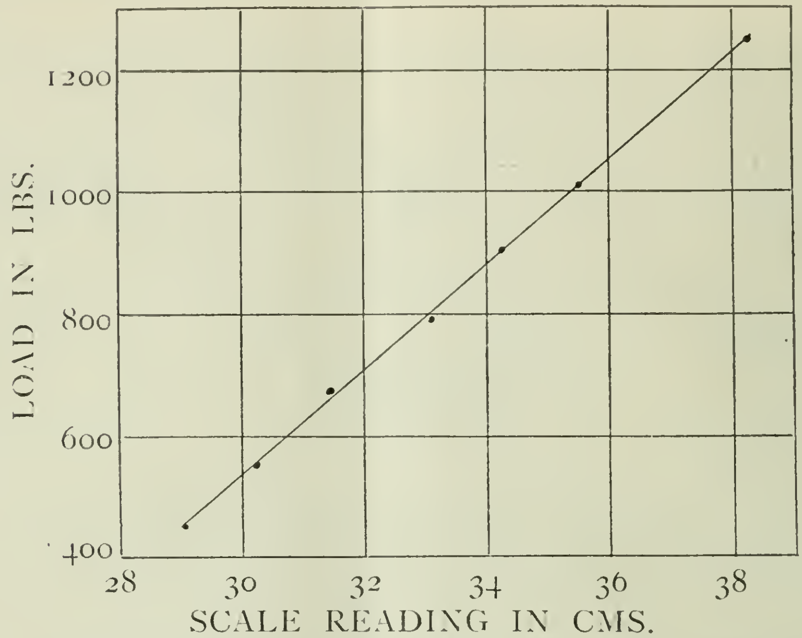


FIG. 5.—*Calibration Graph of $\frac{1}{4}$ B.S.F. Streamline Wire.* $N=276$.

No particular skill or practice is required to use the instrument on wire of gauge up to S.W.G. No. 10. Generally speaking, and within reasonable limits, the thinner the wire and the higher the load, the more easily is the reading obtained. Nevertheless, the instrument *may* be applied to heavy wires under moderate stress, as Fig. 5 shows.

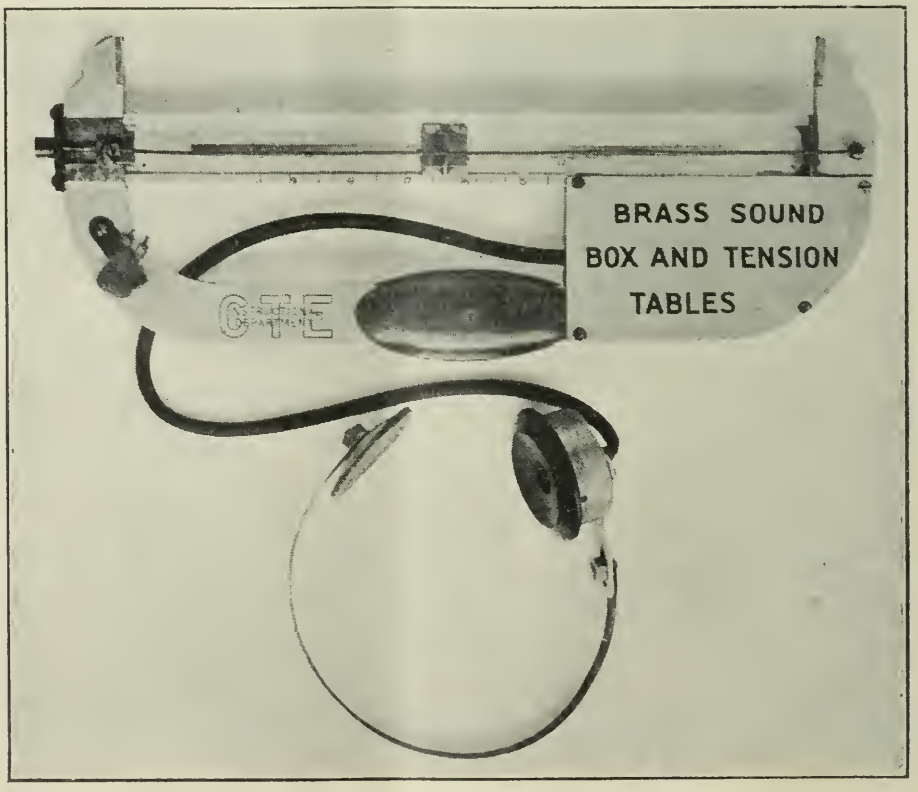


FIG. 6.—*Brass Sound Box and Tension Tables.*

The suitability of the instrument for general use was demonstrated in the following manner :—A length of No. 20 S.W.G. wire was loaded with 57 lbs. A section of eighteen boys of average age just over 16 made the test individually without any consultation or supervision whatever. The scale reading (which is all they were asked to find) for this load should be 30.8cms. (see Fig. 1). Of the eighteen readings obtained, twelve were excellent, and the average for the eighteen was 30.65cms. None of these boys had handled the instrument before, nor had they done any experimental work in sound.

An indication of the suitability of the instrument for experimental work is afforded by the fact that it was used exclusively for obtaining the data discussed in "The Internal Bracing of Aeroplane Wings" published in "Engineering" for August 26th, 1921.

Appendix.

The following tables give the actual experimental results from which the calibration graphs were drawn.

No. 20 S.W.G.
N = 390

Load in lbs.	Scale Reading.
27	21.4
37	24.8
47	28.0
57	30.8
67	33.2
73¼	34.8
83¼	37.1
93¼	39.0

No. 11 S.W.G.
N = 276

Load in lbs.	Scale Reading.
81¼	30.8
91¼	32.4
101¼	34.0
111¼	35.4
121¼	37.0
131¼	38.5
141¼	39.9

N = 390

Load in lbs.	Scale Reading.
141¼	28.0
151¼	28.8
161¼	29.8
171¼	30.7
181¼	31.5
186¼	32.0

No. 17 S.W.G.
N = 276

Load in lbs.	Scale Reading.
32	23.8
42	26.1
52	28.4
62	31.0
73¼	33.7
83¼	35.8
93¼	38.1

N = 390

Load in lbs.	Scale Reading.
52	20.7
62	22.1
73¼	23.6
83¼	25.0
91¼	26.1
101¼	27.2
111¼	28.5
121¼	29.5
131¼	30.9
141¼	32.0
153¼	33.0
163¼	33.9
168¼	34.4

¼in. B.S.F. Streamline.
N = 276

Load in lbs.	Scale Reading.
450	29.0
550	30.2
675	31.4
790	33.1
900	34.2
1010	35.5
1245	38.25

It may be of interest to add that before this instrument was designed, one of the form shown in Fig. 6 was made. In this example the distance between the bridges was fixed (12ins.) and the frequency of the comparison note was varied.

The latter variation was accomplished by providing a suitable "loaded" wire, the tension of which was adjusted until six inches of it gave a note of frequency 276. A bridge moving under this wire and in contact with a scale was the means provided for adjusting the frequency of the comparison note until it was in unison with that emitted by the wire under test.

This instrument was very satisfactory in use, its only disadvantage being that it required "tuning up" before a test was commenced and it was necessary to check this adjustment at intervals during the test. This disadvantage was felt to be very grave and the instrument was discarded in spite of the simplicity of its construction.

REVIEWS

Steel Thermal Treatment

By John W. Urquhart. Publishers: Crosby, Lockwood and Son. Price 35s. net.

This book is an unsatisfactory book from many points of view and is not to be recommended. Data has been gathered from various sources and used with little discrimination, with the result that it contains information of a misleading kind and in part is self-contradictory. As an instance, in the early definitions, annealing and normalising are taken as one operation, but are later shown to be quite distinct treatments. A number of definitions might be severely criticised. Some of the furnace descriptions are good, but the matter relative to the treatment of steel gives such data as, for instance, that the main changes in a 1 per cent. carbon steel were at 1,450°F. and 760°C. on heating and cooling respectively, which is quite inaccurate. The compilation of physical tests is weak and unsatisfactory.

Aircraft Steels and Material

By Brig.-Gen. R. K. Bagnall-Wild, C.M.G., C.B.E., Leslie Aitchison, D.Met., B.Sc., A.I.C., A. A. Remington, O.B.E., M.I.Mech.E., M.I.A.E., A. J. Rowledge, W. A. Thain, A.M.Inst.C.E. With an introduction by Professor W. Ripper, C.H., D.Eng., D.Sc., M.Inst.C.E. Publishers: Constable and Co., Ltd. Price 16s.

This book, embodying the lectures delivered on behalf of the Society at Sheffield University in 1921, is a useful volume to have on one's shelf as indicating the standard of technique of aircraft steels during the war. To the engineer it will be found to contain much information and data which will teach him the necessity for thoroughly understanding the characteristics of the steels which he employs.

Certain matters touched upon in the book are still of a controversial nature and most of the conclusions drawn can by no means be considered as final. One may mention, for example, the reference to hot twisted crankshafts on page 25. One would conclude from the figures here given that twisting was a most damaging process, whilst those familiar with it and with the test results will know that the figures given are misleading and unrepresentative, and hence the conclusions are not satisfactory. Hot twisting was largely used for the crankshafts during the war and is still used very successfully.

The lecture on "Cold Worked Steels" contains a number of anomalous statements, for instance, "Theoretically, it should be possible to obtain any tensile strength in a steel which has been cold worked." What "theory" is here invoked it will be interesting to know. In comparing soft and hard steels for cold working capacity it is stated that "the ratio of original and final strength is constant just as the proportion of cold working is constant." Those familiar with tests on such material know that mild steel is capable of much more deformation as well as of greater proportionate increase in tensile strength than some harder varieties, although perhaps the final strength may be somewhat inferior. The "variation in modulus figures" obtained is not really large enough to be of practical importance. Moreover, the low figures given for untempered cold worked materials are largely open to question on account of the masking of the real figures due to internal stresses left in the material after the cold working operation. In referring to the effect of inclusions on cold working, the author states that the elongation these receive by hot working may be further extended by cold working. Surely the proper view is that the inclusions are harmful in such materials since they break instead of elongating and thus form discontinuities of material.

The other lectures do not call for special comment. The notes on case-hardening and the hints to practical designers are very suggestive. There is, unfortunately, a certain amount of repetition and overlapping of the different sections, but perhaps this is unavoidable in a book of this kind where the sections are contributed by different writers.

The publication is open to one serious criticism. The illustrations of aircraft machines and engines given are generally so small as to be of little use, whilst the micro photographs used throughout the book are generally given without any indication of the magnification employed.

The book may be valuable as a record of certain metallurgical features of the war period.

THE CASE FOR METAL CONSTRUCTION

Tables Omitted from January Issue (Pages 5 and 6).

TABLE I.

Material.	E Tons/□″.	Sp. Gr.	E/S.G.
Spruce, Spec. 2 VI.	670	·45	1490
Steel	13,000	7·8	1667
Duralumin Aluminium Alloy, Spec. 2 L.3	4,790	2·85	1680
Magnesium Alloys	2,400	1·75	1370

TABLE II.

Material.	Identification Composition.	Critical Stress.	Specific Gravity.	Critical Stress. Specific Gravity.
Spruce	Spec. 2 VI.	2.01	0.45	4.47
Hard Rolled Alum Strip	L.4 — Al. 98% ...	8	2.6	3.08
Al. Alloy Strip Duralumin	Spec. 2 L.3. Cu. ... ... 4.0% Mn. ... ... 0.5% Mg. 0.5%	13.5	2.85	4.74
H.T. Alum Alloy ...	L.29	19	3.15	6
Mild Steel	Spec. S.3. C. 0.2% Normalised	18	7.8	2.31
Plain Carbon	C. 0.1% Cold Rolled	28	7.8	3.59
	C. 0.1% Blued ...	24	7.8	3.08
B. and P. Test Results	Spec. S.44 C. 0.4% — Blued ...	35	7.8	4.49
Alloy Steel Strip ...	Spec. S.43. C. 0.2%, Mn. 0.4%, Ni. 3.5%, Cr. 1.0%, Va. 0.1%	40	7.8	5.13
Hardened and Tempered	Spec. S.40. C. 0.3%, Mn. 0.4%, Ni. 4.25%, Cr. 1.0%, Va. 0.1%	65	7.8	8.33

NOTE.—Except where otherwise stated the Figs. are for B.E.S.A. Specification.

The Journal
of the
Royal Aeronautical Society

(Founded 1897 in succession to the Annual Reports)

Edited for the Council by J. LAURENCE PRITCHARD, Fellow

No. 147 MARCH 1923 Vol. XXVII

NOTICES

Election of Members

The following members were elected at a meeting of the Council held on February 20th :—

Honorary Fellow.—J. L. Pritchard.

Associate Fellows.—F. W. Johnson, M. A. Zahra.

Students.—G. A. Fowkes, C. T. Travers.

Foreign Member.—A. R. Stevenson.

Wilbur Wright Lecture

Dr. Joseph S. Ames, of the American National Advisory Committee for Aeronautics, has accepted the Council's invitation to read the Eleventh Wilbur Wright Lecture in the Theatre of the Royal Society of Arts, John Street, Adelphi, at 5.30 p.m., on Thursday, May 31st. He has selected as the subject of his address, " The Relationship between Aeronautical Research and Aircraft Design."

Donations

The Council desire to acknowledge the following donations :—

" Planes and Personalities," by Cunningham Reid.

" Edward Teschmaker Busk," by M.B. These two books, formerly in the possession of the late Sir Walter Raleigh, were presented by Captain J. Morris, A.F.R.Aë.S.

" Notes on the Life of Frederick Marriott," presented by his grandson, Sir Cyril Kirkpatrick, M.Inst.C.E.

" In Full Flight," by E. Vine Hall, presented by the Author.

Lantern slides from Major Cleghorn, C. R. Catesby and C. T. Travers.

Annual General Meeting

The Annual General Meeting of voting members of the Society will be held in the Library at 5.0 p.m., on Tuesday, March 27th. The Council's Annual Report and Statement of Accounts for the year 1922 will be found on a later page.

Students' Section

A visit to the works of Messrs. S. Smith and Sons, instrument makers, Edgware Road, Cricklewood, N.W., has been arranged for Saturday, March 17th. Students wishing to take advantage of this opportunity should meet at Messrs. Smith's works at 9.40 a.m.

Further visits have been arranged as follows, of which details will be announced later :—

Thursday, April 19th.—Marconi Company, Chelmsford.

Wednesday, April 25th.—Napier Engine Company, Acton.

Saturday, May 5th.—Vickers Limited, Weybridge.

Wednesday, May 30th.—Fairey Aviation Company, Hayes, Middlesex.

Forthcoming Arrangements

March 1, 5.30 p.m.	Royal Society of Arts. Major F. M. Green, O.B.E., Fellow, "Air Travel with Special Reference to the Helicopter."
,, 8, 7.0 p.m.	*Students' Section.*—Society's Library. Mr. J. D. Campbell, "Air Transport."
,, 15, 5.30 p.m.	Royal Society of Arts. Professor B. Melvill Jones, A.F.C., Associate Fellow, "The Control of Aeroplanes at Slow Speeds."
,, 20, 5.0 p.m.	Council Meeting.
,, 22, 7.0 p.m.	*Students' Section.*—Society's Library. Mr. S. H. Evans, "The Variable Camber Wing."
,, 27, 5.0 p.m.	ANNUAL GENERAL MEETING. Society's Library.

W. LOCKWOOD MARSH, *Secretary.*

ROYAL AERONAUTICAL SOCIETY

58th Annual Report of the Council, 1922-1923

Council

Chairman.—Professor L. Bairstow, C.B.E., F.R.S.

Vice-Chairman.—Lieutenant-Colonel M. O'Gorman, C.B., D.Sc.

Brigadier-General R. K. Bagnall-Wild, C.M.G., C.B.E., Mr. Griffith Brewer, Wing Commander T. R. Cave-Browne-Cave, C.B.E., R.A.F., Sir Mackenzie Chalmers, K.C.B., C.S.I., Mr. H. P. Folland, Sir Robert Hadfield, Bart., Captain G. de Havilland, A.F.C., Squadron Leader R. M. Hill, M.C., A.F.C., Professor C. F. Jenkin, C.B.E., Professor B. Melvill Jones, A.F.C., Lieutenant-Colonel J. T. C. Moore-Brabazon, M.C., M.P., Mr. J. D. North, Lieutenant-Colonel A. Ogilvie, C.B.E., Professor A. J. Sutton Pippard, D.Sc., Colonel The Master of Sempill, A.F.C., Major R. V. Southwell, Lieutenant-Colonel H. T. Tizard, A.F.C., Major H. E. Wimperis, O.B.E.

Honorary Treasurer.—Mr. A. E. Turner.

Arrangements have been made with the Committee of the International Air Congress, which is to be held in London from June 25th to 30th this year, that the organisation of the Congress shall be undertaken by the Society. A grant, which it is believed will cover the additional expenditure, has been made to the Society for the purpose.

It is felt that considerable progress has been made during the period under review towards strengthening the scientific and technical aspects of the Society's activities. The lectures read have been such to effect this end, and the holding of the first examination for Associate Fellowship has also emphasised this tendency.

Library

In July last the Secretary called the attention of the Council to a number of valuable early works on aeronautics which had been collected by Messrs. Maggs Brothers, and were on the point of being offered by them to an American collector unless they were previously sold in this country. The Council therefore decided to approach the members of the Carnegie United Kingdom Trust Fund, with the result that in November intimation was received that the Carnegie Trustees were prepared to make a substantial grant for this purpose. The Secretary spent some days going through the volumes concerned (which numbered some 1,000), and selected from them all those of historical interest, numbering about 250, which were not already in the Society's Library. This acquisition has aroused great interest in literary circles both in England and abroad. An article on it which appeared in the "Times" Literary Supplement is reprinted in this issue.

Membership

The membership numbered 886 on January 1st, 1923, compared with 879 at the same date last year. All subscriptions owing prior to January 1st, 1922, have now been written off, and 50 per cent. of those still outstanding for the year 1922. The Council follow the practice of stopping the supply of the Journal to all Members who have not paid their current subscriptions by June 30th in each year, and removing from the rolls the names of those whose subscriptions have not been received prior to December 31st.

Silver Medal

Following upon the revival of the old practice of awarding the Society's Silver Medal to the author of the paper deemed by the Council to be the best published in the Journal during each year, the silver medal for the year 1921 was awarded to Mr. H. R. Ricardo for his paper entitled "Some Possible Lines of Development in Aircraft Engines."

R38 Memorial Research Fund

It has been decided to devote some of the money raised for this Fund, which at present totals £1,264 19s. 11d., to the placing of a tablet in memory of those lost in R.38 on the wall of the Library, for which a design by Mr. Paul Cooper has been accepted by the Council. In addition to this a prize of 25 guineas is to be awarded annually for a paper on some technical aspect of aeronautics, preference being given to papers dealing with airships. For the first year of this award, the list for which closed on December 31st last, 12 entries have been received, including entries from Germany, U.S.A., and Italy. All papers so far presented deal with airship problems.

Associate Fellowship Examination

The first examination for Associate Fellowship was held in the Library on Tuesday, September 26th. Most of the candidates offered "Aerodynamics" and "Strength of Materials and Theory of Structures" as their subjects, though the papers set on "Heat Engines" and "Meteorology and Navigation" were also taken. All the papers set were published in the issue of the Journal for November, 1922, as specimens for future candidates.

Technical Discussion

It was suggested to the Council during the year that opportunities should be afforded to members for more detailed discussion of problems in aeronautics than is possible at the ordinary meetings of the Society during the lecture programme. Mr. H. Glauert therefore presented a paper on "Theoretical Relationships for the Lift and Drag of an Aerofoil Structure," which was discussed at well-attended meetings in the Library on November 30th and January 10th and is still continuing. The paper and discussion will ultimately be published in the Journal.

Journal

Commencing with the issue of January, 1923, the title of the Journal has been changed to "The Journal of the Royal Aeronautical Society" in order to identify it more definitely as the medium for the publication of the Society's proceedings.

An interesting letter in this connection has recently been received by the Secretary from Major Baden-Powell, who was for some years President and for a time acted as Honorary Secretary of the Society, recalling the fact that the Journal was originally started in 1897 as a private venture of a group of members, and only later (apparently in 1909) became actually the official publication of the Society.

The Council are able to record that the circulation of the Journal among persons outside the membership has very considerably increased during the last twelve months. The number of annual subscriptions have increased from 81 to 136 in that period, and the casual sales show a similar improvement. They are glad again to record their appreciation of the generous help of Mr. J. L. Pritchard in acting as editor and remitting to the Society as a donation his fees. Owing to his efforts the interest and value of the Journal continues to increase, and they desire to congratulate both him and the Secretary on the results shown by the above figures.

Students' Section

In connection with Students' meetings, it will be remembered that the Council announced last year that the Pilcher Memorial Prize for Students would be awarded annually to the Student author of the best paper inaugurating discussion at these meetings.

The first award was made to Mr. S. H. Evans for his paper on "Some Notes on Commercial Aircraft," and books to the value of £5 were accordingly presented to him.

At the first meeting of the present session on October 12th, 1922, Mr. T. C. Sharwood was elected Honorary Secretary in succession to Mr. S. H. Evans.

Since the last report the following visits have been arranged for Students :—

1922.

May 6.—De Havilland Aircraft Company.

,, 31.—Royal Aircraft Establishment.

June 10.—National Physical Laboratory.

1923.

Jan. 27.—London Terminal Aerodrome, Croydon.

The full programme of meetings for the current session is as follows :—

1922.

Nov. 9.—" Airships," H. C. Brown.

,, 23.—" Some Practical Points in Aero Engine Design and Construction," G. R. Irvine.

Dec. 14.—" The Navigation of Aircraft," A. P. Rowe.

1923.

Jan. 25.—" Discussion on English and German Methods of Estimating Aeroplane Performance," F. Radcliffe.

Feb. 8.—" Experimental Methods in Aerodynamics," W. L. Le Page.

,, 22.—" The High Lift Wing," T. A. Kirkup.

Mar. 8.—" Air Transport," J. D. Campbell.

,, 22.—(Title unknown), S. H. Evans.

The lantern has now been repaired and a screen fixed in the Library so that slides may be shown at Students' meetings.

(*Continued on page* 88.)

AERIAL

(The Royal

Balance Sheet,

		£ s. d.	£ s. d.	
To *Nominal Capital*— Divided into 20 Shares of 1/- each and 999 Shares of £1 each		1000 0 0		
,, *Capital Issued and Called Up*— 19 Shares of 1/- each				·19 0
,, *Sundry Creditors*				1141 0 1
,, *Subscriptions Received in Advance*				29 7 0
,, *Carnegie U.K. Trust*— Grant for the Purchase of Books expended during the year 1922 per contra £355 13s. 3d.				432 0 0
,, *Reserve Fund*— Entrance Fees and Life Compositions of present Members, as at 31st December, 1921		2615 6 0		
Receipts for twelve months to 31st December, 1922 ...		35 14 0		
		2651 0 0		
Deduct Income and Expenditure Account Deficiency at 31st December, 1921 ...	1406 2 0			
Add Surplus of Expenditure over Income for year to 31st December, 1922	278 8 8			
		1684 10 8		
				966 9 4
				£2569 15 5

Income and Expenditure Account

	£ s. d.
To Office Rental, Lighting and Insurance	327 16 8
,, Salaries	1135 17 8
,, Printing, Stationery, etc.	69 8 6
,, Postages and Messengers	85 0 6
,, Library Expenses	36 18 10
,, Office Expenses	49 11 6
,, Exhibitions and General Meetings	77 4 10
,, Journals, Pamphlets, etc.	408 15 11
,, Audit Fee	15 15 0
,, Subscriptions Written Off	170 12 10
,, Donations	9 0 0
,, Income Tax	10 10 0
,, Examination Expenses	2 2 7
,, Honorarium to Editor of JOURNAL	10 10 0
	£2409 4 10

SCIENCE, LIMITED.

Aeronautical Society).

31st December, 1922.

	£ s. d.	£ s. d.	£ s. d.
By *Office Furniture, Printed Books, Bindings, Stationery, Old Prints, etc.*, as at 31st December, 1921		305 16 8	
Add Purchased during Year			
Books	355 13 3		
Furniture	25 0 0		
		380 13 3	
			686 9 11
,, *Stock of* Journals, *Etc.*			368 0 5
,, *Stock of Stationery*			10 0 0
,, *Sundry Debtors including Subscriptions owing*			286 19 0
,, *Investments at Cost*—			
£100 5 per cent. National War Bond		100 0 0	
£783 6s. 0d. 5 per cent. War Loan Inscribed Stock, 1929/47		725 13 6	
			825 13 6
,, *Cash at Bank*			385 0 5
,, *Cash in Hand*			7 12 2
			£2569 15 5

We report to the Shareholders that we have examined the books of the Society and have obtained all the information and explanations we have required. We are not in a position to judge of the value put upon the outstanding subscriptions. Subject to this remark, we are of opinion that such Balance sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of our information and the explanations given us and as shown by the books of the Society.

3, Frederick Place,
Old Jewry, E.C.2.
26th *February*, 1923.

(Signed) PRICE, WATERHOUSE & CO.

for the Year Ending 31st December, 1922.

	£ s. d.
By Annual Subscriptions	2067 16 1
,, Interest on Investments and Deposit	47 5 1
,, Thesis Fee	5 5 0
,, Donation from Mr. J. L. Pritchard	10 10 0
,, Balance, being Surplus of Expenditure over Income	278 8 8
	£2409 4 10

Lectures

The Tenth Wilbur Wright Memorial Lecture was read on June 15th, 1922, by Lieutenant-Colonel A. Ogilvie, who read a paper on "Some Aspects of Aeronautical Research."

The following is the full programme of lectures for the present Session, which is the 58th since the Society's foundation :—

1922.
Oct. 5.—Professor L. Bairstow, "The Work of S. P. Langley."
,, 19.—Mr. J. D. North, "The Metal Construction of Aeroplanes."
Nov. 2.—Major A. R. Low, "A Review of Airscrew and Helicopter Theory with Aeroplane Analogies."
,, 16.—Mr. R. McKinnon Wood, "The Co-Relation of Model and Full-Scale Work."
Dec. 7.—Professor C. F. Jenkin, "Fatigue in Metals."
1923.
Jan. 4.—Herr Hugo Junkers, "Metal Aeroplanes."
,, 11.—R. A. Frazer (Juvenile Lecture), "Testing Model Aeroplanes."
,, 18.—Major J. D. Rennie, "Flying Boats."
Feb. 1.—Mr. G. S. Baker, "Ten Years Testing of Model Seaplanes."
,, 15.—Wing Comdr. T. R. Cave-Browne-Cave, "The Practical Aspect of Seaplanes."
Mar. 1.—Major F. M. Green, "Helicopters."
,, 15.—Professor B. Melvill Jones, "The Control of Aeroplanes at Slow Speeds."

Honorary Officials

The cordial thanks of the Society are due to Mr. B. Woodward, the Honorary Solicitor, who has continued to advise on legal matters; and to Mr. A. E. Turner, who, as Honorary Treasurer, has given invaluable assistance in connection with the financial affairs.

Staff

The Council have pleasure in recording their great appreciation of the services and devotion of the staff during the year.

L. Bairstow, *Chairman.*

PROCEEDINGS

Fifth Meeting, 58th Session

A meeting of the Society was held at the Royal Society of Arts, John Street, Adelphi, London, on Thursday, December 7th, 1922, the Chairman, Professor Bairstow, in the chair.

The Chairman, in opening the proceedings, said it was his pleasure to introduce to the meeting Prof. C. F. Jenkin, or rather, Prof. Jenkin and what was a new phase of an important subject. He did not quite know how old the idea of fatigue was; in an indefinite form it might be a century old. The idea of the fatigue of metals probably existed before the middle of last century, when Wöhler gave quantitative form to the property. The experiments of Wöhler, which taught us not only to talk of fatigue, but to estimate it, made the first very distinct mark in the history of the subject. The name of Bauschinger was generally associated with the next step—Bauschinger discussed the relation between fatigue and elasticity, and propounded the general hypothesis that fatigue happened when the limits of elasticity of a material were exceeded. Difficulty had been found in defining elasticity, but, in spite of that, Bauschinger's work formed, perhaps what might be called the second definite mark in the understanding of fatigue. That evening the meeting was to hear from Prof. Jenkin what he (Prof. Bairstow) believed was destined to be a third definite mark. Not only had the fatigue limit been identified with the elasticity in Prof. Jenkin's work, but he had gone one stage further in the formulation of a theory, and had deduced the complicated results which had been observed in fatigue from certain very simple initial hypotheses. Still further, his hypotheses, having accounted for known facts, had led Prof. Jenkin to make predictions which were coming true. His paper was an excellent example of the importance of theory in practice. It was quite clear that, as a result of the production of this theory of fatigue, progress in experiment and in application would be greatly accelerated. He then called upon Prof. Jenkin to read his paper.

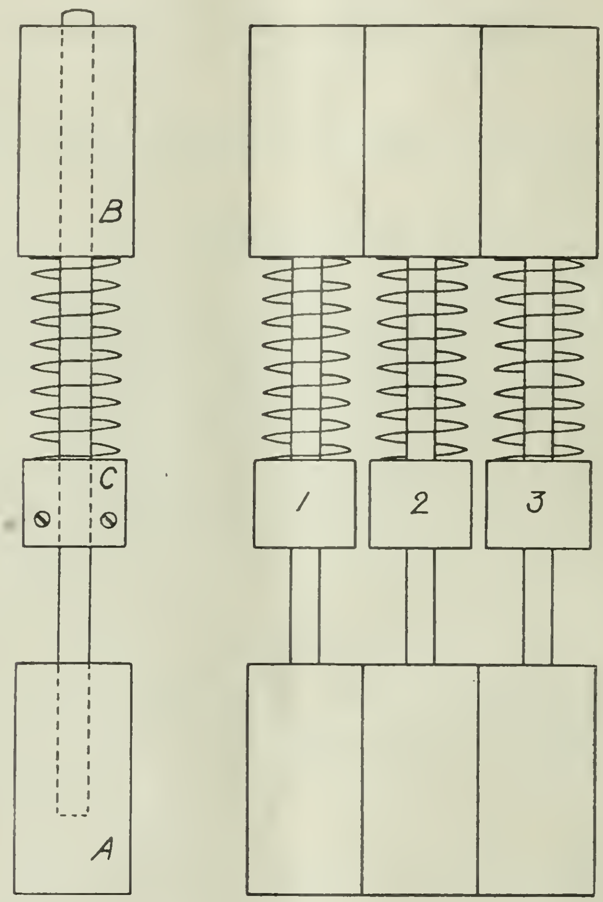
FATIGUE IN METALS

About a year ago I was asked to read this paper. About six months ago I wrote a paper, knowing that I should be very busy this autumn, and made a model to illustrate a small point in it. But as I played with the model to learn how to use it, it grew too strong for me and took command, and for the last six months I have been its obedient slave—for the model explained the whole of my subject—Fatigue. The model destroyed my first paper, and I have been obliged to write a new one in haste, putting it off from day to day as the model taught me new things. The written paper has suffered, but I hope you will find my account of the model more interesting than the tentative theories which filled the old paper.

But in any case, I can speak of nothing else.

A simple form of model is shown in Fig. 1; it is made up of three or more "units" fastened together top and bottom. The units are all similar; each is made up of three pieces—a block A with a rod fixed in it, a block B sliding freely on the rod, and a block C sliding on the rod stiffly, the friction being

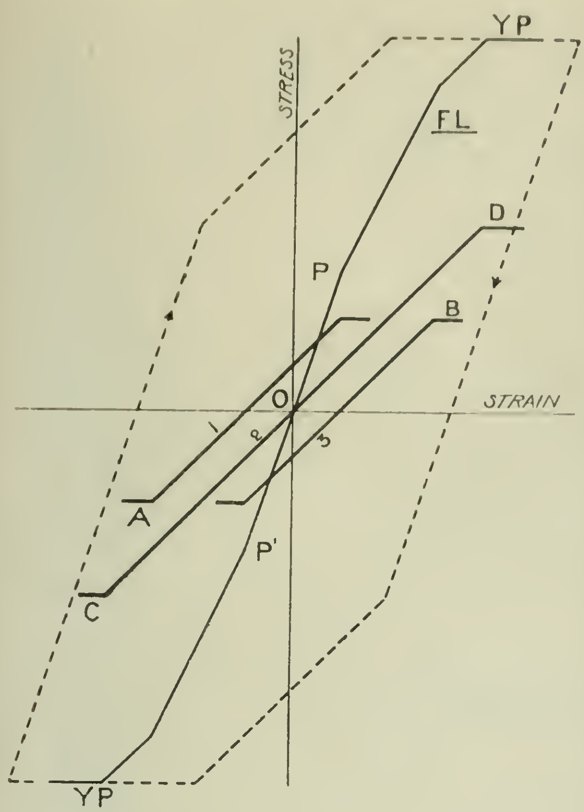
adjustable. Blocks B and C are connected by a spring. The model may be constructed in many other ways; the only requirements are (1) that it shall be elastic up to a point and then slip with solid friction; (2) that all the units shall not slip at once as the load on the model is increased. This latter requirement may be met in two ways—either by making the friction different in the different units, or by making the springs of different strengths; the former method is the simplest and is used in all the models I shall speak of to-day.

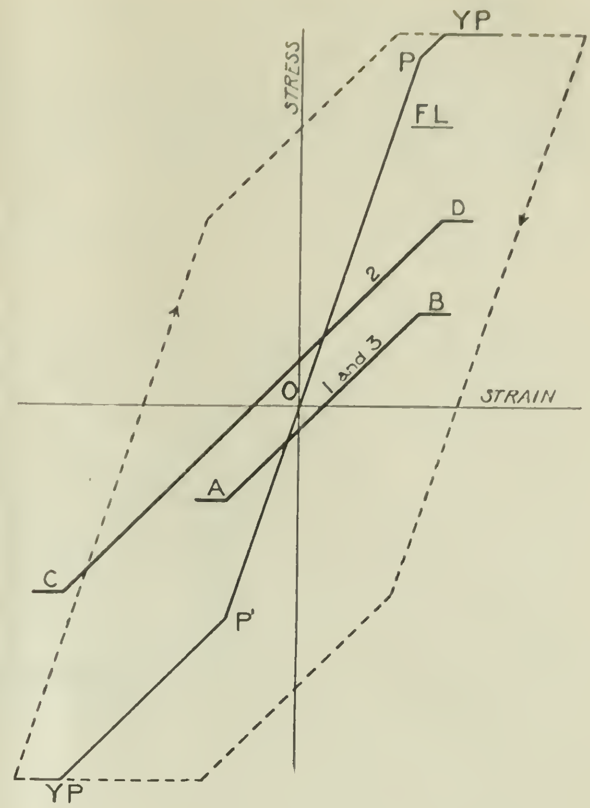


"THE ENGINEER" FIG. 1. SWAIN SC.

The model may be adjusted so as to have different properties by varying the friction in the different units and also by pushing up or pulling down some of the blocks C, so as to give the units initial compression or tension; the model will set itself so that the compression and tension forces balance.

Let us choose a three-unit model for the first test and give No. 2 unit twice the friction of the other two, and give No. 1 unit a small tension, and No. 3 unit an equal compression, so that the model will still be in equilibrium. Let us imagine that the model is put in a static testing machine with an extensometer attached and tested in tension. To show what will happen I will extend the model by hand. At first it stretches elastically, then No. 1 slips, and later No. 3 slips, and finally No. 2 slips. No larger force can be applied. The load/extension diagram given by the extensometer would have form shown in Fig. 2. This figure is easily constructed; the three lines at 45 degrees are the load/extension lines for the three equal springs. They slope up to the points at which they slip and then are horizontal. The graph for the whole model is found by adding the co-ordinates of all three. If the model were made up of more units the graph would be less angular. P is the Limit of Proportionality (Elastic Limit), Y.P. is the Yield Point. F.L. is the Fatigue Limit; how this





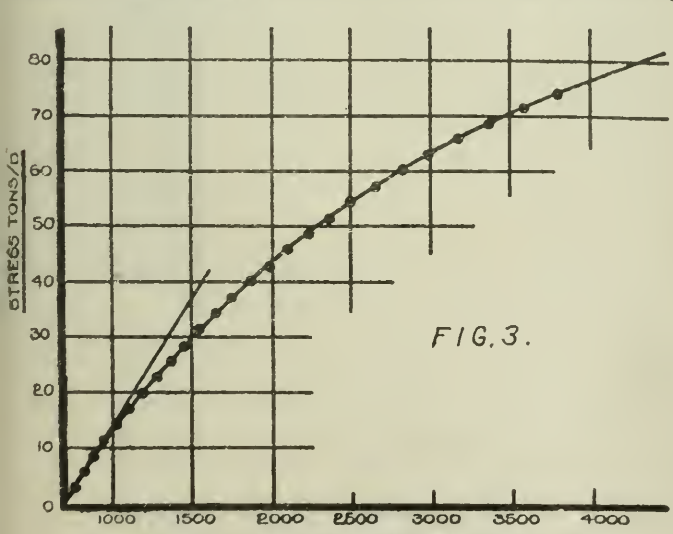
"THE ENGINEER" FIG. 2. FIG. 4.

is found I shall explain later. Now compare this with Fig. 3, which is a load/strain graph for hardened (but untempered) 100-ton steel.

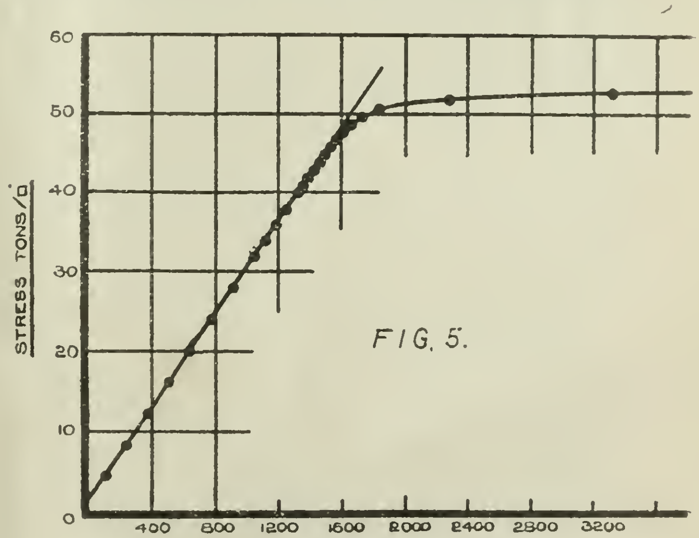
Next let us set the model differently. Give No. 2 unit a certain tension and Nos. 1 and 3 a compression of half that amount. The load/strain graph is shown in Fig. 4. Compare this with Fig. 5, which is the load/strain graph for a well-tempered steel.

Next let us take a model of ten units and give eight of the units nearly equal friction and the remaining two slightly less. As I pull this model, the

AIR-HARDENED STEEL (UNTEMPERED)



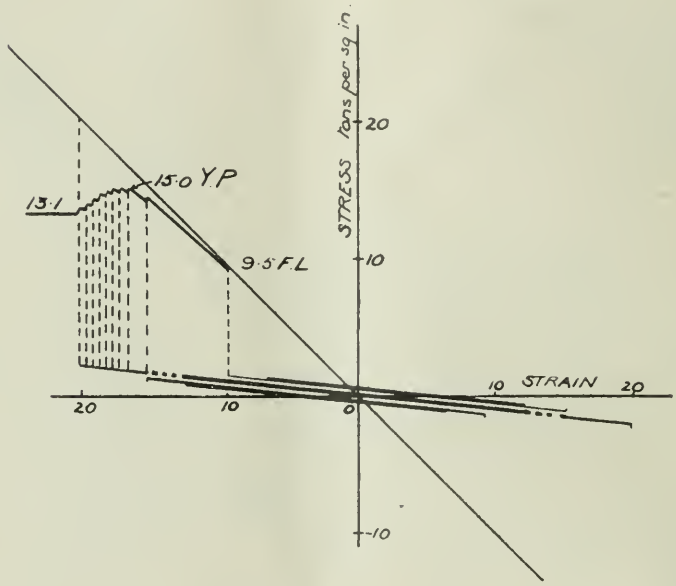
AIR-HARDENED STEEL (TEMPERED)



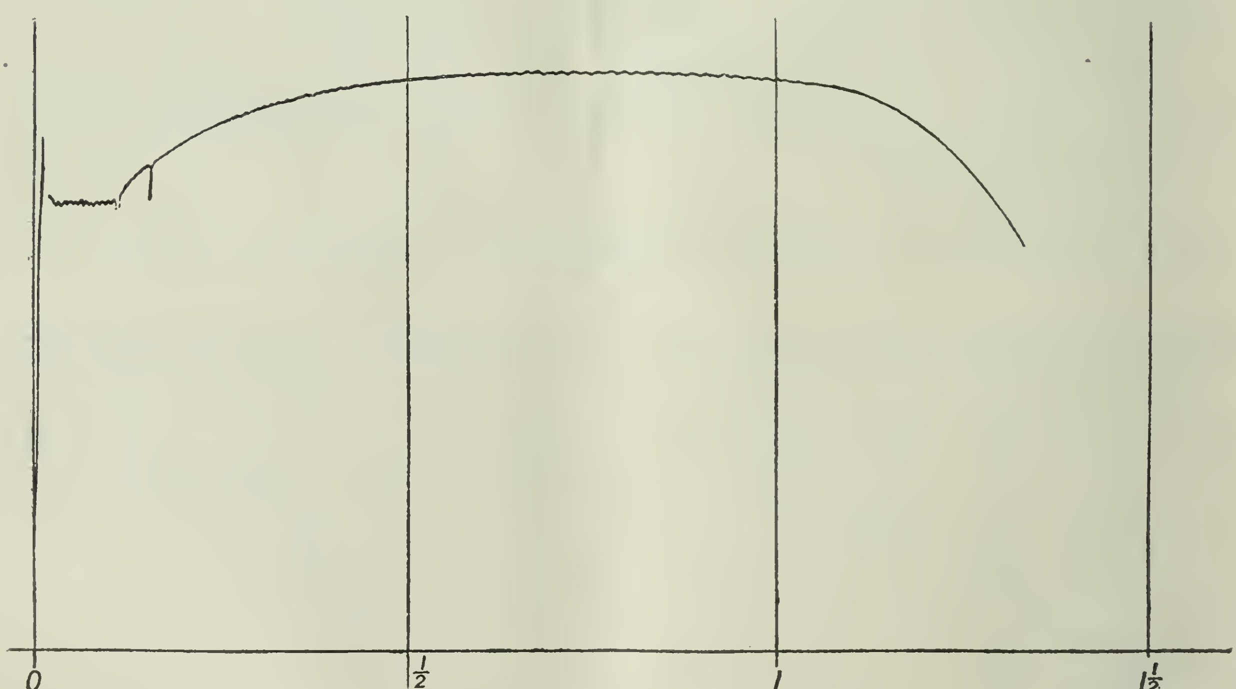
"THE ENGINEER" SWAIN SC.

weakest unit slips first, then the next. As each slips, more load is thrown on the rest, and when the first of the eight nearly equal units slips the extra load will cause the next to slip at once, and they will all slip—Jack-run-for-mustard. The graph for this model is shown in Fig. 6. Compare this with Dalby's photographic record for mild steel, Fig. 7. In this explanation I have assumed that

SWEDISH IRON



"THE ENGINEER" FIG. 6. SWAIN SC.



"THE ENGINEER" SWAIN SC.

FIG. 7.

when the unit slips it becomes weaker and so throws more load on the rest. I return to this point later.

But there is another very pretty point to notice. Watch the model closely; the units tend to move in little jerks, not quite smoothly, owing to the difference

between the static friction and the sliding friction. These little jerks appear in Dalby's photograph of the steel.

If we compress model No. 2, we shall get the lower curve in Fig. 4. Note that the elastic limits are different in compression and tension. If we carry the test on these models round a cycle we shall get the hysteresis loops shown in Figs. 2 and 4. These have the typical shape of metal hysteresis loops, curved rising lines and straight falling lines. The model will thus represent any of the typical steels very closely—up to the yield point. I have not made it go higher for reasons I shall explain later.

Now let us see what it does when tested under alternating loads. To start with, apply a large alternating strain to the three-unit model. All the units slip backwards and forwards, but as the amplitude of the motion is reduced, first one, then another, and finally all the units cease slipping. The amplitude now is the largest which the weakest unit will stand without slipping. The motion is elastic and, as Bauschinger first stated, the metal will stand this motion indefinitely. We have found that the fatigue limit. When the model comes to rest, the units are all stress-free; they are all in what may be called the central position. The fatigue limit points in Figs. 2 and 4 were found in this way.

The truth of Bauschinger's theorem—the identity of elastic range and fatigue range—has been confirmed by Bairstow (Phil. Trans. Roy. Soc., Vol. 210). Also by Gough ("Engineer," August 12, 1921). Gough invented a test which has turned out to be of the greatest value. A mirror is fixed on the end of the Wöhler test piece and adjusted to run truly in a plane perpendicular to the axis of rotation. A spot of light is reflected by this mirror on to a scale, and as the specimen is loaded this spot moves down the scale. If the deflections are plotted against the load, the graph is a straight line as long as the metal is elastic; thus the limit of proportionality in Gough's rotating test indicates the fatigue limit. The accuracy of this test has been repeatedly confirmed both by Gough and Lea for many metals. This test gives a method of finding the fatigue limit in a few minutes on a single test piece.

Let Gough's test be applied to a copper test piece. Under static test copper is not elastic for even the smallest load; the stress-strain curve bends off from the origin without any straight portion. The model representing copper has the maximum possible initial stresses in the units, so that one begins to slip the moment any load is applied. Applying the alternating load to the model the units will slip into central positions and we shall find a fatigue range as before. But this test will leave the units in a stress-free condition, and we ought, therefore, after the test to find the copper elastic. This remarkable result was found experimentally by Gough, and at my suggestion he repeated the test, raising the range gradually first to one-quarter, then to one-half, then to three-quarters of the full range. In this way the elastic limit of the copper was found to be raised first to one-quarter, then one-half, then to three-quarters of the full range. Thus the experiment exactly confirmed the prophecy based on the model.

The same experiment is now being tried on hardened 100-ton steel. The stress-strain curve (Fig. 3) we have already seen bends off almost from the origin. By applying an alternating stress the steel should be made elastic up to the fatigue limit, according to my theory.

The fatigue limit we have found is for equal tension and compression loads, but there is an infinite series of fatigue limits for unequal loads. How can their magnitudes be found? Suppose that an alternating strain were applied to the model unsymmetrically, so that the tension were greater than the compression; this might be increased till the weakest unit began to slip backwards and forwards; when that occurred we should have reached the fatigue limits for these particular unequal loads, and the range is the same as before. We may go on making such tests, making the motion more and more unsymmetrical, and the range will

remain the same till a new limiting condition intervenes, viz., that the maximum tensile stress must not exceed the ultimate strength of the test piece. This condition reduces the range which can be applied. The graph representing the range of stress plotted against the inferior stress will therefore consist of two straight lines—one horizontal at the range for the weakest unit and the other at 45 degrees through the ultimate strength. See Fig. 8. This graph, according to the text books, should be Gerber's Parabola (shown in the figure). The model appears to fail here. But compare the graph with the actual results shown in Fig. 9 (Bairstow's paper, Fig. 7). The model is clearly right and Gerber wrong.

Before considering further properties of the model, let us see whether there is anything in the metal which can behave as the units in the model behave. The crystals are obviously the units. Ewing and Rosenhain* have shown that

SWEDISH IRON

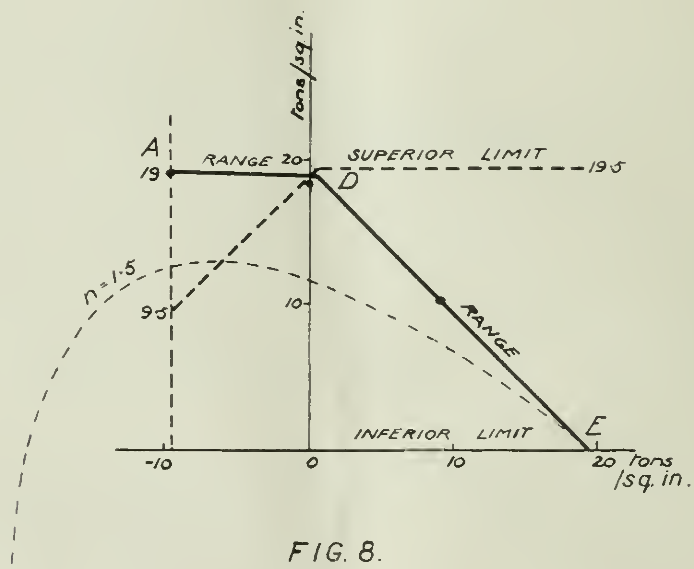


FIG. 8.

"The Engineer" Swain Sc.

they are elastic up to a point and then slip along slip planes. They also pointed out that the slip planes lie at all angles with the line of action of the load, so that the resolved shearing stresses along the planes will have all values from a maximum in planes at 45 degrees, to zero in planes perpendicular to the load. Thus the crystals will not all slip at once, but one after another. Thus we see that the known properties of the crystals exactly correspond with the assumed properties of the model. Finally, Ewing and Humfrey† showed that when slipping backwards and forwards takes place the slipping surfaces wear and a crack is ultimately formed, which grows till final fatigue failure occurs; thus our criterion for the fatigue limit is correct, viz., that we must have no slipping even in the weakest unit.

But if the model represents the metal so accurately, why does it cease to do so at the yield point? The ultimate strength of a metal is much higher, but the ultimate strength of the model is the same as the yield point. It would not be difficult to alter the model to make it imitate the real metal, but the additions necessary for this purpose would not correspond with any reality in the metal. The rise of strength in the metal above the yield point is, I believe, due to the mutual interferences between the crystals. These must have a great effect

* Phil. Trans. Roy. Soc., Vol. 193A. † Phil. Trans. Roy. Soc., Vol. 200, p. 248.

as soon as there is much distortion, that is after the yield point. The model and my theory are therefore limited to stresses not exceeding the yield point.

But fatigue is not quite so simple as this. Think of a crystal shearing and then of the two parts slipping backwards and forwards on each other; we should expect to find that it required a larger force to shear the crystal than to keep it slipping when once it had been sheared. In other words, that the

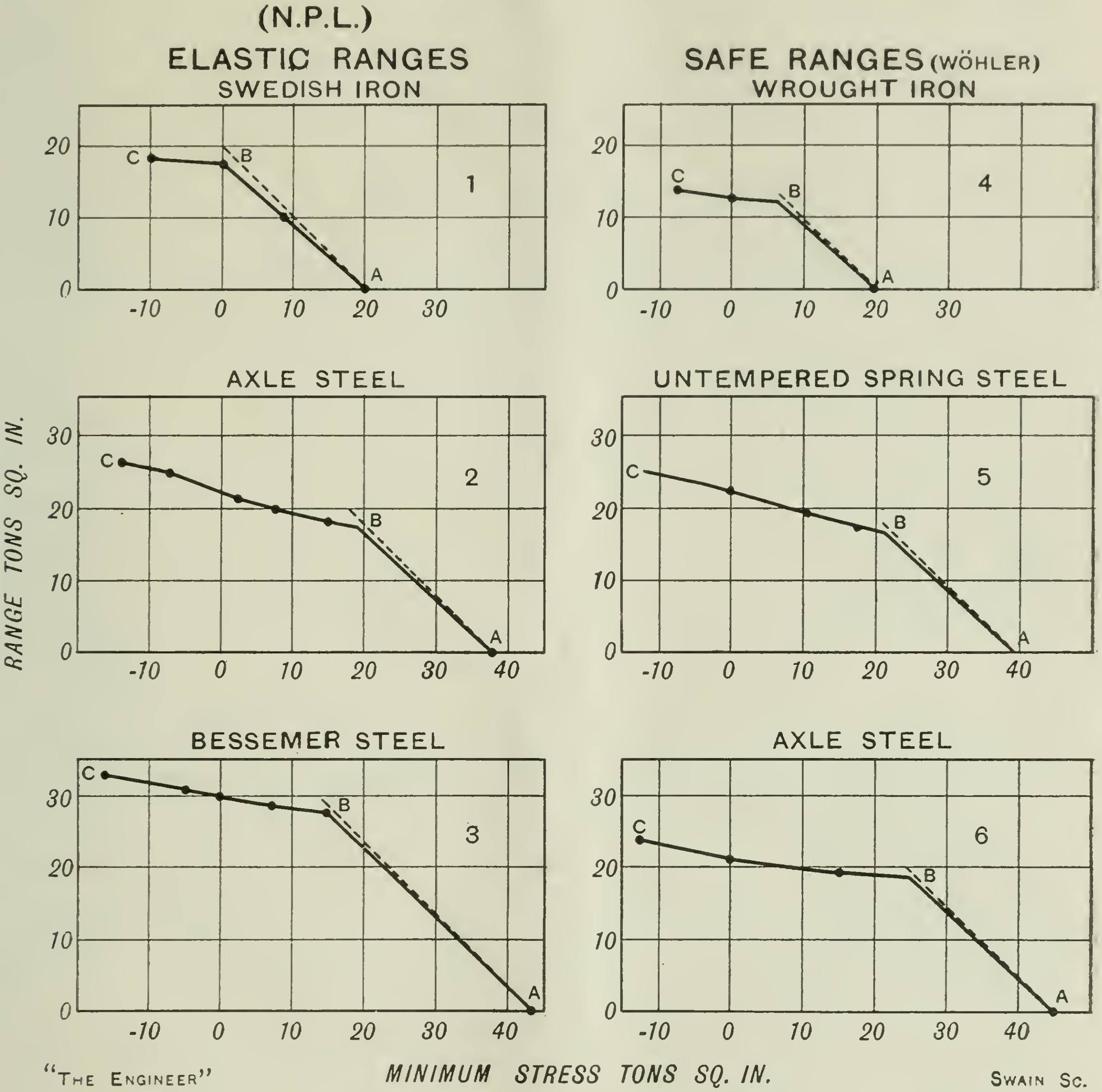


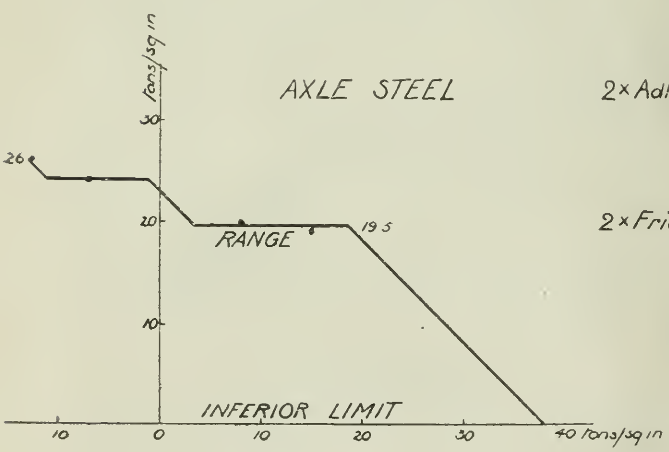
Fig. 9.

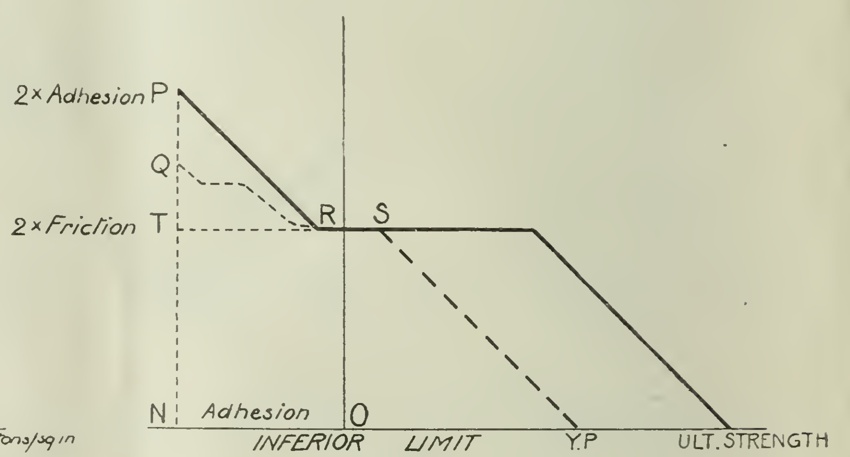
adhesion between the surfaces would be greater than the *friction*. Experiments show that this is the case. The model can be altered so as to show this difference by making the sliding blocks grip a collar on the rod, off which they slip when the force is greater than the adhesion. How will this modification of the model affect the results we have already got? You will remember that this modification was assumed in the ten unit model. I have not time to prove it to-night, but you will find that nothing we have so far found is essentially altered till we come to the graph of the complete series of fatigue ranges. This may be altered, but is not necessarily altered. Fig. 10 shows the range graph

for a five-unit model arranged to represent axle steel allowing for the effect of adhesion. Instead of being horizontal at the top, it rises by steps, but these steps may be smoothed out by using more units in the model. Compare this with Bairstow's graph for axle steel, Fig. 9; you will see how closely they agree.

This graph suggested two remarkable experiments. If all the adhesions could be broken down, then the fatigue limit should fall to the value 19.5. This can be done by bringing the metal into the "cyclic condition" by overloading it under alternating stress and then gradually reducing the load. All the adhesions are broken down by this treatment, and the crystals are left in the stress-free condition, as we have seen. Tested in this condition, axle steel should have a fatigue limit 25 per cent. lower than in the normal condition. I tried this experiment on some 0.33 carbon steel. The fatigue limit was lowered 29 per cent.

The second experiment was even more remarkable. If the steel could be made stress-free and then have its adhesion restored, the fatigue limit should be raised to the full adhesion value—some 20 or 30 per cent. above the ordinary value. I took the sample which I have just described, which was in the stress-free condition, and boiled it in water for two hours. This caused the crystals to heal, and so restored their adhesion. I then tested it, and the fatigue limit





"THE ENGINEER" FIG. 10. FIG. 11. SWAIN SC.

was 21 per cent. above the original value. These remarkable experiments show that the fatigue limit of steel can have any value between two limits; they are shown in Fig. 11. Finally, these experiments give a direct measure of the ratio adhesion/friction, which for the steel tested was 1.7.

Other methods may be employed for raising the fatigue limit. If the metal is very slightly overloaded in the Wöhler test for a few seconds and the load then reduced again, the adhesion of some of the crystals is broken down, but so little damage is done that the sheared faces heal at once and the process can be repeated again and again, each time raising the maximum load a little. Gough, at the National Physical Laboratory, raised the fatigue limit in this way 20 per cent. for the steel already referred to, and Professor Lea in Birmingham raised another specimen by rather more.

These experiments show that healing of sheared crystals occurs and that this action may be hastened by very moderate temperatures. Also that the time required for healing depends on the extent to which the slipping faces have been damaged by rubbing. We are thus introduced to a new idea, healing may be taking place during a fatigue test and the ultimate result—fracture or safety—may depend on a sort of race between slipping and healing. Healing may be

thought of as the recrystallisation of the minute quantity of metal disarranged by slipping, and it is not difficult to see why this takes place easily and quickly when we remember that the surfaces between which it occurs are parallel arrangements of crystallised material.

If healing produces an appreciable effect it will raise the fatigue range above the elastic range, and the amount it raises the fatigue range will be greater the more time there is for the action to take place, *i.e.*, the slower the alternations of stress. A second modification of the fatigue range may occur, due to the fact, first demonstrated by Hopkinson, that it takes time for the crystals to slip. Hopkinson showed that for a very short duration of stress, say, one-thousandth of a second, metals were elastic far above their ordinary limit. This phenomenon should also raise the fatigue range above the elastic range, but will be most effective at high speeds. These two actions make it necessary to qualify Bauschinger's general statement on the equality of the elastic range and the fatigue range. The magnitude of the two effects is being investigated; it seems probable that healing may be very effective at the temperatures at which some parts of engines—for example, valve springs—work.

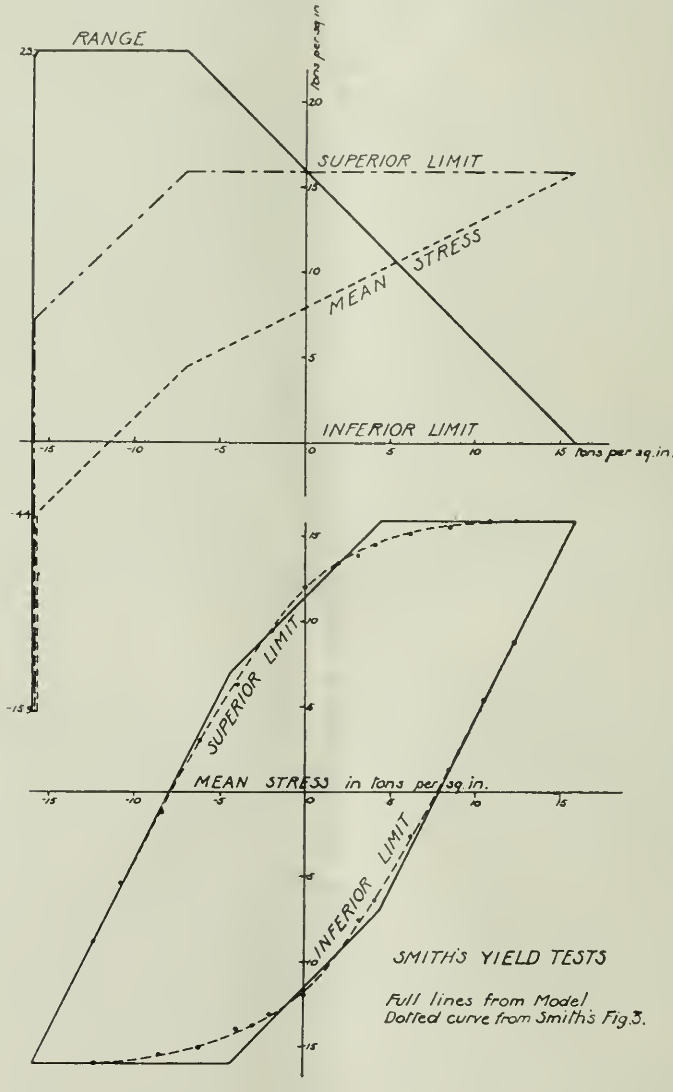
The events occurring in the metal undergoing a fatigue test may be examined in other ways. If there is friction between the parts of crystals there must be heat generated, and we can measure this heat by observing the rise of temperature of the metal. This was first done by Hopkinson, and many observers have used the method since. The temperature rise follows closely on the departure from elasticity, *i.e.*, it becomes large when the crystals start slipping. But there is found to be a very small evolution of heat at stresses far below the fatigue limit, and that the heat gradually increases as the load is increased. In other words, there is a small elastic hysteresis. The model does not indicate this elastic hysteresis, and what it is due to is not yet known; several explanations have been suggested.

Temperature observations have brought to light some very striking phenomena. When nickel is tested in the Wöhler machine there occur sudden heat bursts which rapidly disappear again. They recur every time the load is raised. How can these temporary evolutions of heat be explained? Consider the model. If it were stretched and compressed 2,000 times a minute without lubrication, what would happen? The sliding block would "run hot and seize." While it slipped heat would be generated, but the moment it seized the generation of heat would cease. That this may be a true explanation of what happens in nickel is confirmed by two striking facts. If the heat is due to slipping, heat bursts cannot begin below the elastic limit, for this is the point at which the first crystal slips. Experiment shows that they do not. Again, if a sample is tested up to fairly high stresses and all the crystals which slipped have seized, then if the test is repeated on the same sample no heat bursts can occur. This remarkable result is also confirmed by experiment. The trouble is like an infantile illness—a rapid rise of temperature which a day or two later subsides and immunity from the illness follows for the rest of one's life. Similar heat bursts occur in hard steel, but they last very much longer and are less pronounced.

In 1915 Professor F. H. Smith and Mr. Wedgwood published a paper* on what they call Yield Ranges, illustrated with a very large number of figures showing the results of their tests. I have gone through those figures and find that a model consisting of only three units will reproduce them all. Of course, the figures given by the model are very angular since it has so few units, but that defect could be remedied to any extent by multiplying the number of units. Four of Smith's figures are shown on the screen with the model graphs beside them.

* Journal of the Iron and Steel Institute, Vol. xci.

I am not sure if I have made it clear that the model will give quantitative results as well as qualitative. It is quite easy to construct a model to represent any given metal to scale. To show how accurate the results are, I will give one more illustration. Smith and Wedgwood summarised a large number of their results in a single figure giving yield ranges plotted against mean stress. I designed a model of three units to represent the same steel and constructed to scale the corresponding graph. Fig. 12 shows the graph of the yield ranges, which is very similar to that for the fatigue ranges. On it is plotted the superior stress and the mean stress. Below it, the superior and inferior stresses are plotted



"THE ENGINEER" FIG 12. SWAIN Sc.

on a mean stress base; the figure is a skew polygon. On this, to the same scale, I have drawn Smith's figure representing his test results. The agreement is, I think, striking. If the model could speak it would say: "I could have foretold the whole of the results of the tests which have taken you years to make."

The theory I have outlined applies to iron, steel, nickel and copper. Whether it is true for all metals is not yet known. It is not unlikely that brittle materials like cast iron may behave differently. It ought to apply to fatigue in shear as well as in tension, but whether it does has not yet been ascertained.

Perhaps you wish to know the practical end of all these researches and may feel inclined to say: "What is the use of your pretty models and fine theories?"

We hope before long to be able to put figures into designers' hands which will be a sure basis for all their strength calculations, figures which will not need factors of safety to allow for our ignorance of the strength of the materials. We hope to introduce fatigue limits into steel specifications, so that you will buy your materials on the basis of their useful strengths.

We hope to have methods of fatigue testing as simple as tensile tests.

We hope to be able to issue instructions which will save endless failures of engines.

All these things are already possible; we have now only to verify and confirm before issuing our reports.

I will venture to make one suggestion to-day to all engine builders. Never run a new engine on a full power test till you have raised the fatigue limits of all the highly-stressed parts. This can be done, as Gough and Lea have shown for test pieces, by making a series of short runs at gradually increasing loads, finishing up at the highest overload which the engine will ever be called on to exert and slowing down between each. No marine engineer ever starts up a ship's engine at full load; he coaxes it up gradually. This may be partly to give the bearings time to run in; but the experiments I have described suggest that it may also be most valuable in giving time for the steel to "slip into the central position," and to heal. A proper start may add 20 per cent. to the engine's strength. We shall soon know the best way to make this gradual start. This suggestion is partly guesswork still, but it is the sort of practical result we hope to achieve very soon.

In conclusion, I wish to express my very great obligations to the Aeronautical Research Committee. The Fatigue Panel of that Committee, of which I have the honour to be a member, has been working on this subject since its formation, and all the members of it have been indefatigable in their endeavour to throw light on this most puzzling subject. Researches have been going on at the National Physical Laboratory, Royal Aircraft Establishment, Royal Naval College, and the Universities of Oxford, Birmingham, Liverpool, Manchester and Edinburgh, financed by the Board of Scientific and Industrial Research on the recommendation of the Aeronautical Research Committee. This work is proceeding, and is now advancing rapidly. I have been keenly interested in it since I had to deal with fatigue failures during the war, and I feel sure that the Aeronautical Research Committee are performing a most valuable service to aeronautics by enabling these researches to be carried out.

Discussion

Sir Robert Hadfield said he had come to the meeting intending only to inspect the model which Prof. Jenkin had made, but he had been asked by Prof. Jenkin to hear the opening remarks, the result being that he not only remained, but was deeply interested in the admirable paper on this new method of explaining the behaviour of metals under stress. They were especially glad to have a communication from Prof. Jenkin, because he was one of those who had done so much during the Great War. He (Sir Robert) could not tell them how much Prof. Jenkin had done during the war to help on the technical man. He had had the pleasure of coming into contact with him on a number of occasions, and he (Prof. Jenkin) had helped to elucidate problems, the solving of which had proved of the highest value to the country. Continuing, Sir Robert said that during the past thirty years he had had to deal with the curious metal called manganese steel. This had a very curious property, namely, that, unlike ordinary steel, when a load was put on it, even quite a small load, it immediately began to change its form. If this material showed such an exceedingly low elastic

limit, how was it they could use it structurally? Yet it was so used. There was a certain point where mild steel was better than manganese steel, and yet for certain purposes manganese steel was immeasurably better than mild steel. In other words, the testing machine in such cases did not seem to help us to understand the problem. The other day he had had a communication from M. Frémont, the French engineer, who had done so much with regard to the subject of shock testing, and he (Sir Robert) was rather surprised to see such a question coming from so eminent an engineer. Monsieur Frémont had written and asked what was the reason for the drop in stress at the elastic limit. It seemed to him (Sir Robert) that the points put forward by Prof. Jenkin clearly explained the reason why this drop occurred. He would therefore be pleased to send a copy of Prof. Jenkin's paper to M. Frémont if he might be permitted to do so, because he thought he would find that the explanation now offered would answer the question put.

Prof. Jenkin had dealt with the tensile strengths of adhesion and friction. That was a most valuable fact, and he did not think anyone had ever put that forward so clearly as Prof. Jenkin had done that evening. They were all much indebted to the lecturer for having dealt with the matter so clearly and for having explained his odd-looking but most interesting models. They hoped that Prof. Jenkin would be led to continue his valuable research and give still more information. He (Sir Robert) noticed that Professor Jenkin would like to raise the point of introducing fatigue limits into steel specifications. Those who had to meet the stringent specifications that existed to-day hoped that Prof. Jenkin would thoroughly elucidate all these points, some of which he had admitted were not quite solved, before they were introduced into specifications. He made the suggestion specially because metallurgists were dealing with so many kinds of metal and varieties of steel. He was not speaking of non-ferrous compounds, because he did not pretend to know much about them; but, having in view the enormous range of tenacities of various steels, he did not see how fatigue limits could be introduced into the specifications, except, perhaps, for ordinary types of steel, which were used on a large scale, and which possessed much the same qualities whether made here, in America or on the Continent. He was sure that Prof. Jenkin would be careful before he introduced the absolute definition of fatigue limit, because it was not desirable to add to the difficulties of the manufacturer. In conclusion, he would say that the facts mentioned in Prof. Jenkin's paper would, he felt sure, enable both the steel maker and the engineer to get better and still better results, and he desired therefore to express his indebtedness to Prof. Jenkin for his valuable paper.

Mr. C. E. Stromeyer said the model was a very interesting one. He had seen it at Oxford, and it certainly explained a number of questions in regard to fatigue and elasticity, and allowed one to form a picture of the process going on; but he hoped the model would not become so perfect as to explain everything, because he was afraid that if that were to happen we might get settled down to some definite theories, which would hamper experimental progress. Bauschinger had been referred to, and he believed that Bauschinger's activity had been rather detrimental with regard to this whole question. He had laid down laws and theories about fatigue even before it was known whether a fatigue limit really existed. All that Wöhler had done was to show that a comparatively small stress would ultimately break down a material, but he could not say whether there was or was not a fatigue limit. The first time that the fatigue limit was definitely determined was when he (Mr. Stromeyer) had made his experiments on 27 pieces of steel. He, at the time, tested half-a-dozen examples of each, these being cut from 12in. lengths so as to ensure equality of material. The numbers of revolutions had ranged from about 2,000 up to a million, and, with the help of this steel tested both for bending and for torsion, he was able to construct that law of fatigue which he believed was now well established. He

had not at first got to the fatigue limit itself; but, having completed those experiments, and having found a rough law, he had devised a method for determining the fatigue limit by the detection of evolution of heat. He had in this connection to correct Prof. Jenkin when he had said that he (Mr. Stromeyer) had used the heat method for determining whether the slipping actually took place. What happened was that it had occurred to him that, as heat was generated during fatigue, this would be due to what might be called molecular friction, and if that heating did not take place up to a certain limit, but took place above it, he assumed the boundary would be the fatigue limit, in which case it ought to be in line with the other fatigue results. He found that that was so. Then Mr. Gough had carried out experiments, comparing the temperature rise with the deflection of the samples under the fatigue stresses, and had found that there was a kink as regards this deflection. Therefore, we had three methods of determining fatigue limits. We could find them by a series of actual fractures, we could find them by measuring the heat evolved, and by the extra deflection of the samples. At any rate, it was not until 1914 that we could talk of a fatigue limit. We did not know before then that it existed. All the theories evolved beforehand might be interesting, and it was very nice for people like Bauschinger to discuss possibilities, but he (Mr. Stromeyer) did not think his theories should weigh if they clashed with the subsequent experiments.

With regard to combined static and fatigue stresses, he would like to mention that he had lent a fatigue testing machine to Prof. Jenkin which was the only machine that had yet determined the fatigue limit for these compound stresses. The limit line was a curve, and if the model showed that the line must be straight, then the model showed what he hoped it would show, that it was not infallible and would therefore not discourage experimenters. He had read a paper in South Wales in which he had urged people to make fatigue experiments, and in which he had analysed Wöhler's only reliable experiment on compound stresses, with a view to determining the fatigue limits by extrapolation. The analysis confirmed his (Mr. Stromeyer's) experiments about the fatigue limit lines being curved and not straight. It was interesting to note that one sample stood 800 repetitions under a compound stress which was $1\frac{1}{2}$ tons above the ultimate breaking strength of the material. It was an interesting case, because it showed that there was some sort of freezing going on even during the fatigue, and it knocked some of Bauschinger's theories on the head altogether. He again hoped that the model would not be too perfect. He hoped that Mr. Gough would say something about the raising of the fatigue limit. He himself had not explored that field. He had made a very large number of fatigue tests and had measured the heat evolved during the life of the samples from the very beginning until the sample had broken, but had never noticed that the fatigue limit was raised. When once he had got past it, heat was generated, and the generation went on until the sample broke. The elastic limit, he believed, might be raised, and he believed some experimenters in America had proved this.

Prof. Jenkin said that they had actually raised the fatigue limit.

Mr. Stromeyer said he expected they would find that something must happen when they dealt with a combination of a very high permanent load and weak fatigue stresses. Evidently if the static elastic limit was passed, either the fatigue limit had also been passed and could not be recovered or a new fatigue limit would be created. Possibly this would require time—a sort of ageing process would be progressing. He had not been able to carry out that experiment, but he hoped Prof. Jenkin would do so.

Mr. H. Gough said that in going through all the published work on this subject he had seemed to get into a morass, until he had come to that paper which was the finest paper on fatigue ever written—that of Prof. Bairstow in

1910. At his department there were three men who were very keen on the subject of fatigue, and they had almost formed a society for discussing Bairstow, because every time they went through the paper they found something new and important. But there were two apparent inconsistencies in the theory of the subject that were, he believed, now cleared up. They were the question of perfect elasticity and the association of the appearance of slip lines at the limiting range of stress. Dealing with elasticity, the experiments of Guest and Lee, Hopkinson, Rowett and other workers showed that perfect elasticity was never obtained. He believed Prof. Bairstow would agree that the elasticity referred to in his paper was elasticity as consistent with elastic hysteresis. The other point was that researches had been made by Ewing, Rosenhain and others, who, although very cautious in their deductions, led one to believe that the first sign of the limiting range of stress having been exceeded was the formation of slip bands. A colleague of his, Mr. Hankins, had some time ago tested nickel and had obtained the extraordinary result that the fatigue range, under reversed direct stresses, was more than twice the range of the yield stresses in tension and compression. Thus a piece of nickel under alternating stress had been taken past the range of the elastic limits and of the yield points, and, presumably, no slip bands had been formed in the material. This they could not understand. Again, lately in America some researches on fatigue had been completed, one material used being "Armco" iron. It was practically a pure iron, with an ultimate tensile strength of about 19 tons per square inch, a yield stress of about $8\frac{1}{2}$ tons per square inch, and a fatigue range under reversed bending stresses of ± 11.7 tons per square inch. He was rather interested in their results and had obtained some Armco. In collaboration with a colleague, Dr. Hanson, he had been examining it under a microscope and was pretty well convinced that as higher magnifications were used, so more and more indications of slip would be seen on the surfaces examined, in the same way that more refined methods of measuring strain showed greater deviations from perfect elasticity. They had taken some specimens of Armco (possessing a limit of proportionality and yield stresses at 10 and 11 tons per square inch respectively), tested them under simple bending stresses in a Wöhler machine, and examined prepared surfaces at all stresses up to and exceeding the fatigue limit. The results were not yet reported on, but the tests showed most conclusively that once the elastic limit was passed, *i.e.*, a range of applied stress that was double the static limit of proportionality, undoubtedly curved lines appeared on the polished surface. Dr. Rosenhain had pronounced these curved lines to be true slip bands. Once the yield range was exceeded, there were undoubtedly thousands of slip bands, and they were fairly certain that slip bands appeared under a stress that was even less than the static limit of proportionality. Prof. Bairstow had used, in addition to reversed stresses, ranges of stresses in which the upper limit of stress exceeded the static yield point, and if he had used a microscope he would have found slip bands formed on the surface of the material. It boiled down to this—that with Prof. Jenkin's model, all the apparent inconsistencies were absolutely cleared up, and he wished to add his congratulations to Prof. Jenkin on his excellent paper.

Prof. C. H. Desch said that the conclusions expressed in Prof. Jenkin's Paper were of the greatest interest to the metallurgist. The behaviour of the model corresponded in the most wonderful manner with what was known of crystal structure at the present time, and he had been unable to detect any flaw in its application to metallographic theory. In the older treatment of the strength of materials, metals had usually been regarded as isotropic substances, but as a matter of fact their properties were different in different directions. It must be the case that when stress was applied it would not be equally distributed over all the crystal grains, but that some would be less favourably placed than others. In some work which was being done at Sheffield they had tried to follow the behaviour of a piece of metal while under tensile stress by using a polished sur-

face and keeping it under microscopical observation during stress with the object of detecting the formation of the first slip bands. This proved to be difficult to do, and a new method was devised by one of his research students, Mr. Cecil Handford. This method consisted in using a flat polished test piece, on the surface of which was laid a piece of mica, and on that again a strip of brass, the whole being clamped together and constituting a small electrical condenser. This was connected with a thermionic valve, oscillating with a certain frequency. At a short distance was placed a second oscillating valve, giving with the first beats of, say, 100 per second. The note thus produced was intensified by a two-valve amplifier, and was then heard in a telephone. The idea was that as soon as the first slip occurred there would be a minute ruffling of the surface, which would alter the capacity of the condenser. It was found that this actually occurred, and that the first production of slip bands could be recognised by a sharp rise in the pitch of the note in the telephone. He believed that in Mr. Stromeyer's method of testing, nickel gave out heat at a certain stress, then became elastic again, and then yielded again at a higher stress. This behaviour of nickel had also been observed by Mr. Handford's method.

In his account of copper, Prof. Jenkin had assumed that there were decided stresses in the individual grains of the annealed metal, some being in a state of internal tension and others in compression. He was inclined to doubt whether there were really such stresses, or whether neighbouring grains did not differ greatly in the ease with which they yielded under the stress, on account of the varying orientation of their crystal planes. If that were the case, one would expect to find considerable differences between the behaviour of specimens differing in the size of their grains. That explanation would not apply to the hardened steels, the grains of which were exceedingly small. With regard to the behaviour of metals beyond the yield point, he did not think that metallurgists would be greatly troubled with difficulties in that region, as the whole course of events could be understood on existing theories now that the information given by the X-ray method was available. It was the behaviour below the yield point, especially between the fatigue limit and the yield point, that presented so many difficulties, and that had now been so remarkably elucidated by the work of Prof. Jenkin.

The CHAIRMAN said that, so far as his experience of fatigue went, the model which Prof. Jenkin had shown accounted for practically everything. One of the difficulties he had always felt in connection with fatigue—and it was a matter which he was not able to take up because of the advent of aeronautics—was the possibility of reducing the yield point. It was quite clear, from his own experiments, that if the range of stress exceeded the fatigue limit, then premature yield could be produced; but he had not been able to produce yield by the application of a range of stress less than the fatigue range. One other point had always puzzled him, and Prof. Jenkin had not dealt with it that evening, and that was how did a specimen fail when both the limits were compression? The specimen became thicker and thicker, and more able to stand the load, and he could not understand how fatigue could produce failure. Perhaps Prof. Jenkin would be able to give some information on the point. Generally speaking, he thought they would all agree that the subject had been made a great deal clearer by the fact that Prof. Jenkin had found that observed phenomena depended on two important factors—a coefficient of adhesion and a coefficient of internal crystalline friction—and that these factors, utilised in a simple model, explained much that had previously been wholly empirical.

Prof. JENKIN, in reply to the discussion, dealt first with Sir Robert Hadfield's question as to manganese steel and its low elastic limit. He believed he had shown by the model that the elastic limit was practically an accidental point and had no importance whatever from any user's point of view. The points that were important were the fatigue limit and some of the other points, but not the initial elastic limit. That was more or less accidental, so that he did not think

Sir Robert need feel any anxiety about the merits of manganese steel because its elastic limit was low. He believed that Sir Robert Hadfield had had enough experience of his (Prof. Jenkin's) methods of dealing with specifications during the war to relieve him of any anxiety about his issuing them too soon, but he did intend to get fatigue limit into specifications. (Laughter.)

Mr. Stromeyer's remarks were interesting, but he believed that he disagreed with almost all of them. It was unfortunate when one's theory said that one could not do a thing which had been done experimentally. (Laughter.) He had done things which Mr. Stromeyer considered were impossible, and he believed he could claim to be one of the first to have done them. The raising of the fatigue limit was a point beyond discussion. It had been done by Prof. Lea, who had applied a very large number of repetitions, 40 million reversals of stress, 20 per cent. above the initial fatigue limit. Similar results had been obtained with even higher stresses by Mr. Gough at the National Physical Laboratory. He was particularly pleased to hear from Mr. Gough that the model had cleared up some of his difficulties. It was very difficult in discussing this subject to follow the arguments, and he did not quite follow what Mr. Gough meant. He was particularly pleased also to find that Prof. Desch considered that the model fitted in with metallurgical requirements. He had been urging metallurgists to deal with that side of the problem, but so far he had had very little response. The crucial point in the theory was the initial stresses in the crystals, as Prof. Desch had quite rightly suggested. What these were due to it was for him (Prof. Desch) to say, and not himself. He had suggested that a possible cause of the initial stresses might be the unequal contractions of the crystals along different axes. He was not metallurgist enough to say whether that was sufficient or not. Dr. Rosenhain had suggested a possible modification of the idea of initial stresses, namely, that some of the crystals might take the load before the others. He believed he (Prof. Jenkin) had devised a test which would determine whether that was true or not. He would be surprised if it turned out to be a possible explanation. It seemed impossible to him to conceive any doubt about the initial loads on the crystals when they saw them slipping under very low loads. The crystal must be under load if, the moment they put on more, it began to slip. That was a very convincing experiment, to his mind. He had not thought of the point raised by Prof. Bairstow about the fluctuating compression. That point would have to be considered; but the model had already suggested such a number of lines of research that were necessary that it was difficult to make a promise as to how soon he could take up that one. He thanked the members for the very kind way in which the paper had been received.

The meeting then closed.

JUVENILE LECTURE—MODEL AIRCRAFT

The following lecture was delivered by R. A. Fraser *before the Society on January 11th, 1923.*

Types of Aircraft

I have not prepared a formal lecture for this afternoon. Instead, I hope to interest you in a few experiments with models of aircraft.

In the first place, however, it may be useful to look at one or two pictures of actual aircraft, and at the same time to compare these parent machines with their smaller brethren, the models.

Let us commence with the largest known type of aircraft—the rigid airship; the R.38, for instance, was nearly 700ft. in length. With airships of this type, shape is preserved by means of an elaborate framework of light metal girders, and the whole weight of the machine, including the powerful engines, adds up to many tons. Whilst the airship is flying level, this enormous load must, of course, be kept lifted and balanced by some upward force. The upward force is called "buoyancy," and is similar to that which keeps a ship at sea from sinking; it is provided, in this case, by the volume of hydrogen contained in the gas bags of the airship.

Let us now imagine that we have successfully balanced an airship, such as the R.38, so that it is floating steady and level in still air. In order to drive the airship forwards against the resisting force of the air, we should have to exert a considerable forward push. For instance, if we wished to keep moving at a speed of 50 miles per hour, we should have to drive with a force of about two tons. During steady flight, the engines of the airship are allowed to turn half, or all, of the airscrews, and these provide the forward thrust constantly needed to balance the air resistance. On a model airship 1/200th the size of the R.38, the air resistance at 50 miles per hour would amount only to some two ounces.

Other smaller types of airship, such as the S.S.Z., exist in which form is maintained by gas pressure and not by a rigid girder-work. In a wind of 50 miles per hour the air resistance on a fully rigged model, 1/50th the size of an actual S.S.Z., would be about six ounces. Roughly two-thirds of this whole resistance acts upon the cars, fins and rigging, and only the remaining one-third upon the hull.

Let us now turn for a moment to the large commercial aeroplane, such as a typical Handley Page machine, in which the wing span extends to 100ft. During horizontal flight at 100 miles per hour the air resistance would be as much as two tons, and the air lift on the wings over 11 tons. This air lift is the force which balances the weight of the machine—fulfilling much the same purpose as the gas lift on the airship.

As a final example we might refer to the parachute. This is, perhaps, peculiar amongst aircraft in that its shape is designed to yield a large, rather than a small, air resistance. Once filled with air and dropping fully open, the parachute experiences an upward resisting force, which so usefully helps to balance out the weight of the passenger.

How Wind Forces are Measured

I now want to give you a rough idea as to *how* the wind forces which act upon these aircraft models, can be measured. Later on I shall attempt to show you *why* a knowledge of the forces is really helpful to the designer.

Before any exact measurements can be attempted, a thoroughly steady air current must be provided. A "wind tunnel," or "channel" is used for the purpose. You can regard a wind tunnel quite simply as a long tube, through which a stream of air is sucked by means of a propeller driven by an electric motor. Careful precautions are, of course, taken to ensure a thoroughly steady and reliable flow. The air which circulates from the outside is often much disturbed and apt to contain numerous eddies or air swirls. Such irregularities at the intake mouth can be straightened by means of a honeycomb grating placed at the mouth of the tunnel, and an even flow is thus secured. It is also found most effective to allow the air which has already passed through the tunnel to escape or diffuse back into the containing room through a group of parallel slots, spaced in a particular manner. For test the models are supported by means of spindles or wires in the most uniform portion of the air current, the forces exerted by the wind being measured out on accurate balances arranged sometimes above, sometimes below, the wind tunnel.

At the National Physical Laboratory the largest wind tunnel measures 7ft. by 14ft. in section; its containing room is 150ft. in length. Perhaps you do not fully realise the enormous air movements which take place when such a tunnel is running at high speed. At 70 miles per hour, for instance, over 20 tons of air pass through the channel in every minute.

Before the tunnel can be made of real use, some method must be found to determine the speed of the wind. I shall not attempt to explain in exact detail how this is accomplished. Generally, use is made of air pressure such as that which acts at the mouth of an open-ended pipe held facing the wind. If the other end of such a pipe be connected by means of a length of rubber tubing to a glass U-tube containing some convenient liquid, air pressure will be communicated to the liquid and bring about a difference of level between the two columns of liquid. For each wind speed there will be a new condition of balance, and so we might expect to find some relation giving a measure of the wind speed in terms of the difference of level. This, with certain modifications, is the principle actually adopted.

In order to find the *direction* of the wind, an instrument is used consisting of a pair of such open-ended pipes inclined at an angle, and connected to opposite limits of a U-tube. Suppose this instrument to be turned until no difference of level can be noticed between the liquid columns of the U-tube. The measured air pressures must now clearly be equal, and you will readily guess that the true wind direction must, therefore, equally divide the angle between the open-ended pipes.

Perfectly steady flight is not the only condition which has to be allowed for in tests upon aircraft models. Actual aircraft in flight are always liable to meet with small disturbances, such as atmospheric gusts or cross-currents, and a question of very great practical importance is the future behaviour of a machine after such irregularities have acted. Some aircraft recover their original condition of flight after disturbance; whilst others might, for instance, begin to take up an ever increasing swinging motion, and in time become quite unmanageable. I hope shortly to give you some examples of such different kinds of motion, by dropping a number of small parachutes. This branch of the subject contains, of course, many interesting and difficult problems, which call for very special arrangements and devices in the wind tunnel; but I shall not be able to tell you these methods this afternoon.

Flow-Patterns

You will perhaps be wondering how it is that measurements made upon a small model can teach us anything useful about the forces upon a full-sized machine. A complete explanation would not be altogether simple, but in a rough and very indirect way I hope to convince you that laws exist, under which one

might reasonably expect to fix some connection between the small and the large machines.

The wind forces acting upon any body are a direct outcome of the disturbance caused by the body to the passing stream of air. A careful comparison between such stream disturbances might, therefore, bring to light the required laws. Air moves past obstacles in a way similar to the flow of water, but with both fluids the stream disturbance or so-called "flow-pattern" is invisible. You have all seen, at one time or another, a smoke ring; it is really an air-ring—a very special type of air flow. The air-ring carries smoke along, and the flow becomes visible to your eyes. Perhaps not all of you have seen a water-ring. You can quite simply produce and make visible such a ring by allowing a single drop of coloured liquid to fall from a small height into a basin of water. If the coloured liquid is fairly heavy compared with water, you will find that the ring cannot last in a perfect form for very long. It will soon bud out into a set of smaller rings, and each of these will in turn continue budding (Fig. 1). These are only simple

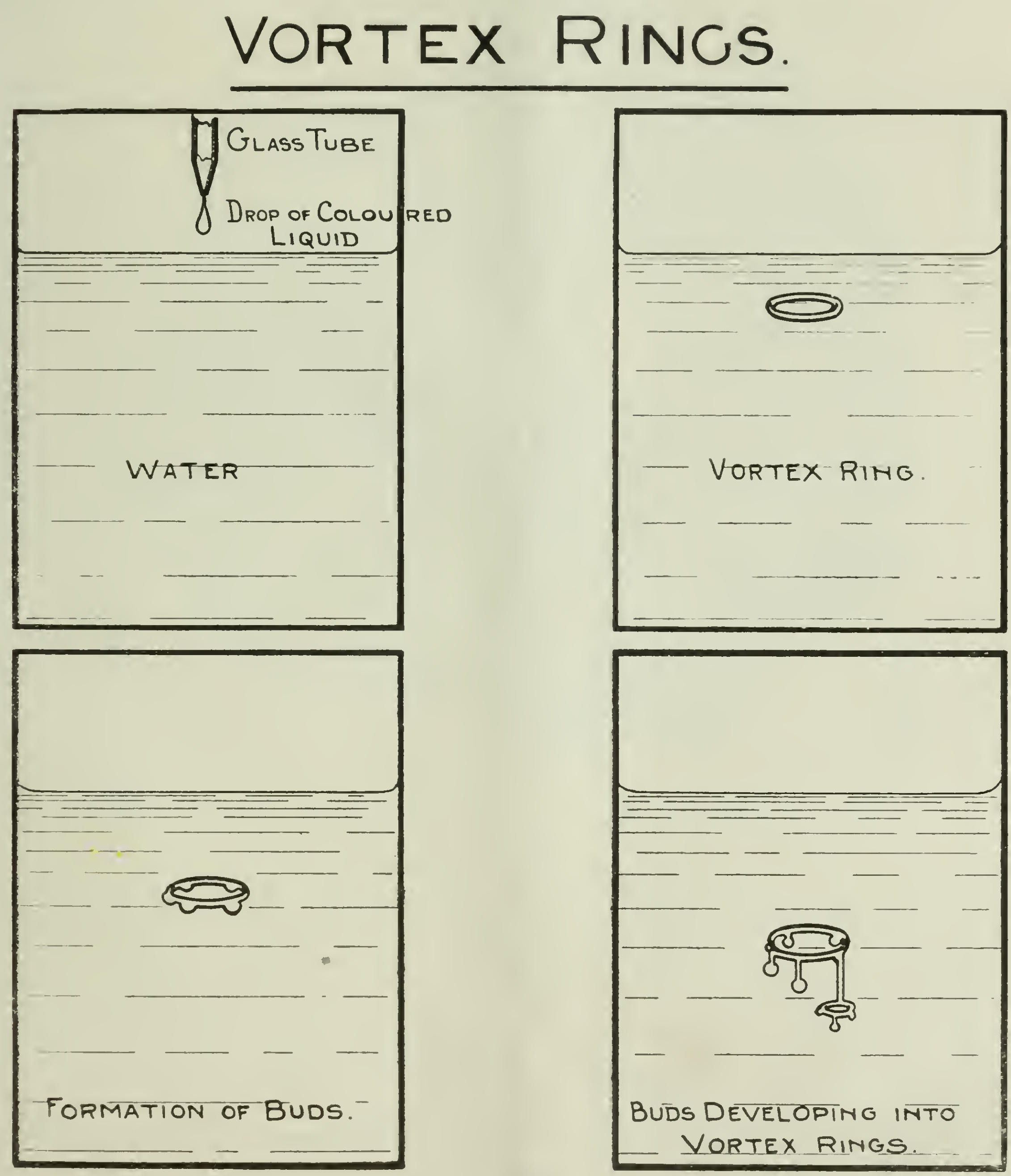


FIG. 1.

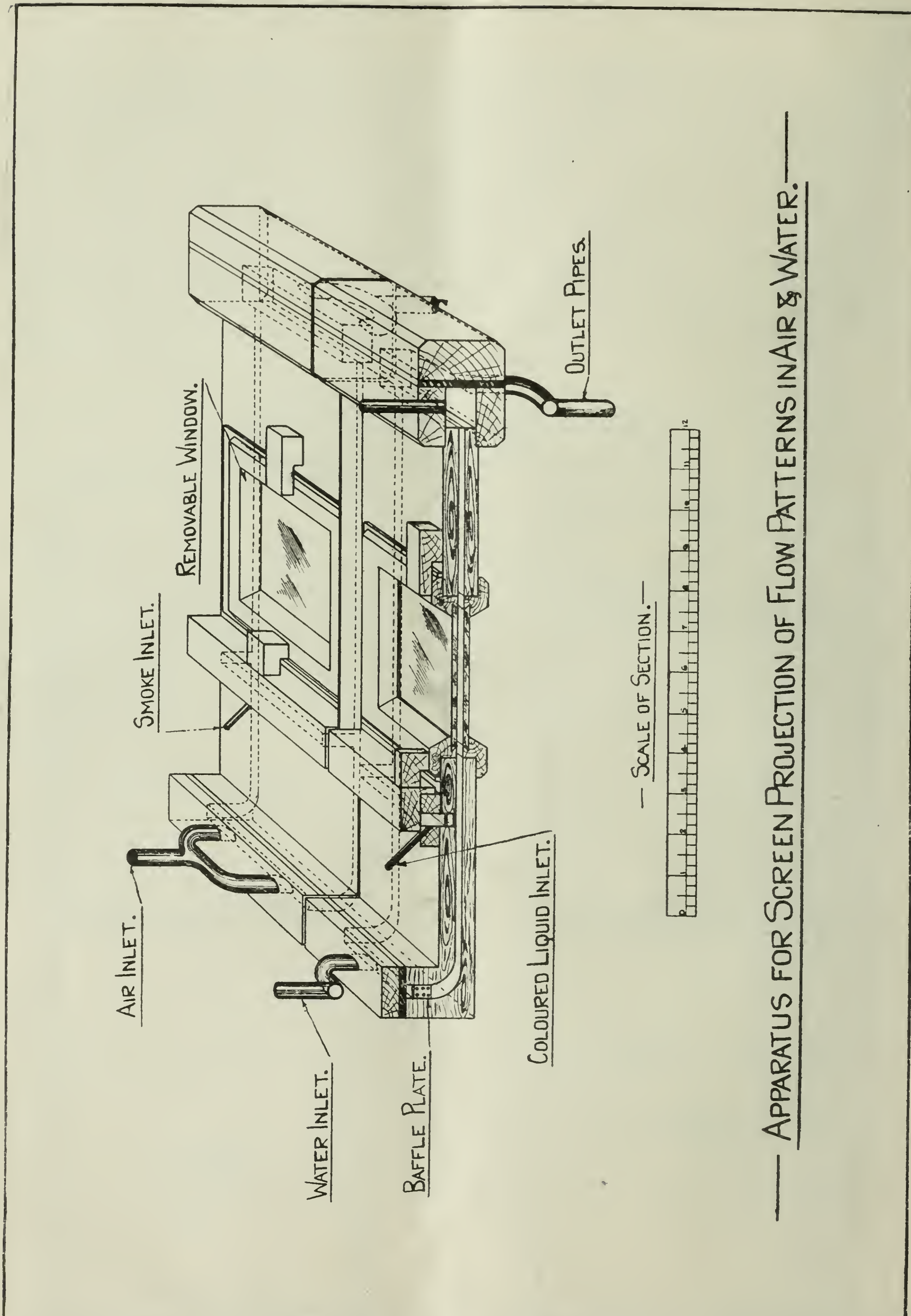

Apparatus for Screen Projection of Flow Patterns in Air & Water.
Outlet Pipes.
Removable Window.
Smoke Inlet.
Scale of Section.
Air Inlet.
Coloured Liquid Inlet.
Water Inlet.
Baffle Plate.

Fig. 2.

examples, but they suggest how flow-patterns can be shown up clearly with use of smoke or coloured liquid.

The apparatus I shall make use of in order to throw flow-patterns upon the screen, can be thought of most simply as a pair of shallow closed glass boxes, forming separate channels, ⅛in. deep, for a uniform air-current and a water current. (The arrangement is shown in fuller detail in the perspective sketch, Fig. 2.) Smoke can be introduced into the air current through a transverse row of small holes, and coloured liquid (methylene blue), through a similar set of holes, into the water current. The two boxes can be used very much like ordinary lantern slides.

With no model introduced, you would, at a low speed, merely see upon the screen a row of parallel straight streamlines, along each of which smoke, or coloured liquid, was being steadily carried down channel with the stream. If a model, however, be placed in position, the current is deflected by the obstacle, and the streamlines become pushed aside, forming a curved flow-pattern.

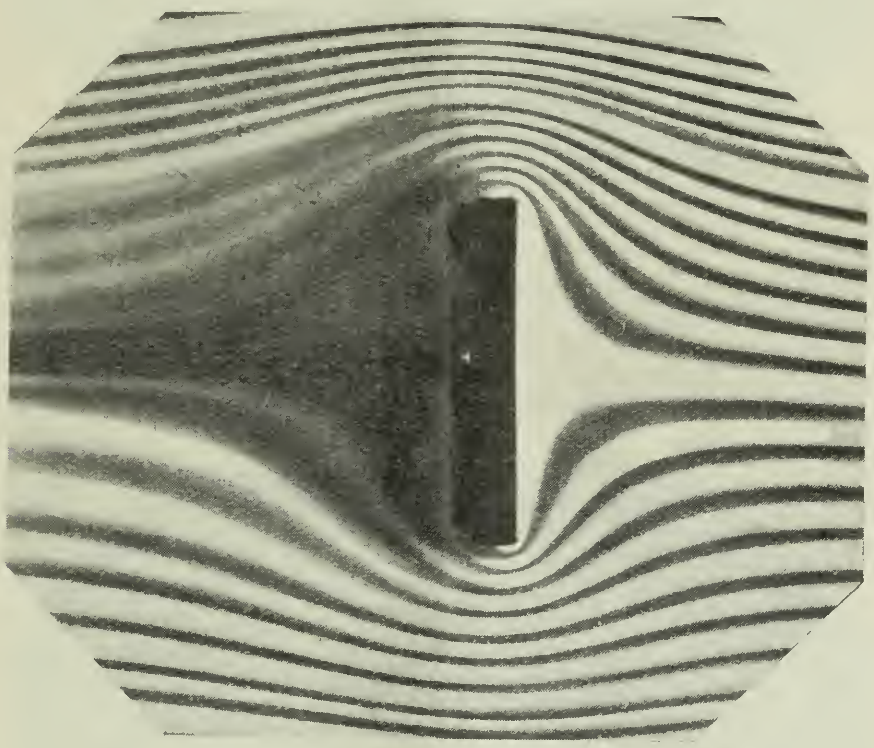


FIG. 3.
Flow pattern with flat plate (water).

Many important points concerning the nature of the flow can be illustrated by a careful study of these pictures. For instance, by regarding each streamline as steadily draining its own particular supply of colour, we should naturally suspect at places where the line widened a lower speed than at the narrow or constricted places. A glance at the flow pattern will, therefore, tell you how the speed changes from place to place in the current. Again, with some pictures, a tail of colour, practically motionless or stagnant, appears to have become trapped at the rear of the model. The extent of the stagnant region, which is called " dead," should tell something about the resistance of the model. With a square plate held normally across the stream (Fig. 3) the " dead " region is large, furnishing a high resistance. You will obtain similar effects, in a smaller degree, with a circular model (Fig. 4), or with an aeroplane wing section held at a large angle to the current (Fig. 5). When the wing section is inclined at only a small angle

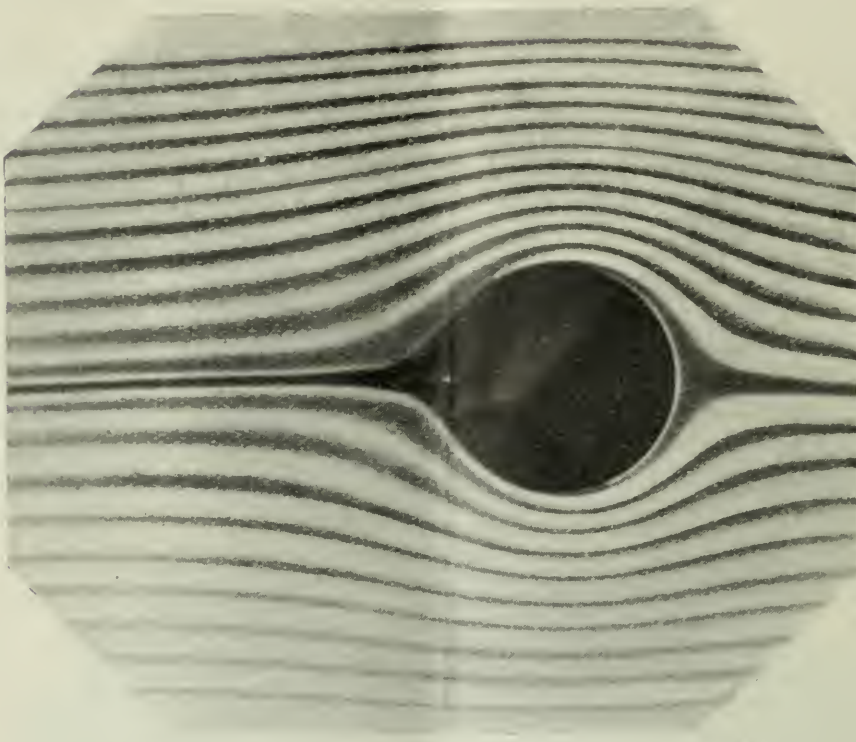


FIG. 4.
Circular cylinder (water).

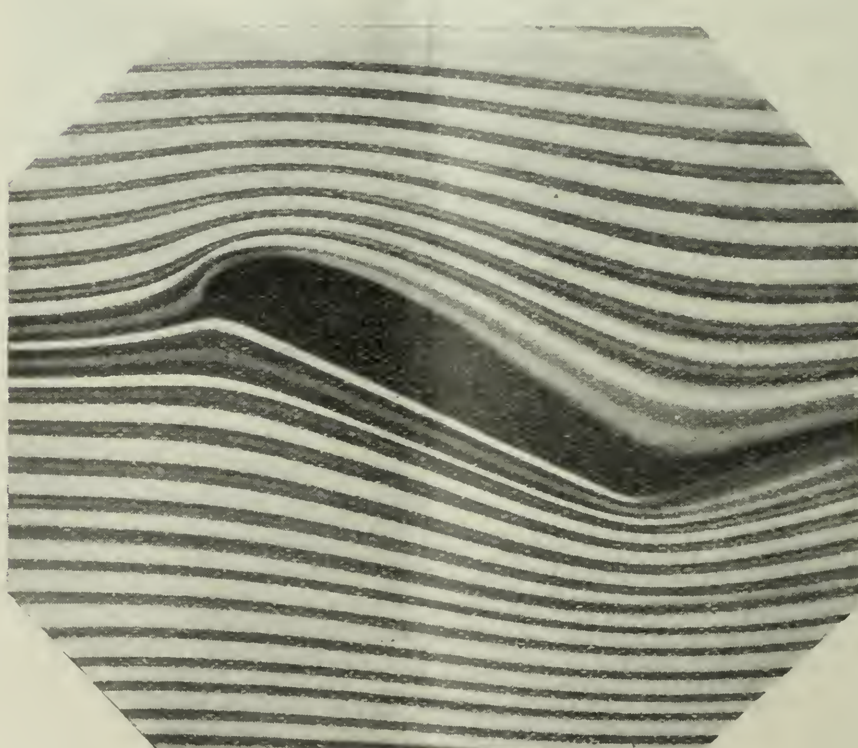


FIG. 5.
Wing section at large incidence (water).

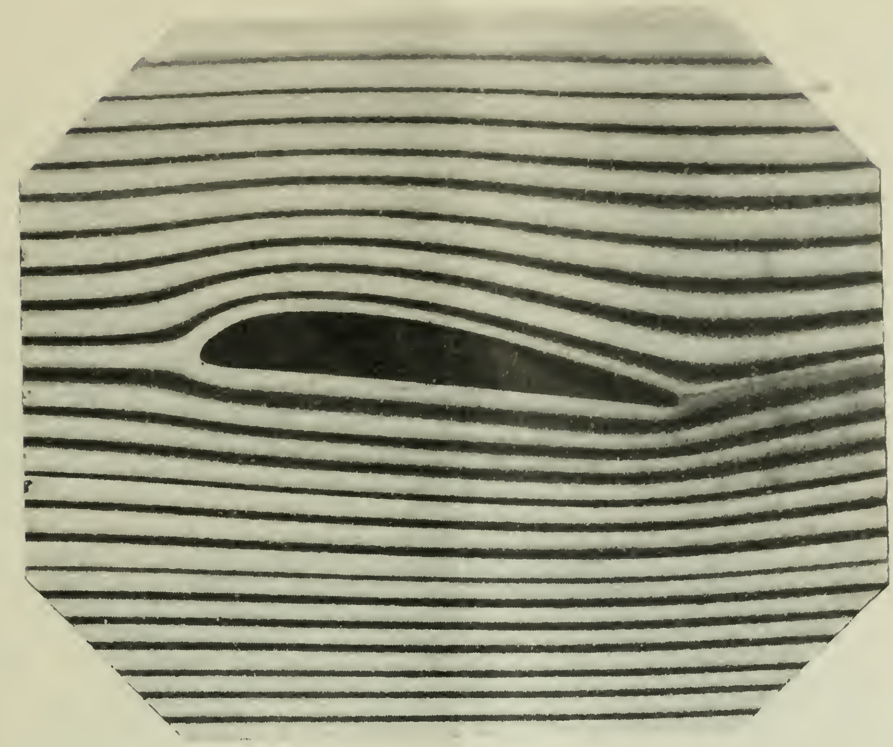


FIG. 6.
Wing section at small incidence (water).

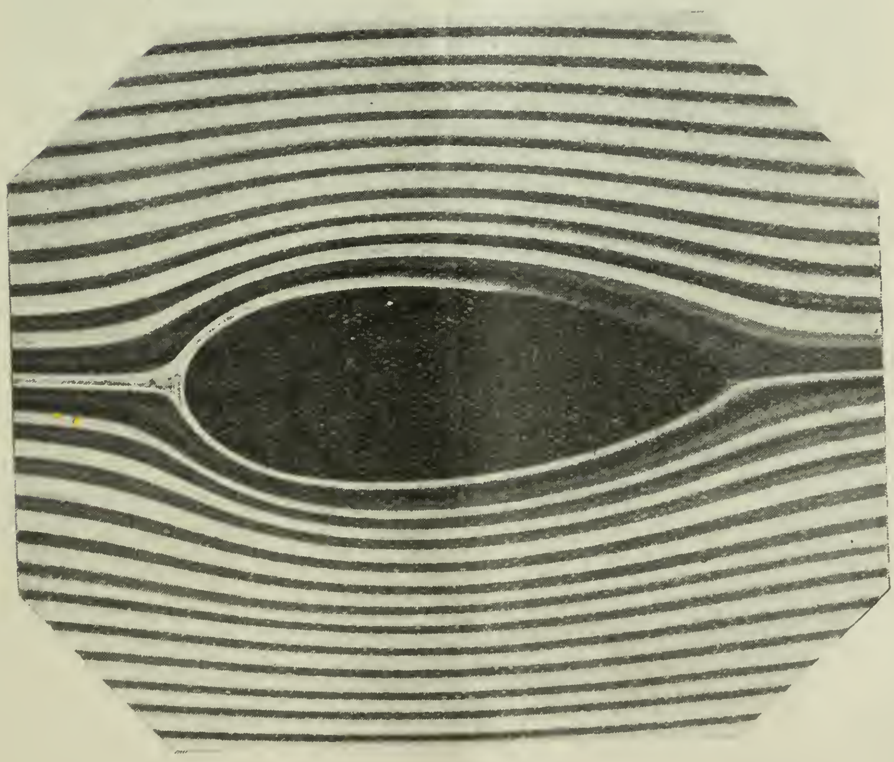


FIG. 7.
Strut section (water).

(Fig. 6) the resistance becomes much reduced, and the "dead" region scarcely noticeable.

Under certain conditions, many curious effects can be obtained. I shall only draw attention to one, which is most effectively shown with the water channel. It can be obtained best by first closing the water supply and allowing the channel to flood dark with colour. The water current is then allowed to run, and gradually wash the curtain of colour away from the model. A marked "halo" of perfectly clear water will soon develop round the nose of the model, showing up distinctly against the still deep colour of the main stream. The study of flow is rich in puzzling problems, and this "halo" is one of many for which you might care to try and think out an explanation.

If we were to make a careful collection of these different flow-patterns, we should find that every pattern possible with a given model in air could be repeated, although not necessarily at the same speed, with the same model placed in water.

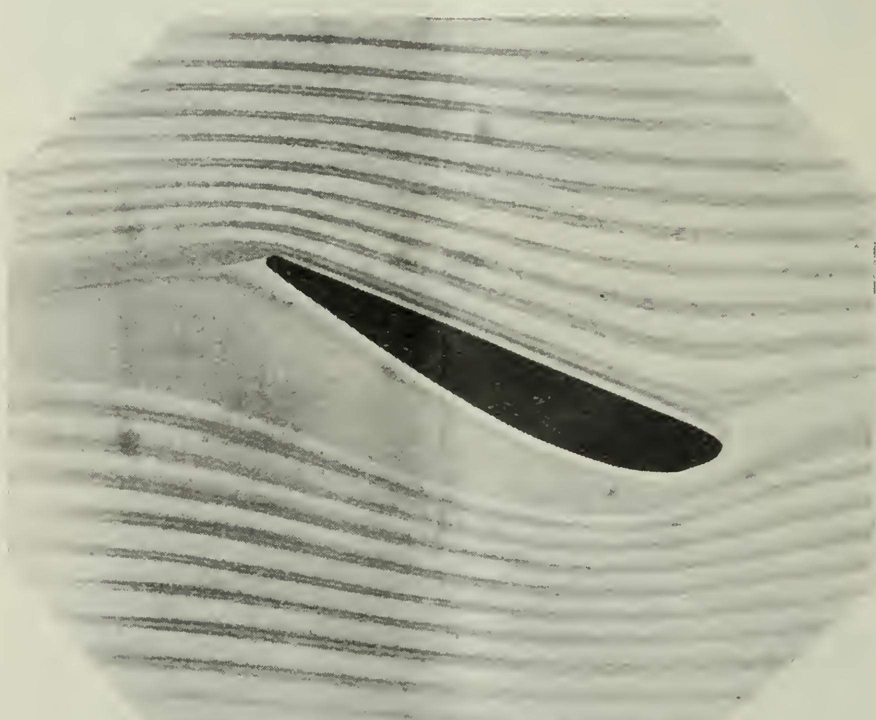


FIG. 8.
Wing section at large incidence (air).

In other words, our picture gallery for water would include every picture we could hope to compose in the air current (compare Figs. 5 and 8). This is only a simple case of a more general law which allows us to substitute in the above way one fluid for any other fluid.

We are, therefore, able to confine our attention to pictures obtained with one special fluid only, such as water. For a given *shape* of model, we could compile sets of pictures at different speeds, each set obtained with a different *size* of model. With a large model we would, of course, obtain large-sized flow-patterns, and with a small model correspondingly smaller patterns. But every pattern possible with the large model could be repeated in shape, although not necessarily at the same speed, with the small model. A sufficiently complete collection of pictures for the small model, would, therefore, enable us to predict results for the large model. Since the forces acting are directly connected with these flow-patterns,

some general law can be suspected connecting measurements on models of different sizes. A law of this nature does actually exist. It lies at the root of all wind-tunnel measurements, and provides for the designer the all-important link between the factors which enable him to design his machine and the measurements which can so conveniently be carried out in the wind-tunnel.

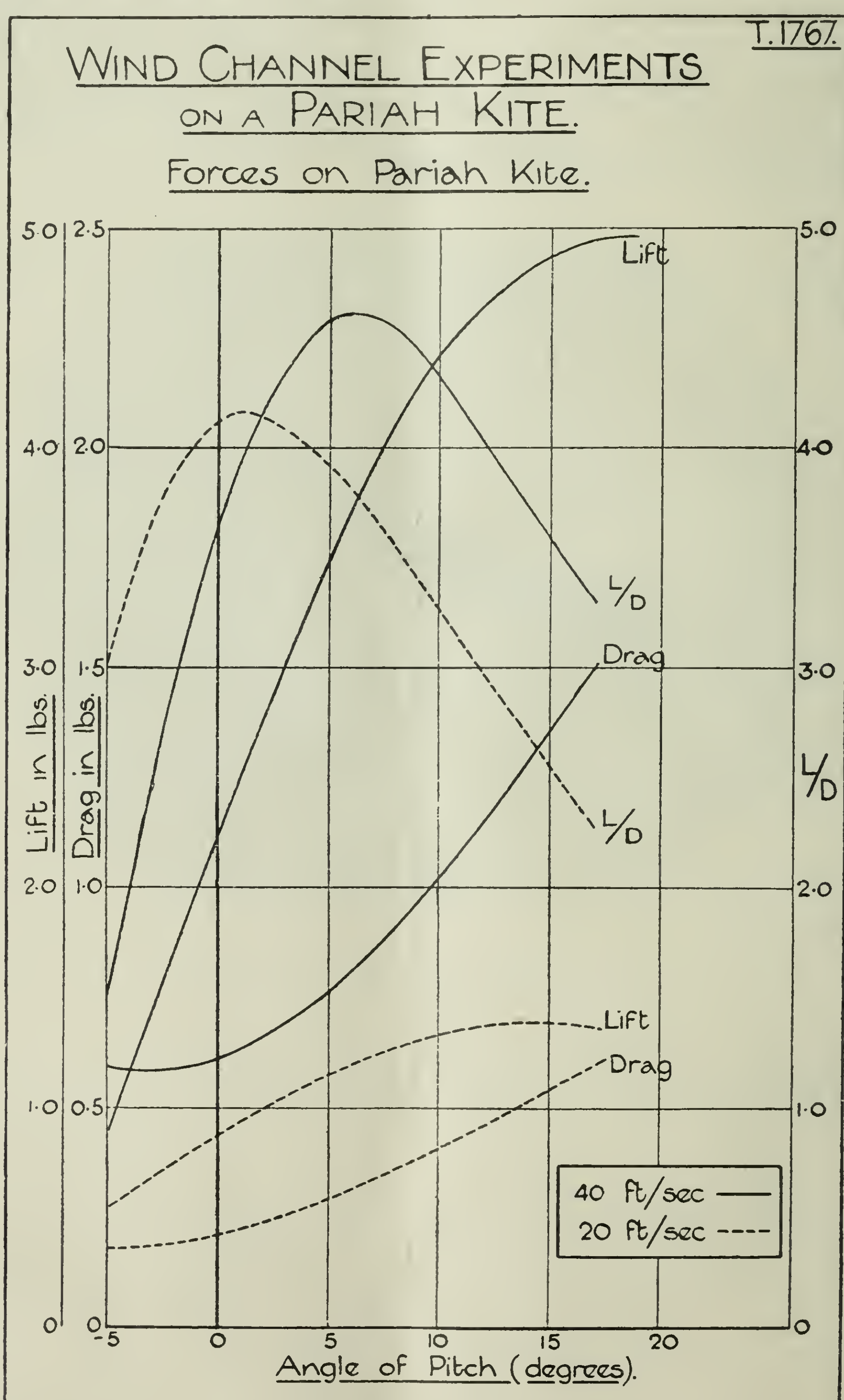
The points touched upon in the above written communication were illustrated by lantern slides and by experiment; a number of models were also exhibited. A 1ft. wind channel was shown in working order, simple force measurements and speed regulation being demonstrated. Model parachutes were dropped in illustration of types of instability. The formation of vortex rings and buds in water was dealt with by projection upon the screen, both plan and elevation views being shown. A number of representative flow-patterns were obtained in air and water, these pictures concluding the lecture.

(The lecturer is indebted to Mr. A. J. Webber, of the National Physical Laboratory, for his assistance in preparing the above lecture.)

WIND CHANNEL EXPERIMENTS ON A PARIAH KITE *

BY W. L. LE PAGE.

T.1541 contained a description of experiments carried out on an Alsatian swift, a bird of small dimensions and of a "flapping" species. It was thought desirable to repeat the experiments with a large bird and one belonging to the soaring species common in tropical countries.



* T.1767. Printed at the request of the Aeronautical Research Committee.

The pariah kite, or cheel, is reported to be a very efficient bird and, at the same time, one of the commonest of the class. The specimen obtained for these experiments had a span of 53¼in., and measured 24in. from the tip of the beak to the extremity of the tail. The chord of the wing varied from 10.75in. at the body to 9in. at the tips. A wooden former was arranged in the body for the purpose of taking a steel sting, which extended to the end of the tail feathers. The wings were externally braced by means of a strip of metal placed round the body and continuing along the undersurface of each wing for a distance of about one-third of the half-span on each side. This bracing device was concealed as much as possible in the feathers, as will be seen from the photographs,* which also show the symmetry of the bird to be moderately good.

The customary methods for testing aerofoils were adopted The bird was supported in a 7ft. channel by two wires from the main lift balance to the metacarpal joints of the wings, the sting being connected in the usual way to the rear vertical force and drag balance. The preliminary experiments were carried out with the bird in characteristic gliding attitude. Lift and drag forces were measured over a range of incidence, at wind speeds of 20ft./sec. and 40ft./sec. The incidence of the wings relative to the body was then increased, and a further test made.

It was thought that the most efficient disposition of the wings might not have been attained in this manner, and a few tests were therefore carried out with the body of the bird at the angle of minimum drag, the incidence of the wings alone being varied. The original results were, however, better, and only these are included in the present report.

Results

It will be seen from the appended table that results comparable with those of the Alsatian swift (T.1541) have been realised. The investigation generally confirms the poor results obtained in previous tests on birds. The maximum lift/drag ratio is 4.6, indicating a gliding angle of 12.2deg., which compares very poorly with that of recent gliders.

During the wind channel work it was noticed that the trailing feathers of the wings were being flexed upwards by the wind pressure; the reversed curvature started at a point approximately one-third of the chord from the leading edge, and was of considerable magnitude. This fact might account for the inferior results obtained, if the bird were known to have the power of keeping its feathers straight and unflexed, since it has been shown that reversed curvature in an aerofoil has a detrimental effect upon the maximum L/D ratio. (R. and M. 72, 1912-13).

TABLE I.
FORCES ON PARIAH KITE.

Angle of pitch (Degrees).	20ft./sec. Lift (lbs.).	20ft./sec. Drag (lbs.).	20ft./sec. L/D.	40ft./sec. Lift (lbs.).	40ft./sec. Drag (lbs.).	40ft./sec. L/D.
—5	0.542	0.180	3.01	0.890	0.595	1.50
—2	0.750	0.194	3.87	1.690	0.589	2.87
0	0.875	0.213	4.11	2.230	0.612	3.64
+2	0.995	0.241	4.13	2.745	0.659	4.17
4	1.102	0.275	4.01	3.245	0.724	4.49
6	1.195	0.314	3.81	3.715	0.807	4.61
8	1.270	0.359	3.54	4.115	0.906	4.54
10	1.327	0.408	3.25	4.415	1.024	4.31
12	1.372	0.460	2.98	4.645	1.149	4.04
14	1.390	0.513	2.72	4.810	1.287	3.74
16	1.385	0.571	2.42	4.915	1.429	3.44
17	1.365	0.599	2.28	4.940	1.501	3.29

* Not reproduced.

AERONAUTICA*

The Royal Aeronautical Society (7, Albemarle Street, Piccadilly) is endeavour ing to form, what it ought to have possessed long ago, a comprehensive library of books and prints on every phase of aerial flight. Thanks to a substantial grant from the Carnegie United Kingdom Trust, a very fine selection of the older books has been secured on satisfactory terms from Messrs. Maggs, who have made, as indicated in this column on January 22nd, 1920, a speciality of aeronautical books. The whole of the books and prints described in their catalogue issued three years ago was purchased *en bloc* by the late George D. Smith, of New York, and passed into Mr. Henry E. Huntington's collection. But duplicates of practically all the earlier books were obtained by Messrs. Maggs, and are now with many others on the shelves of the Royal Aeronautical Society, whose Secretary, Lieutenant-Colonel W. Lockwood Marsh, O.B.E., is also a keen private collector of such books. Important collections of books on aeronautics may be found in the British Museum, the Patent Office, and in the Science Library of the Victoria and Albert Museum, but it may be doubted if any of these libraries could produce anything like all the titles registered in Tissandier's "Bibliographie Aéronautique," Paris, 1887; and since that date a vast mass of printed and artistic matter has been issued not only in Europe, but also in America and elsewhere. Aerial navigation has undoubtedly come to stay; and as very few of the earlier books on the subject were printed in large numbers, they have all become rather rare. The ever-increasing number of those who collect such books, public institutions and private individuals, has naturally much enhanced market values. The extraordinary variety and interest of the subject are fully demonstrated in Tissandier's sumptuous "Histoire des Ballons et des Aéronautes célèbres," 1783-1880, and in the Comte de la Vaux's equally splendid continuation volume recently published.

Which was the first published treatise on navigation in the air? The idea itself was much older than Blanchard, Montgolfier, and other modern pioneers, for it occurred to the Persians nearly two thousand years ago. Messrs. Maggs have a page from a Persian MS. in which a king is represented shooting at birds from a floating car. One of the earliest books in the Royal Aeronautical Society's library is the first of several editions of Francesco de Lana, "Prodomo overo saggo di alcune inventioni nuove," etc., a folio printed at Brescia, 1670, with twenty fine plates, including one of the "aerial ship." This is well worthy of being described as the First Folio of aeronautics; a translation of the fifth and sixth chapters was printed by the Society in 1910. A counterblast, perhaps not the first, to Lana appeared at Rovereto in 1753, in Cavalcaho's "L'Impotenza del Demonio di trasportare a talento per l'aria da un luoga all'altro i Corpi Umani," in which the author declares that "the atmosphere has always been unknown to man, and will continue to be a region unknown to him. No one, not even the Demon himself, has the power to teach man any method by which he may explore that region, either by increasing his motive power or by diminishing very considerably his specific gravity." Another early book, also in the library, is Antonio de Fuenta la Peña's "Discurso," "El Ente Dilucidado," Madrid, 1677, a substantial quarto of over 500 pages, and the subject of an article by Señor Vicente Castañeda y Alcover in the "Revista de Archivos, Bibliotecas y Museos,"

* Reprinted from the "Times" literary supplement.

1916, with the title "El Primer libro impreso sobre Aviación ¿ es Español?" Another early author is J. A. Borelius, "De motu animalium," Rome, 1680-1, which went through several editions, and is remarkable in having a chapter, "De Volutu," on the theory of the flight of birds and on aviation, in which the author comes to the conclusion, "Est impossibile ut homines propriis viribus artificiose volare possint." This section of the book was translated and printed by the Society in 1911, with copies of the curious diagrams—Leonardo da Vinci's earlier work on the subject of the flight of birds was then unknown and was undiscovered until the end of the last century. Earlier than any of these is the Veranzio, "Machinae Novae Fausti Verantii Siceni," a folio published at Venice about 1595, with an engraved title and woodcut border showing the flight of Daedalus and the fall of Icarus, and containing on p. 13 the first description of a parachute.

The idea of aerial navigation was in the minds of men, scientific and otherwise, throughout the seventeenth and eighteenth centuries, long before ballooning became an accomplished and practical fact. The Marquis of Worcester, in his "Century of the Names and Scantlings of such new Inventions," London, 1663, wrote of "how to make a man fly; which I have tried with a little boy of ten years old in a barn, from one end to the other, on a hay mow." Three years later we have a romance from the pen of Francis Godwin, Bishop of Hereford, "L'Homme dans la Lune," Paris, 1666 (a new edition appeared in 1671), of which the hero flies to the moon with the help of an apparatus which is carried through the air by ten "Gansas," wild geese or swans. Robert Paltock's "Life and Adventures of Peter Wilkins," 1751 (translated into French and issued in Paris in 1763) is a well-known air book, and may have suggested Dr. Johnson's chapter "A dissertation on the art of Flying," which appears in "The Prince of Abbissinia (Rasselas)," 1759. In the interval we have, among other things, Samuel Brunt's "Voyage to Cacklogallinia," London, 1727, with a curious plate of the author being carried through the air in a flying machine supported and escorted by cocks. In the region of romance also, there is another interesting early English item, "The Scribleriad: an Heroic Poem in Six Books," London, 1751, with several plates, one depicting an aerial combat between an Englishman and a German, a rarity not known to Tissandier or any other modern bibliographer of aeronautical books.

The most prolific period of printed matter concerning aerial navigation dates from 1783, and in connection with this mention may be made of Joseph Galien's "L'Art de Naviguer dans les Airs," Avignon, 1757, an excessively rare book, in which the author suggests filling large cloth balloons with rarified air of the high regions, gathered on mountain tops—probably the first real instance of the mention of balloons.

REVIEWS

Les Hélicoptères

Par W. Margoulis. Paris: Gauthier-Villars et Cie., Quai des Grands-Augustins, 55. (pp. xi. + 90.)

To anyone who is concerned with the problem of the helicopter this volume must be of considerable interest, if only because it is, so far as the writer is aware, the first technical book devoted entirely to the subject. Certain portions of the work have previously appeared as a technical supplement to "L'Aéronautique." The first part of the book is given up to the exposition of experimental results obtained with model airscrews and is of special value in so far as a wide range of V/nD, for positive and negative values, is covered, with the plane of rotation of the screw making various angles from $+90^\circ$ to -90° with the direction of the wind. At the same time one wishes that the experimental results had been presented more completely in tabular form and that M. Margoulis had extended his researches to cover a smaller pitch/diameter ratio. A chapter is devoted to the special study of screws working without forward motion. Here, quoting experiments of Durand and Lesley (U.S. Report No. 30), the author points the interesting result that "the thrust of a two-bladed screw, when at rest axially,* depends only on the diameter, the rotational speed, and the power expended, whatever may be the section, the blade width, the plan form, the pitch and its change along the radius."

In the second part of the book helicopter flight is considered and the author deals with the general case of a machine fitted with both sustaining and propulsive screws. In the case of vertical descent, with the motors running or shut off, there appear to be three possible conditions giving the same speed of descent, two of which are stable and one unstable. On the subject of free descent with engines shut off, the author does not seem very optimistic. Horizontal and oblique flight are also treated at length.

To English readers the greatest drawback is the frequent use of nomographs and Kith's logarithmic curves, the use of which is not so common here as in France.

The Internal Combustion Engine

By Harry R. Ricardo, B.A., A.M.I.C.E., M.I.A.E. Volume I. Slow Speed Engines. (Blackie and Son, Limited.)

As a rule, the more one studies the technical problems of aeronautics the more cautious one is likely to be in predicting the future. The amateur sees visions while the expert does the work. It is refreshing to find that Mr. Ricardo combines the enthusiasm of the amateur with the knowledge of the expert. "It is rather to the air that we must look for the influence that the internal combustion engine is destined to exert on the civilisation of the future. However great the progress in motor cars, in stationary engines and in marine engines of

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* *Au point fixe.*

the internal combustion type, it is in connection with air locomotion that the internal combustion engine will probably play the most important rôle."

These words are taken from the introduction to the first volume of Mr. Ricardo's treatise on the internal combustion engine. The author is well known for his work on the high speed engine. He is one of a small band of engineers who, by their mechanical genius and scientific insight, have developed the high speed engine in this country to a degree unsurpassed, and perhaps unequalled, by any other nation. More than ordinary interest therefore attaches to this book in which Mr. Ricardo expounds and illustrates his beliefs and methods.

The first volume is devoted to the study of slow speed engines. The opening chapters deal with general thermodynamic considerations on accepted lines. Then follow some suggestive and original chapters on mechanical and volumetric efficiency and on general principles of design. The author's treatment of the problem of mechanical losses is particularly interesting; he rightly points out that in practice these losses are just as important, and perhaps more so, than thermal losses; yet far more scientific research has been directed to the improvement of thermodynamical than to that of mechanical efficiency. In the rest of the book examples of different types of slow speed engines are considered in detail, and thus serve to illustrate the general principles developed in the earlier chapters.

Most readers will agree with the author that the gas engine has seen its best days. In small sizes it cannot now compete with the electric motor supplied with current from a central station; and in large installations it has been beaten by the steam turbine. Not everyone will, however, share his views on the Diesel engine. His analysis of the technical advantages and disadvantages of this type of engine is clear and convincing; but when he compares the commercial value of Diesel engines, gas engines and steam turbines, we think he is too much influenced by pre-war conditions. Further, though the Diesel engine has not been developed to the same degree as the "petrol" engine since 1914, it has not stood still; and the author has perhaps hardly given sufficient consideration to recent developments. We should have liked, in particular, to have learnt his views on the proposed Diesel steam combinations, which utilise exhaust heat.

For those interested particularly in aircraft engines, this book will serve mainly as a general introduction to the second volume, dealing with high speed engines, to the appearance of which we shall look forward with great interest. There are comparatively few misprints in the first volume; its worst fault is the absence of an index. Even a list of illustrations would have helped; then perhaps we should not have spent so long in finding Fig. 15, owing to a misprint on p. 51. Perhaps a complete index to the two volumes will be given in the second volume. We hope so; if not, it will not be easy to devise a punishment to fit the crime. Something with boiling oil in it will certainly be necessary.

Who's Who in Engineering, 1923

Compendium Publishing Co., London.

The third edition of this reference book has been improved out of all knowledge compared with its predecessors, and now constitutes a very valuable contribution to the shelf containing those books which, though undoubtedly coming under Charles Lamb's stricture of being "*biblia abiblia,*" are nevertheless a necessity for office purposes. The list of engineering institutions seems to be exceedingly complete, and is made more useful by the inclusion of such bodies in the British Dominions. Another feature which may be commended is the alphabetical list of abbreviations in use to denote the various grades of membership of technical bodies, as these are so frequently given incorrectly, even at times by the

individuals concerned. How often, for example, has one seen a member of the Institution of Civil Engineers described as "M.I.C.E."? The classified register of engineers appears to be tolerably representative so far as aeronautical engineering is concerned, though it is naturally not by any means exhaustive. To the ordinary person connected with aeronautics this reference book is probably more useful than the ordinary "Who's Who," and seems to be, on the whole, more complete than Whitaker's Almanack. In fact, if only one work of this sort is to be kept, this is probably the best for engineers.

THE JOURNAL

OF THE

ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

No. 148 APRIL 1923 VOL. XXVII

NOTICES

Election of Members

The following members were elected at a Council Meeting held on March 20th :—

Foreign Member.—F. W. Follett.

Student.—John M. Radcliffe.

Silver Medal

The Council have awarded the Society's Silver Medal for 1922 to Lieutenant-Colonel E. W. Stedman, O.B.E., for his paper on "Some Technical Aspects of Aviation in Canada" published on page 349, Vol. XXVI., of the Journal.

Associate Fellowship Examination

The next examination for Associate Fellowship has been postponed from April to September next. The exact date will be announced later.

R38 Memorial Prize

The following members have been appointed to adjudicate on the papers submitted for the first award of the R.38 Memorial Prize :—

Professor L. Bairstow, C.B.E., F.R.S.
Wing Commander T. R. Cave-Browne-Cave, C.B.E., R.A.F.
Professor A. J. Sutton Pippard, D.Sc.
Major R. V. Southwell.

Students' Section

It is proposed to hold another Smoking Concert at the Engineers' Club, Coventry Street, W.1, at 6.45 p.m. on Friday, April 27th. The students hope that members of other grades will join them on this occasion. Tickets (price 1s. 6d.) may be obtained from the Secretary.

Students are invited to send in offers of papers to be read at Students' Meetings during next Session (commencing in October) to the Honorary Secretary, Students' Section.

Students' Visits

Thursday, April 19th.—Marconi Works, Chelmsford. Meet at Main Line Booking Office, Liverpool Street Station, at 1.55 p.m., for cheap travelling facilities.

Wednesday, April 25th.—Napier Engine Company, Acton. Meet at the Company's Works at 2.50 p.m.

Saturday, May 5th.—Vickers Ltd., Weybridge. Meet at Main Line Booking Office, Waterloo Station, at 9 a.m.

Those desirous of attending these visits should send in their names to the Honorary Secretary, Students' Section, at least four days beforehand in each case.

Forthcoming Arrangements

Tuesday, April 17th.—Council Meeting.

Thursday, April 19th.—Students' Visit to Marconi Works, Chelmsford.

Wednesday, April 25th.—Students' Visit to Napier Engine Co., Acton.

Saturday, May 5th.—Students' Visit to Vickers Ltd., Weybridge.

Thursday, May 31st.—5.30 p.m., Royal Society of Arts. *Wilbur Wright Lecture.* Dr. J. S. Ames, "The Relation between Aeronautical Research and Aircraft Design."

Correction

In R. A. Frazer's Juvenile Lecture in the March number, Figs. 3, 4 and 8 were unfortunately printed upside down.

W. LOCKWOOD MARSH, *Secretary.*

PROCEEDINGS

Seventh Meeting, 58th Session

An Ordinary General Meeting of the Society was held at the Royal Society of Arts, John Street, Adelphi, London, on Thursday, January 18th, 1923, the Chairman, Professor Bairstow, in the chair.

The Chairman, after introducing Major J. D. Rennie, who was to read a paper entitled "Some Notes on the Design, Construction and Operation of Flying Boats," said the Paper was a very full one. In this connection he pointed out that the Council of the Society welcomed full papers of this description which were suitable for publication in the Journal of the Society, and that it was not necessary for authors, in preparing their papers, to make them so short that they could be delivered within the limits of a lecture. The Society welcomed Major Rennie, who had a unique experience of the technical side of the development of the British flying boat. He had been associated with the late Colonel J. C. Porte, C.M.G., at Felixstowe, who was chiefly responsible for the early development of the flying boat in the British Air Service. He (the Chairman) also welcomed the Paper, because, to some extent, it challenged outside designers as against Air Ministry designers, and in issuing the challenge—quite a moderate one—the Author had taken care to provide anyone who wished to reply with the necessary material. That, again, was good for the development of the subject. It was not usually a sign of good health that one should take peacefully and quietly all that one was told.

Major Rennie then delivered his Paper, which he illustrated with a number of lantern slides.

SOME NOTES ON THE DESIGN, CONSTRUCTION AND OPERATION OF FLYING BOATS

Introductory

Since the Armistice, the development of the flying boat in this country has been practically at a standstill; this for two main reasons. Firstly, the unfortunate death of the late Col. J. C. Porte, C.M.G., who was directly responsible for the development of the flying boat in the British Air Service, robbed the aeronautical world of one of its greatest pioneers in this branch of aeronautical engineering; secondly, as about 99 per cent. of the flying boats used on active service during the late war were designed, and the first of each type constructed and tested, at the Seaplane Experimental Station, Felixstowe, where the late Col. Porte was Commanding Officer, and the lecturer was for some time Chief Technical Officer, private firms had not the opportunity to gain the necessary practical experience obtained from extensive flying operations carried out under all sorts of conditions and weather.

The result of this lack of experience, which could only be acquired by private firms at a prohibitive cost, has been that flying boats produced by them during and since the war, with possibly one exception, have proved unsuccessful, and in several cases complete failures.

Such being the case, the chief object in writing this paper is to attempt to put on record the main results of the experience in flying boat design and construction obtained at Felixstowe and elsewhere, and to compare the merits and demerits of the other known types of hull construction with that developed there.

At the same time, in order to make the paper more or less complete, it is proposed to give a brief discussion of the various problems in design, apart from the hull, which have arisen from time to time and present difficulties unique to the flying boat.

Originally it was intended to treat in all its aspects the problems connected with the design, construction and operations of flying boats, but at the request of the Society such matters as model testing, operations, and others likely to be dealt with in the forthcoming lectures of Baker and Wing-Commander Cave-Browne-Cave have been dealt with less completely.

Some General Considerations

It is probably true to say the flying boat is the least known of all types of aircraft, very likely due to the fact that during the war their operations, mainly submarine patrols and ship escorting, were conducted from a few seaplane stations on the coast, and the Orkney and Scilly Islands. Little limelight was given to their achievements. In this connection, it may be mentioned, as a matter of interest, that flying boats operating from Felixstowe were responsible for the sinking of thirteen German submarines and the destruction of one Zeppelin. Also, during a period of eighteen months' operations from two seaplane stations, 289,000 sea miles were flown without the loss of a single boat, apart from enemy action, providing sufficient testimony to the sea-keeping qualities and airworthiness of these boats.

Again, commercial services so far have been confined to routes over the land or at most covering short stretches of sea, such as the English Channel, where no advantage would be gained by the use of flying boats. When, however, these services become firmly established and the consequent extensions entail long flights over the sea, the use of flying boats will become imperative.

It would seem as if such operations will be confined mostly to countries, as our Colonies, where there is an extensive coast line, or broad rivers extending far inland, or vast tracts of virgin forests with inland water, as in Canada, and where, under these circumstances, aerodromes are impracticable.

With the possible exception of to Norway and Sweden, the prospect of establishing long sea routes from this country are not hopeful at present. In the not too far distant future regular services to America will probably be available for those to whom the saving in time will warrant extra expense over the steamship. Such route would most likely be via the Azores Islands in the Atlantic.

Over long sea routes, say, 100 miles and upwards, the flying boat possesses overwhelming advantages over its only rival, the steamship, contrasted with the aeroplane competing with the train. In the former case, taking the maximum speed of a ship as 20 knots and the cruising speed of a flying boat as 80 knots, the voyage would be accomplished in a quarter of the time; in the latter case, the average speed of the train might be about 40 miles per hour and cruising speed of aeroplane 95 miles per hour, only slightly over half the time.

With regard to the effect of weather on operations, the conditions are more favourable to the flying boat. Where the operations are carried out from sheltered harbours the flying boat can take off in weather in which it would be dangerous for the large aeroplane to make the attempt. Night flying is also safer, as in case of emergency a safe landing can always be made, even with a moderately rough sea running, when the trouble may be located, and if possible repaired; otherwise the boat may be taxied so long as one engine is available to the nearest seaplane station, sheltered water, or steamship route.

In the event of fog, should it be necessary to land, this may be achieved reasonably safe (provided hull bottom is properly shaped) by means of a simple method of automatic control, which was successfully used early in 1915, and has

since been considerably improved. A point worth mentioning which is not generally realised is that new types of flying boats, up to any size likely to be built in the future, may be tested with safety before taking off the water to any extent. Choosing suitable weather and sea conditions, it is possible to plane at speeds below and well above the lowest flying speed of the boat, and then gradually to take off and fly over as much of the speed range as desirable while only a few feet off the water, thus enabling controls, trim, etc., to be sufficiently tested before actually gaining a height, such if trouble arose a dangerous situation might arise. This, coupled with the fact that should one engine cut out suddenly when taking off the consequences are not likely to be serious, the converse being the case with a multi-engined aeroplane, accounts for the total absence of any serious accident during the testing of experimental flying boats at Felixstowe.

For the above two reasons alone it would seem logical that the most suitable type of aircraft to develop in large sizes is the flying boat.

It has been remarked frequently by critics of the flying boat that in performance and general design it is inferior to that of the modern aeroplane.

Obviously, with regard to performance, this must be true of the single-engine flying boat, owing to the interplane engine mounting—an undesirable feature for which there would seem to be no practical alternative at present. Also the aerodynamical and detail design and general lay-out unfortunately is much inferior to that represented in the best modern aeroplane practice for no apparent reason other than lack of experience and ability on the part of the designers. The above, however, cannot be said of the multi-engined flying boat, as a few simple considerations may show. For the purpose of aerodynamic comparison, the trials of the F.5 flying boat and the Vickers Vimy commercial aeroplane carried out at the Air Ministry test stations at Grain and Martlesham respectively have been analysed. Both machines are fitted with two Rolls Royce Eagle VIII. engines, and are of approximately the same weight and class. Taking as a basis of comparison the lbs./h.p. carried at a speed of 95 m.p.h. at 2,000ft. with a loading of 9 lbs./sq. ft., the figures are 21.2 for the F.5 and 17.4 for the Vimy. The former had bare engines and the latter faired nacelles. The greatest uncertainty in the above analysis was the engine-power curve, as tests were not carried out at the time of the trials. The curve used was an average one from a large number of A.I.D. tests.

That it is quite reasonable to expect a result of this order may be seen from other considerations :—

TABLE I.

BODY RESISTANCE.

	Drag at 100ft./sec. Max. cross sectional area.
Streamline $4\frac{1}{2}$ fineness ratio	0.66
P.5 flying boat hull	1.38
F.5 flying boat hull	1.53
Bristol Pullman fuselage	1.90

In Table I. are compared the air resistance of the F.5 and P.5 flying boat hulls and the Bristol Pullman fuselage. The resistance of a good streamline form is also given as a matter of interest. The figures given are for model only, without wing interference; but assuming the effect is similar, it shows that a hull can be designed to have as good, if not a better, low resistance form in spite of the limitations imposed on the shape by hydrodynamical requirements than the corresponding aeroplane fuselage.

TABLE II.

PERCENTAGE WEIGHTS.

Aeroplane.	Gross Weight. lbs.	Lbs./sq. ft.	Lbs./H.P.	Wing Structure.	Tail Unit.	Fuselage and Tail Skid.	Chassis, or Wing Tip Floats.	Plant.	Supplies.	Structure.	Useful Load.
H.P. 0/400	13,200	8.00	18.4	16.4	1.5	11.4	5.5	22.3	2.5	34.8	40.4
H.P. 4.8 B.	11,553	7.92	16.32	17.8	1.54	12.3	4.36	21.64	1.37	36.72	40.2
H.P.V. 1500	30,000	10.00	20.7	17.45	1.71	5.7	4.50	19.11	2.01	30.06	48.9
Bristol Tramp	18,795	8.25	19.6	21.09	1.43	7.38	4.04	30.17*	2.16	35.82	31.8
Vickers Vimy	11,120	8.40	15.45	17.55	2.1	13.0	3.45	29.4	1.9	36.1	30.8
										Average	38.5
Flying Boat.											
P.S.B.	32,000	10.2	18.5	16.5	1.7	12.8	0.75	19.0	4†	33	44
F. 5	13,000	9.1	18.6	16.0	2.2	15.7	0.92	20.0	3	36	41
P. 5	12,055	9.4	17.02	15.4	3.1	15.5	1.22	22.56	2.41	35.8	39.3
Short Cromarty ...	19,633	8.7	18.7	19.01	2.21	14.7	0.80	22.93	2.0	37.32	37.8
										Average	40.5

NOTE.—Wing structure includes interplane engine mounting.
Fuselage does not include controls or instruments.
Hull includes wing roots, seats and bulkheads, but not controls or instruments.
Useful load includes fuel and oil.

* Includes engine shafting, couplings and gearing.

† Includes tankage for 1,500 gals. of petrol and 100 gals. of oil.

Further, at top speed the air resistance of a fuselage and tail skid is of the order of 12 per cent. and the undercarriage from 10 per cent. to 15 per cent., making a total of from 22 per cent. to 27 per cent. of the total resistance, whereas the resistance of hull, wing tip floats and wing root struts does not exceed 10 per cent. of the total.

Table II. gives some per cent. weight data of a number of aeroplanes and flying boats of approximately the same performance and class, which, with the notes attached, needs no further explanation. The main point to note is that the average useful load of the flying boat is 40.5 per cent. against 38.5 per cent. for the aeroplane.

TABLE III.

Aeroplane.	Weight lbs.	A Area Main Planes.	c Chord M.P. ft.	Area Tail Plane. sq. ft.	l. ft.	Tail volume ratio. $\frac{a_t \times l}{A \times c}$	Type of Tail Plane.
Victoria ...	17,700	2166	13′ 0″	214	31.5	0.239	Biplane.
Awang ...	17,015	2301	11′ 9″	229	41	0.339	,,
Braemar ...	16,500	1905	8′ 6″	180	36	0.395	,,
H. Page ...	13,200	1645	10′ 0″	184	44.9	0.5	,,
Vickers Vimy..	12,500	1329	10′ 6″	180	31.0	0.4	,,
Bolton ...	9,100	928	7′ 9″	100	27	0.376	Monoplane.
De H. 10 ...	8,500	851	7′ 0″	109	21.5	0.394	,,
Flying Boat.							
P.S.B. ...	32,000	3108	10′ 0″	378	33.75	0.41	Biplane.
N.4	32,000	2675	12′ 6″	368	37.7	0.41	,,
F.5	13,000	1432	8′ 0″	186	24.5	0.39	Monoplane.
P.5	12,055	1273	9′ 0″	196	25.0	0.42	Monoplane R.A.E. 15 inverted.
Valentia ...	20,650	2025	10′ 9″	296	29.5	0.4	Biplane.

Table III. gives some particulars of tail volume ratio and dimension l, the distance between the C.G. of the whole machine and C.P. of tailplane.

It will be noticed that the dimension l is smaller in the flying boat than in the aeroplane, while the chord of main planes in the former case is generally smaller, the tail volume ratio being of the same order in both types.

This dimension might be increased, but only at the expense of a considerable addition in hull weight, and as experience shows it does not seriously affect the stability or performance, the increase is not warranted. As will be shown later, l is not fixed from purely aerodynamical considerations.

Making due allowance for a certain amount of inaccuracy in the above data and tables, it is fair to say that a multi-engine flying boat can be designed to give as good a performance as the corresponding aeroplane, if not even better.

There are three types of flying boats in common use :—

(1) The more orthodox, of which the F.5 and P.S.B. are typical. It consists of a hull to which the wing structure and tail unit are attached direct. Interplane engine mounting and wing-tip floats.

(2) In this type the hull is shorter than in type (1). It is cut off at the rear step, and tail unit carried on outriggers. The American N.C.4 and R.A.E. C.E.1 are examples.

From the aerodynamic point of view, this type offers the best solution for a single-engine flying boat, as it is possible to place the fin and rudder symmetrically with respect to the rotational component of the propeller slipstream, thus

eliminating the necessity to carry rudder. Also, as the tail plane is higher up than in type (1), it is clear of wave formation set up when taking off.

(3) To avoid the necessity of using wing-tip floats, especially in the case where a cantilever monoplane wing placed relatively to the hull about where the top plane of normal biplane arrangement would be, is employed, the fin has been extended outwards symmetrically with respect to the C.G. of the boat in order to obtain a positive metacentric height. The extension may be either parallel to the water line or form a small cantilever wing set at a large dihedral angle, thus providing an increasing righting couple with angle of hull. It also contributes to the aerodynamic lift in flight.

It is with type (1) that this lecture is concerned, and in particular multi-engined boats having a displacement not less than 4,500 lbs.

Below this displacement the float seaplane would appear to have the advantage except under most exceptional circumstances, as when operations are carried out from sheltered inland waters. Unless the hull is much larger than other requirements would indicate, the pilot and passengers are liable to get a good wetting when taking off in a short sea causing great discomfort during the flight, especially in cold weather. On the other hand, if totally enclosed, there is a serious element of danger, owing to the flinging up of spray and solid water on the windows, which renders clear vision most difficult at a time when it is all-important.

Aerodynamic Structure

The usual biplane wing arrangement of a flying boat differs in at least two respects from that of the large aeroplane. The span of the top plane is considerably greater than that of the lower. This was not intended primarily to obtain greater aerodynamic efficiency owing to the absence of biplane interference on the extensions, but because it is advisable to have as small a wing area as can be conveniently arranged in close proximity to the water, thus minimising risks of danger in a rough sea. For the same reason ailerons should not be fitted to the lower plane. To make up for this, and to ensure adequate lateral control, the ailerons should be placed as far out board as possible.

Also, paying due regard to increase of dimensions and consequently weight of the wing-tip floats, the closer they are to the hull the less shock is transmitted to the wing structure when the boat is rolling, taking off or landing, one wing down. The result of these considerations led to the adoption of the wing arrangement as described.

In the case of the "Fury," which was a triplane, ailerons were fitted on the top and middle planes only. The latter were of equal span, and greater than that of the lower plane. It has been said frequently of the "F" boats that the use of stabilisers on the top plane is a very inefficient aid to lateral stability. They were never really intended as such, as it was well known an increase of dihedral would be much more effective. The top plane extensions had to be braced to withstand down loads which are applied as an air load at high speed flight as its own weight or as inertia loads caused by a bad landing. The rectangular cross-braced pylon was considered to be a more sound job than the triangular type, and was thus adopted. It was completely faired in, thus giving less air drag than would be given by the sum of the component parts.

While extensions have the disadvantage of requiring a large number of bracing wires, on the other hand, if the planes were of equal span, another set of interplane struts and wires would probably be required if normal wing sections were used.

In order to keep the propellers reasonably clear of spray or solid water, the distance of the line of propeller thrust from the C.G. is much greater than that of the aeroplane, in which by mounting the engines on top of lower planes this line may be made to pass nearly through the C.G. While disadvantageous from some points of view, it is not without its compensations. It is helpful to stability, and especially to absence of change of trim, engine off and on. By arranging the C.G. well forward, which means good longitudinal stability, other things being equal, there will be a down load on the tail over the speed range, which by suitable aerodynamic design will be further increased by the propeller slipstream, thus balancing the propeller thrust moment.

If accurate data were available as to the angle, the increase of velocity and the boundary of the downwash at the tail plane, a simple calculation would fix the range of adjustment and the position and area of the tailplane in the propeller downwash to obtain the necessary balance at at least one speed, say, cruising speed, and a fair compromise over the remaining speed range. Unfortunately this data is not known with sufficient accuracy.

Various formulæ have been devised, the best known being that given in R. & M. 326. Wind tunnel investigations have shown this formula may be an error to the extent of 2 degrees, under-estimating the deflection. Further, the same test showed that in the case of a tractor propeller the angle of pitch relative to the main plane chord had little effect on the angle of downwash on the tail plane. Mr. R. McKinnon Wood has suggested that the increase in the angle of downwash is due mostly to the increased speed of the air over the portion of the wings in the slipstream, thus increasing the lift, of which the downwash is a function, a view which receives considerable support from the Prandtl theory of circulation.

Again, the formulæ derived so far neglect the effect of the rotational motion of the air in the slipstream. For example, in the "F" boats the change of incidence of the tail plane to trim at the same speed, when for two engines turning in opposite directions were substituted two turning in the same direction, was of the order of ¾deg., which gives some idea of the error involved in this assumption.

In the "F" boats, which had twin tractor propellers, the tail plane of which, about three-quarters, was in the slipstream, was checked by the above formula and seemed fairly satisfactory, but on trial was decidedly tail heavy engine off, as were all the "F" boats. This may be partly accounted for by the fact that the C.G. when carrying full military load was further back than was originally intended.

With pusher propellers the conditions are obviously simpler, and the results of calculations likely to be more in agreement with the observed facts. On the other hand, pusher propellers on a flying boat are very liable to damage from parts which may either break off or work loose from nuts, and even spanners, etc., which have been left through carelessness in such a position that they fall into the propeller in flight. This has happened frequently in the lecturer's experience, no matter what precautionary measures are taken.

The most satisfactory, and really the only way at present, apart from full scale experimenting, which is both costly and wasteful of time, to ensure the correct disposition of the tail plane surface to fulfil the above requirements is to test a complete model in the wind tunnel, reproducing the actual slipstream by a propeller.

In making out a case for the tail plane supported on outriggers, it has been said that there is great difficulty in fixing the tail unit rigidly on a flying boat hull. Experience has not shown this to be the case. There is no difficulty in mounting either a monoplane or biplane tail of any aspect ratio and in any vertical position relative to the propeller axis likely to be required. With the

latter type it will usually be found necessary to have the lower plane of smaller aspect ratio than the upper one in order to avoid damage from the side waves which close in at vicinity of the tail when "taking off."

An adjustable tail plane may, however, present some difficulty, as in case of a biplane tail plane, of which the fins and rudders form an integral part; the effort required to operate it under its own weight, apart from air load, may be beyond the capability of the pilot, unless some auxiliary motive power is used, such as oil pump and ram. Anticipating this trouble on the "Fury," the elevator on the lower plane was operated separately by a long lever on a quadrant centred on the elevator control shaft. This was used to obtain trim and the upper elevator for extra control in the usual way. The result was quite satisfactory, but owing to hull interference the efficiency of the lower plane was relatively low.

With regard to fin and rudders, owing to the long forebody, these are of relatively large area. The rudder area on the "F" boats was barely sufficient for control with one engine cut out completely. The "Fury," however, was very satisfactory in this respect; the arrangement there used, namely, three propellers abreast, and three fins and rudders in their respective slipstreams, would seem to solve the problem of one engine cutting out. Little data appears to be available on the subject; calculations are liable to be greatly in error as the yaw taken up by the machine after one engine has cut out is difficult of determination with any degree of accuracy. A figure of some interest in this connection and not generally available is the air drag of a stationary propeller. Tests of a fair variety of different propellers gave from 12 to 13 lbs. per sq. ft. of projected area at 100ft./sec.

It is a matter of some importance to be able to estimate the aerodynamic balance and maximum moment which can be exerted by the elevators at or near the taking-off speed. Wind tunnel tests on a complete model show there is a slight increase in kl of the order of 10 per cent. over the usual range of angle of incidence, and a considerable effect on pitching moment, if the model is tested under conditions corresponding to the aeroplane being at ground level. The change in moment is obviously the result of the flattening out of the downwash at the tail plane due to the interference of the ground.

This produces a larger tail lift or nose-diving moment, becoming greatest at large angle of incidence. With a flying boat the conditions are more complicated, owing to the wave formations at the tail. With slipstream effect it would seem impossible to predict the angle of downwash at the last plane, and recourse must again be made to wind tunnel tests, reproducing the actual working conditions. Fortunately, flying tests have shown that aerodynamic balance can be obtained, as at least two types of boats have taken off into the air without the use of elevator control.

From the above brief discussion it will be observed that there are many problems of first importance in the aerodynamical design of a flying boat which, for a satisfactory solution, depend upon an accurate knowledge of the resultant propeller downwash at the tail plane. As the many complex factors entering into the complete equation are probably beyond mathematical analysis, and the various more or less empirical formulæ unreliable and likely to lead to disappointing results, the necessity for developing wind tunnel tests on complete models, propellers running at the appropriate advance per revolution, cannot be over-estimated.

As with the aeroplane, controllability at low speeds is naturally of great importance, but probably relatively less, as generally the choice and extent of a landing ground is not so restricted. Also, once on the water the boat pulls up quickly, owing to the large hull water resistance. Thus it is possible, and is

the usual practice, to glide at a comparatively high speed until close to the water before flattening out and alighting.

In connection with the operation of control surfaces on large flying boats, it may be of interest to state that on the Felixstowe "Fury" the rudder, ailerons, but not the elevators, were balanced; servo-motors were also fitted to all the control surfaces as it was thought adequate control under certain flying conditions might not be within the capability of the pilot, there being no previous experience with a boat of this size. However, she proved to be extraordinarily light on all the controls and superior to the much smaller "F" boats. The servo-motors were therefore removed, as Col. Porte decided that any advantage to be gained by their use did not warrant the additional weight and complication.

Hull Lines and Dimensions

The setting out of the lines and fixing upon the main dimensions of a flying boat hull to meet the requirements of a given h.p. and loading specification is a problem by no means easy, owing to the many varied and often conflicting conditions which have to be fulfilled. A large amount of data from tank tests on models is available, especially with regard to the effect of modification in form and dimensions; but it should be remembered that such tests are carried out under smooth water conditions, under which conditions it would be expected that full scale and model would be in agreement; such conditions in actual practice are the exception rather than the rule. Tank tests, however, afford the only reliable means of obtaining data with regard to resistance, hydroplaning efficiency, etc., and the effects of minor alteration in form of which the designer must have full information. The development of the technique of tank testing is, in this country, due almost entirely to the work of G. S. Baker of the N.P.L. Probably the main point in which the experience gained on full scale work is at variance with the results of tank tests is in the dimensions of a hull for a given displacement, about which there has been much controversy, and finality has not yet been reached.

The most successful design will result from the best compromise between the various water and aerodynamic requirements, and will depend primarily upon sound judgment based upon the results of considerable full scale and tank experiments.

The modifications carried out on and the new types of hulls evolved at Felixstowe were arrived at from full scale experimenting, and with the exception of the "Fury" hull, tank tests on the corresponding models were not available at the time. In so far as the conditions may be compared, with the possible exception of the relation of dimensions to displacement, the deductions from full scale are in agreement generally with those from the model.

As facilities were not available to measure the full scale resistances, the criterion adopted was the lbs./h.p. at which a boat could be taken off. This was justifiable, because in many cases the same or a similar type of superstructure was attached to different hulls. Admittedly this was a rough and ready method, although quite rational under certain assumptions. With experience it was possible in this way to predict with fair accuracy the loading capacity of any proposed type of hull design.

As the hydrodynamic forces acting on a hull during the period of taking off are even more complex than the corresponding aerodynamic forces acting on a body, recourse must again be made to model testing. The principles involved in the transition from model to full scale will now be discussed briefly for the sake of interest and completeness.

The resistance to the passage of a hull through the water is made up of two parts, frictional and wave making. The water forces may, therefore be assumed

to depend upon the density ρ, the speed v, a dimension l, usually the length between perpendiculars, the kinematic viscosity γ, and acceleration due to gravity g. Accepting such, it is easily proved from the theory of dimensions that the resistance R must take the functional form

$$R = \rho v^2 l^2 \ (vl/\gamma \ gl/v^2) \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which (vl/γ) (gl/v^2) are the friction and wave making terms respectively. Many years of ship design experience and experiment have shown that these terms may be treated separately, and further, in the case of hydroplanes, Baker has shown that skin friction may be neglected.

We may therefore write

$$R = \rho v^2 l^2 \ (gl/v^2) \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which is the basis of Froude's Law of Corresponding Speeds, at which speeds the wave formation of full-scale and model are similar. Denoting full-scale by capital letters, we have from (2) above

$$R/V^2L^2 = \gamma/v^2l^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

provided $V/v = \sqrt{(L/l)}$ and the under-water parts geometrically similar, that is, attitude of full-scale and model identical, and displacements in the ratio of the cube of the linear dimensions. If Δ is the displacement at rest, and P at any other speed V, then assuming the whole load air-borne at the taking-off speed, and air load at any other speed proportional to the square of the velocity, as the attitude is nearly constant, P is readily obtained, since $P = \Delta$ — air load.

In the same way it may be shown if M is the full-scale moment

$$M/m = (L/l)^4$$

It may happen, and frequently does, that, in a proposed design of hull, the under-water lines are similar to a hull of which only the full-scale resistances and moments are known. To this case the above applies also, further noting the taking-off speeds will be in the ratio $(W/w)^{\frac{1}{6}}$.

As an illustration, Fig. 1 shows typical water resistance and attitude curves under smooth water, no head wind, and natural trim conditions for a twin-engine flying boat hull of 12,500lbs. displacement. The air drag has been calculated, allowing for slipstream effects and added to the water resistance curve giving the curve of total resistance. In Fig. 2 these results are exhibited in a more interesting form, to which has been added the propeller thrust horse-power available. Under these conditions, which are the most adverse from the h.p. point of view, obviously the criterion that the boat will take off is that the curve B lies wholly below the h.p. available curve, also as the thrust available = water + air resistance + mass x acceleration it will be seen that the area between these two curves up to the ordinate at the getting-off speed is proportional to the length or time to unstick, provided the hull is running at its natural trim.

In order to help toward a clearer understanding of the discussion which follows, it is now proposed to give a brief description of a process of taking off, and the action of the hull under normal sea and weather conditions.

At rest, the total weight is supported by the hull buoyancy, and as the metacentric height is generally negative, lateral stability is obtained by means of wing tip floats. Owing to the high centre of thrust and low water and air resistance at speeds up to say 10 knots, the throttle is opened slowly, and elevators held up to prevent trimming by the bow. As the speed increases, elevators are put neutral when hull will gradually trim back of its own accord. From this speed onwards the load supported by buoyancy is gradually taken up by the aerodynamic water forces acting on the planing surface, the fore body rising first, followed by the tail, which may be assisted by putting the elevators down slightly until the boat is planing cleanly. During this period the water resistance increases

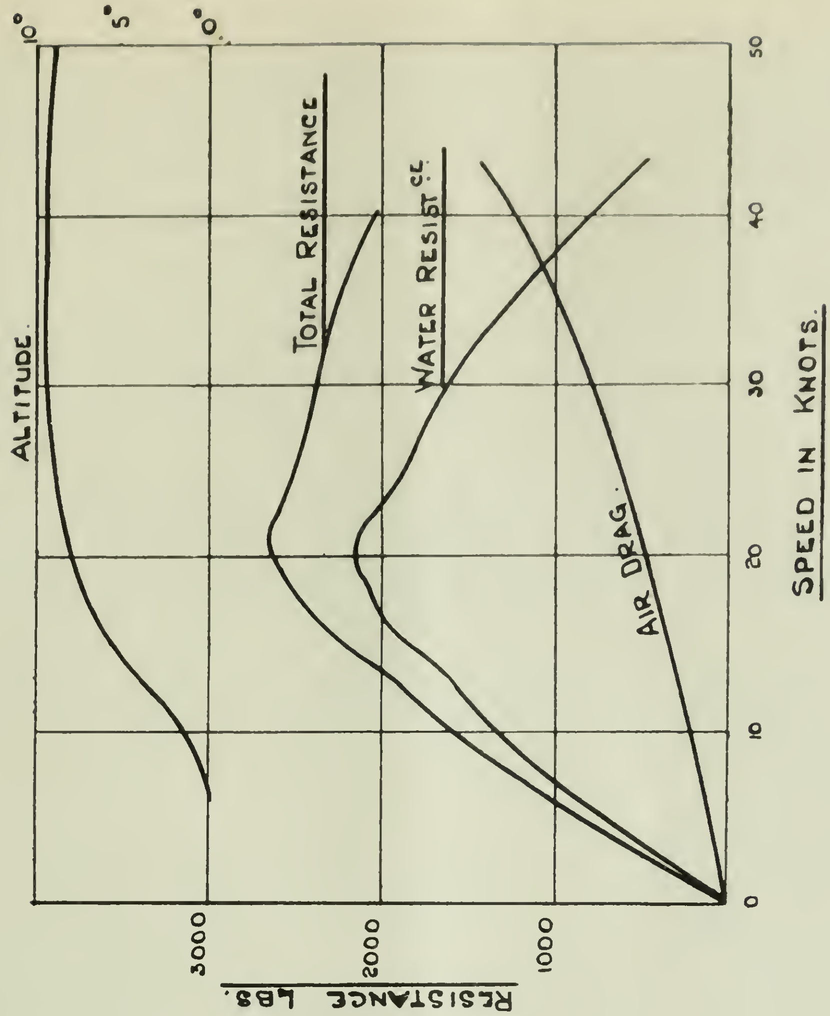

ALTITUDE.
TOTAL RESISTANCE
WATER RESIST^CE
AIR DRAG.
RESISTANCE LBS.
3000
2000
1000
10°
5°
0°
0
10
20
30
40
50
SPEED IN KNOTS.

FIG. 1.

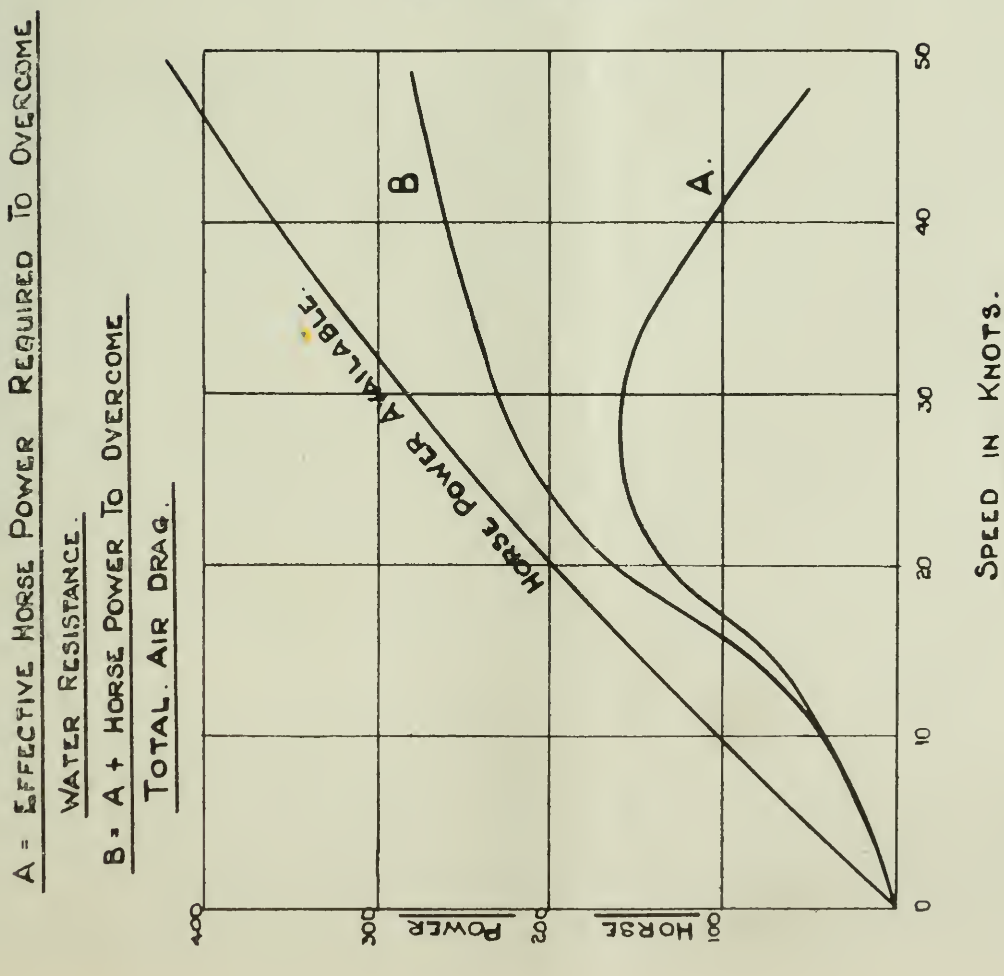

A = EFFECTIVE HORSE POWER REQUIRED TO OVERCOME WATER RESISTANCE.
B = A + HORSE POWER TO OVERCOME TOTAL AIR DRAG.
HORSE POWER AVAILABLE.
A.
B
HORSE POWER
400
300
200
100
0
10
20
30
40
50
SPEED IN KNOTS.

FIG. 2.

steadily, after which if the hull is well designed it will continue to drop owing to the improved working of the step and to the increased share of the load taken by the wings. The speed at which the water resistance reaches a maximum is usually known as the "hump" speed, and is shown clearly in Fig. 1. Generally a boat which gets over the "hump" has ample power for flight. From this speed onwards the pilot has sufficient elevator control to trim the boat to any attitude giving least resistance. If there is little change of resistance with change of attitude, the boat is gradually trimmed back, accelerating all the time, until the lowest flying speed, or more commonly several knots above, is reached, when by a sharp pull up of the elevators, a clean break-away is made from the water, after which the load is entirely air borne. The elevators are then depressed before trimming to gain height.

Up to about "hump" speed lateral stability is obtained by the use of wing-tip floats and such aileron control as is available. After which speed, the boat trims naturally on an even keel due to the stable hydrodynamic forces acting on the planing surface. Also the aileron control becomes more effective as the speed increases.

The consideration of the above and of the problem of safe and easy landing leads to the following requirements, which are fundamental to success in the design of a flying boat hull.

(1) Avoidance of diving at low speeds.
(2) Seaworthiness.
(3) Hydroplaning efficiency, and landing with minimum shock.
(4) Stability at high speeds on the water.
(5) Ability to trim fore and aft to enable to take off or land.

Before discussing the influence which the requirements under the above headings have on the hull lines and dimensions, a brief description of the hulls tested and evolved at Felixstowe will now be given.

The first experiments were taken in hand early in 1915, when no engines of high power were available, hence the chief difficulty met with was the problem of taking off with a reasonable load. Thus the early experiments were made with the object of developing hydroplaning efficiency, such questions as safe landing, seaworthiness, stability, etc., were more or less neglected until more powerful engines became available. The first hull tested was a modified Curtiss "America" flying boat (Fig. 3) No. 950. Weight, light, 3,100lbs.; fully loaded, 4,500lbs.; h.p., 160; length of hull, 30ft., single step, projecting fin forward, ending at step, which was under the C.G. Fore and aft angle between the underside of the tail and planing surface of the ship was 10deg.

At high speeds all single step hulls balance on the step, and trim depends upon the angle of the tail portion, which, to avoid water drag, should be held up during the acceleration period. The original tail plane was lifting, which was excellent from this point of view, and as much load as could be flown with could be taken off the water. To improve the stability in flight, especially trim engine on and off, the tail was made negative. In smooth water there did not seem to be any appreciable loss in hydroplaning efficiency, but in rough weather, owing to the lack of buoyancy forward, she was very wet.

A new hull, No. 1230, was built in which the fins were narrower, and carried further aft, the underside of tail rounded, and of lighter construction. This hull proved to be inferior to No. 950, owing to the rounded underside of the tail portion, which caused increased suction, making it very difficult to lift out of the water in calm weather. With the different type of construction adopted there was a saving of about 300lbs., but it was considerably weaker than the previous hull, and failed after several landings.

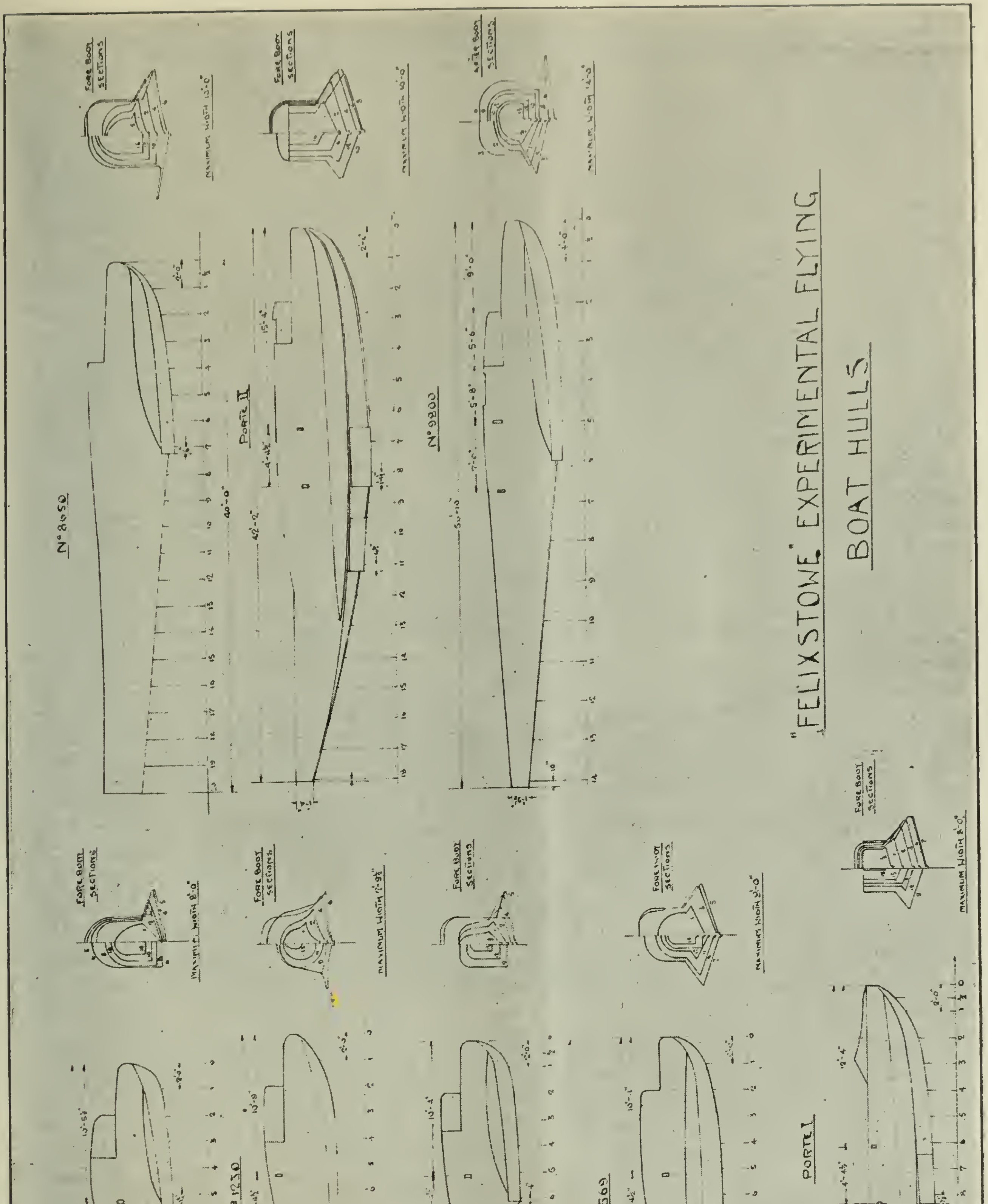

"FELIXSTOWE" EXPERIMENTAL FLYING BOAT HULLS
N° 8650
PORTE II
N° 9800
N° 950
N° 1230
N° 3545
N° 3569
PORTE I
FORE BODY SECTIONS

FIG. 3.

The next hull (Fig. 3), No. 3545, was similar to No. 950, but the tail portion was 2ft. longer, and fore and aft angle reduced to 7deg. Owing to the reduction of this angle, it was not possible to trim back to the same angle as with No. 950, resulting in a higher "taking off" speed.

The chief lesson to be learned from these hulls was, from the point of view of ability to hydroplane and low taking off speed, the tail portion should be flat-bottomed to reduce suction, and fore and aft angle should be large to obtain the necessary trim.

It was now decided to tackle the problem of easy landing conditions, and increased strength without sacrifice of planing efficiency.

As most landing breakages in those days occurred at the step, the next experiment was to find if a step was necessary. A complete new bottom was built on the previous hull, with no step, but steeper "Vee" bottom, and tail well swept up. No. 3569. The engine power available was not sufficient to take off. A step was then added 5ft. behind the C.G., when 4,200lbs. were taken off. Landing was exceptionally easy owing to the large fore and aft angle. The deeper "Vee" resulted in little or no shock, with either a normal or nearly stalled landing. Owing to the step being so far aft of the C.G. the hull ran at a small angle, and a large moment was necessary to trim back to take off. To relieve the pilot of this load, the step was shifted 3ft. forward.

The next hull was called the Porte I., and was the prototype of all the "F" boats. An entirely different type of construction was used, which will be shown later. The hull was built at Felixstowe, and carried the same "Curtiss" superstructure as before. Originally a single step, below the rear spar, was fitted. Hull 2ft. longer in the nose and tail than No. 3545, being 36ft. overall; fore and aft angle 18deg., tail portion 7in. higher than on No. 3569. Fins were carried well aft of the step and swept back into the hull. "Vee" bottom as on No. 3569. Bows fuller with a distinct flair on the first 3ft.

Difficulty was experienced in taking off owing to tail drag. A second step was therefore fitted 7½ft. from the stern. It was now possible to take off, but with less load than earlier hull. Finally, a third step was added intermediate between the main and aft steps. This brought load capacity up to that of the earlier hulls. This hull was in many respects far superior to any hull tested previously owing to the improved form of bows, cockpits were dry, landing shocks were reduced to a minimum, and behaviour generally landing and getting off was excellent.

It was now decided to experiment with larger hulls, and to begin with a large Curtiss "America" was obtained (Fig. 3), No. 8650, particulars of which are as follows:—Total weight, 8,700lbs.; twin 160 h.p. Curtiss engines; hull 40ft. long, 11ft. maximum beam; fore and aft angle 7½deg. With this load there was not sufficient power to take off. 240 h.p. Rolls-Royce engines were then fitted. Load taken off with difficulty, principally due to the lack of buoyancy forward. A most marked hump speed, about 18 knots, was noticed. At a later date, when more powerful engines became available, these boats did some quite good work, but hulls were weak structurally. As the Porte I. hull was much superior to No. 8650 it was decided to design and build a new hull on the same lines to take the "America" superstructure, and known as the "Porte II." Particulars:—Weight, loaded, 16,500lbs.; hull, 56ft. 10in. and fore and aft angle 20deg.; bows 2ft. longer than No. 8650; two steps, one under rear spar, and the other 7ft. aft of spar, proved a much superior boat. Hump speed less marked, accelerated evenly and rapidly to the taking off speed. Loading capacity increased. General seaworthiness greatly improved, gain in buoyancy, and structural strength without increase in weight.

Porte Baby No. 9800.—The building of this large experimental flying boat was carried out during the same period as the experimental modifications to the Curtiss "America" hulls were in hand, and while the experience gained from them was incorporated, it was not possible to take advantage of the results from the Porte I. Experience with these hulls confirmed the results obtained with the "America" hulls. Particulars:—Weight, loaded, 16,500lbs; hull 56ft. 10in. long; beam at step, 14ft.; width of body, 7ft.; fitted with three 250 R.R. engines; wing as tractors, and centre a pusher.

Trials showed the length of forebody was not sufficient to prevent wallowing in a following sea. Bows were lengthened 3ft., which enormously improved water performance. Air pipes fitted to the step were found to be unnecessary.

Considering the low power and the use of stranded cables throughout, the air performance was quite good. Top speed, 68 knots; climb up to 3,000ft., about 150 ft./min. Time to unstick with full load, about 35 seconds.

It was decided not to proceed further with the development of the type, as the water performance was much inferior to the Porte II., and type of hull construction weak.

Taking into consideration water performance and hull construction, the most promising type to develop was obviously the Porte II. This led to types known as the F.2A., F.3, and F.5. In the F.3 and F.5 types the hull was lengthened 3ft., otherwise the lines were essentially the same as those of the Porte II. Increased h.p. and improved detail and aerodynamic design led progressively to greater load capacity and air performance. These types were put into production and used extensively during the late war.

From the experience gained with these later types, it was decided to construct a still larger boat. The P.S.B. or Porte Super Baby, officially known as the Felixstowe Fury, was the result. Fig. 4 shows profile, plan, and body sections in detail. It was originally designed for 24,000lbs. total weight, and to be fitted with three 600 h.p. R.R. Condor engines. As these engines did not become available, five Eagle VIII.'s had to be used, which led to a decided drop in air performance.

From every point of view the boat was the best design turned out at Felixstowe. It was found that the normal load could be increased to 28,000lbs., under which loading, seaworthiness, ease of taking off and launching was superior to that of the previous F-boats. Loading tests were continued up to 33,000lbs., at which load Colonel Porte took her off in Harwich Harbour. Landing under all loads was without perceptible shock.

The extraordinary behaviour of the hull in rough seas was due mainly to the buoyancy of the hull and the lines adopted. With a load of 28,000lbs. the chine at main step was not submerged. Under all conditions, the propellers, cockpit and tail plane were clear of water thrown up. From this point of view it was found that the bow sections could be improved considerably. These were too blunt, resulting in unnecessary pounding when getting off or landing in a rough sea, or at moorings. It was decided in any future designs to drop the keel profile forward, keeping everything else the same, thus obtaining a finer entry, without loss of buoyancy forward. The net result of all these experiments is that the lines and dimensions (Fig 4) of a successful flying boat hull for a given displacement have been evolved. It now remains to show how the various features in the design contribute towards the fulfilment of the requirements laid down above, and to indicate where, if at all, these differ from what might be deduced from tank tests.

Taking the headings in the same order—

(1) At low speeds all boats have a tendency to trim forward, which is further increased in a flying boat by the high propeller thrust line, and clearly any boat

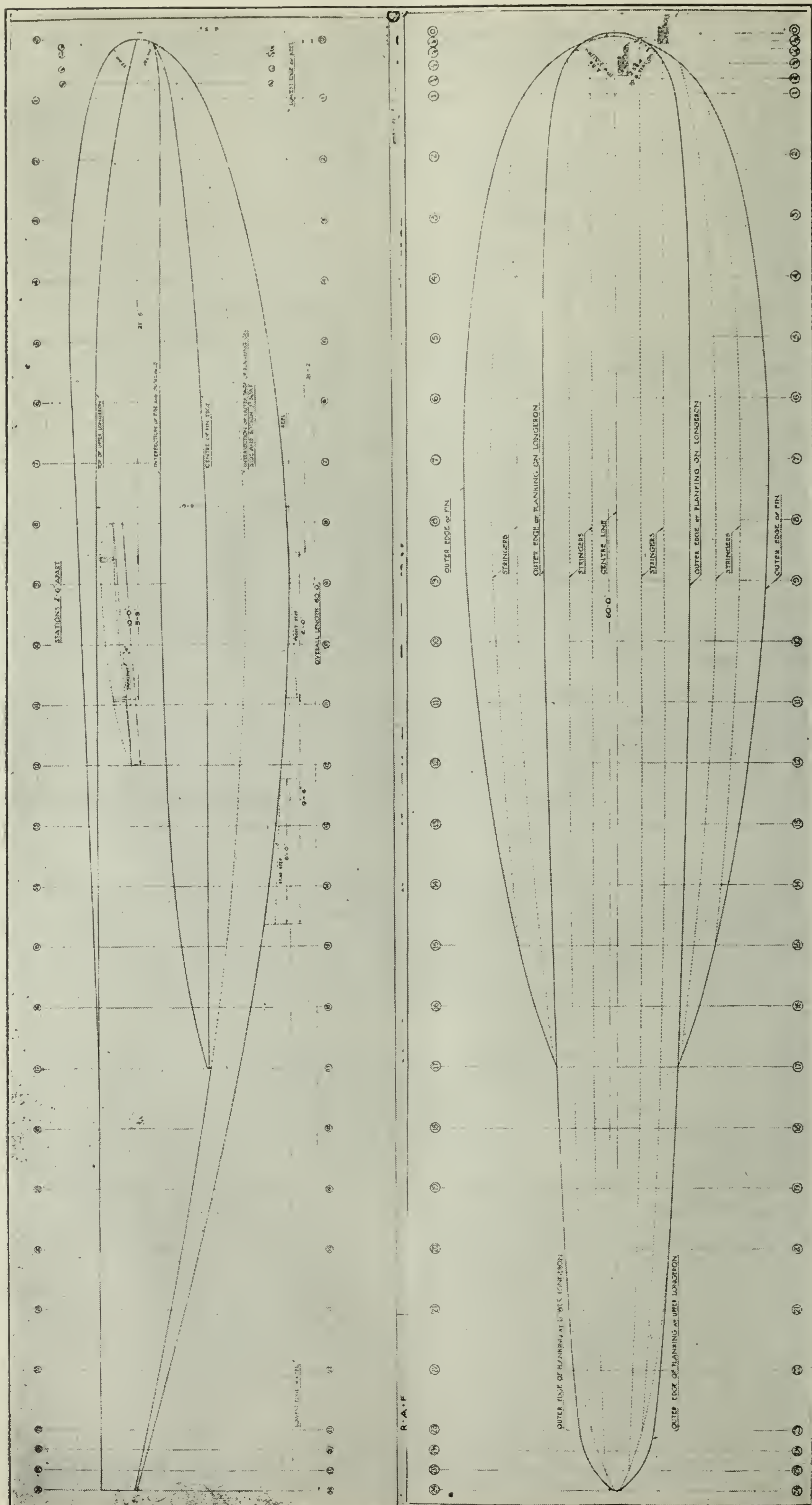

STATIONS 2'6" APART
OVERALL LENGTH 60'0"
CENTRE OF FIN EDGE
R.A.F
OUTER EDGE OF FIN
STRINGERS
OUTER EDGE OF PLANKING ON LONGERON
CENTRE LINE
60'0"
OUTER EDGE OF PLANKING AT UPPER LONGERON

FIG. 4.

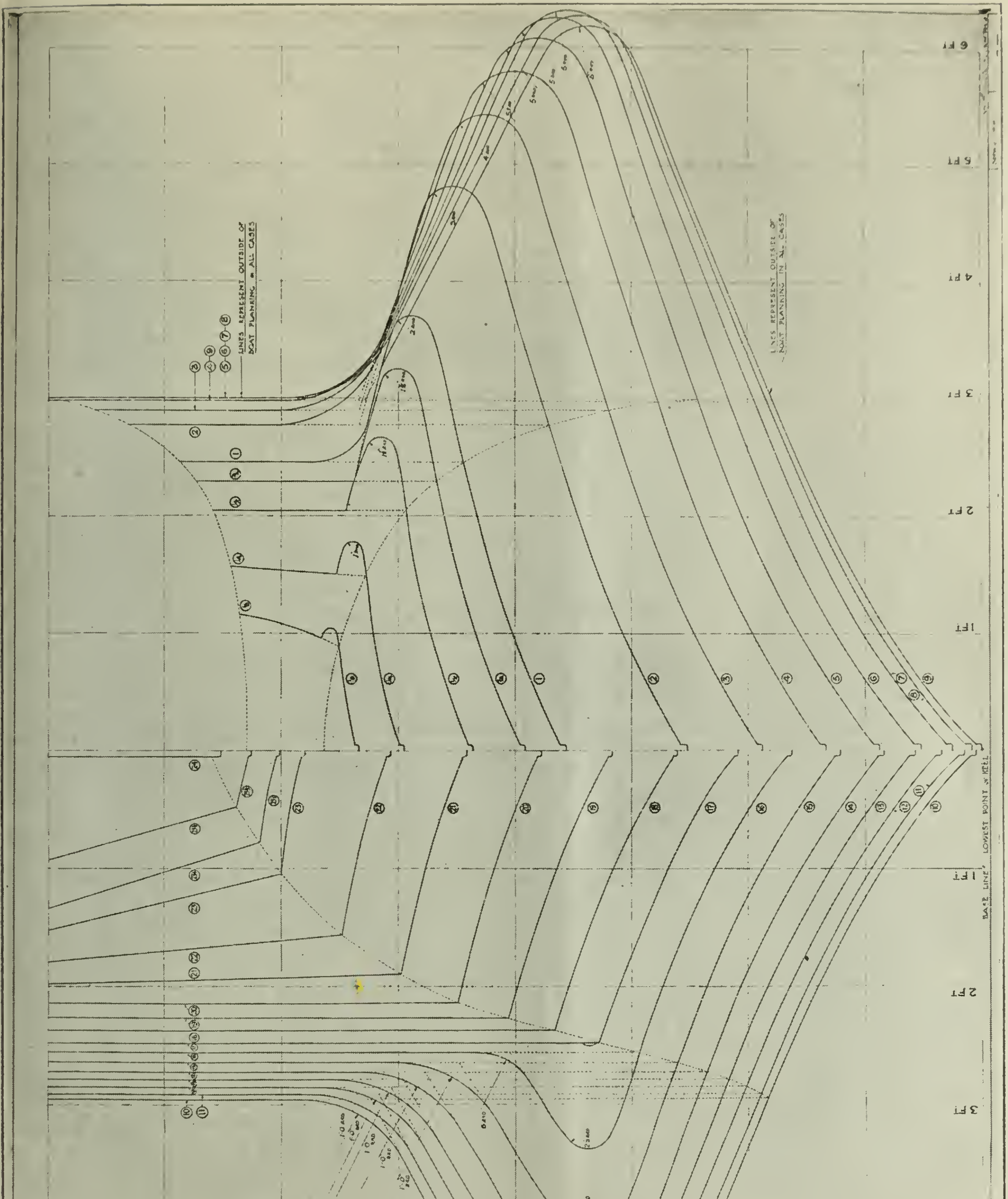


FIG. 4 (*continued*).

could be made to dive under by applying a large enough nose trimming moment. This is overcome by setting the hull surface under the chine from the main step forward at a slight positive angle about 2deg. to still water line, the angle increasing in a gradual manner up to the stem. This results in a well uplifted chine and a fairly long forebody; otherwise, should this surface attain some negative angle, called the critical angle, an area of reduced pressure will be created, drawing the bows below the water.

As the tailplane is well in the slipstream, further prevention is obtained by opening the throttle slowly, keeping the elevators well up.

(2) Seaworthiness. By this term is meant seakeeping qualities, with special regard to ability to take off, and land in smooth or moderately rough seas, keeping the cockpits dry, propellers and tailplane clear of solid water. Also, in addition, such matters as reserve buoyancy, wallowing in a following sea, and easy riding at moorings.

Clearly, other things being equal, the larger the hull the more seaworthy it will be. Increase of dimensions means increased weight and air resistance, therefore the hull should be of minimum dimensions to fulfil the above requirements, provided other considerations are of little importance.

All hulls send up and outwards from under the chine, beginning at the bows, a V-shaped blister in plan. To turn down this water the sections should be fine and flat just under the chine. The P.S.B. was better in this respect than the F-boats, which had straight sections of 150deg. included angle. A flying boat hull meeting a wave should cut it, then lift to it, without pounding, and swamping the cockpits, etc., and with minimum retardation of its forward motion. This is best achieved if the bow sections are fine and flat under the chine, to facilitate which the keel profile forward may be dropped. The buoyancy forward should be large to attain this, the chine should be full in plan, the net result being an increasing stern trimming moment with depth of immersion of the bows, and sea thrown well clear. In this respect the features which are influenced by (1) and (2) are mutually helpful.

Assuming good sections forward, the longer the forebody is, the cleaner is the hull, and the further outwards does the blister meet the wings. This is of great importance in the case of a multi-engined boat, with engines between the wings, taking off in a rough sea. From considerations of directional stability in the air, the forebody should be short to decrease the side surface forward. When, however, this dimension does not exceed .45 of the length of the hull, no trouble is likely to be experienced in this respect.

The freeboard at the lowest cockpit opening should not be less than 3ft. 6in., and the still water level line, the boat fully loaded and trimmed slightly by the stern, should not be below the chine at the main step, otherwise the boat will be very dirty and sluggish, especially in a moderately rough sea until running well on the step. While this occurs mostly during the transition period from about 10 knots until just over the hump speed, it is the ability to get over this stage in a rough sea which settles the possibility of taking off at all.

For a given displacement this will mean a larger and more beamy hull than would be found necessary from tank tests in smooth water. For example the N.4 hull (lines of which are shown in Fig. 5) of 32,000lbs. displacement, 64ft. long, 9ft. beam over chine at step, may be compared with the P.S.B. hull, at 28,000lbs. displacement, hull 60ft. long, and maximum beam 12ft. 6in. In the N.4 type of hull the fin top, until well forward, is entirely below the still load water line. The excellent behaviour of the P.S.B. hull under smooth and rough sea conditions was conclusive proof that in hulls with projecting fins the chine at step should be not more than 2in. below, and preferably above the water, when at rest. In this respect only the straight sided hull has the advantage over the projecting

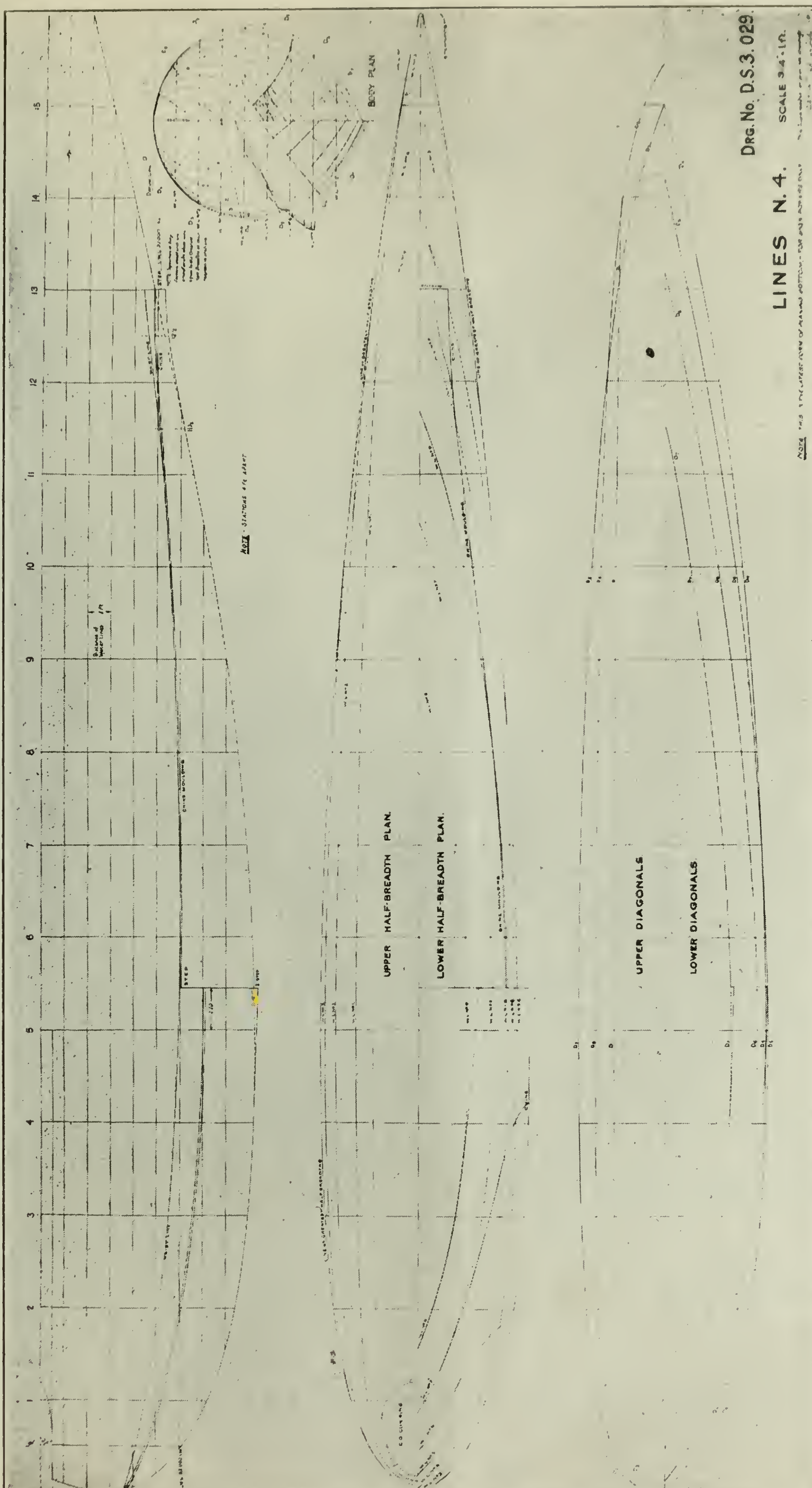

LINES N. 4.
DRG. No. D.S.3.029
BODY PLAN
UPPER HALF-BREADTH PLAN.
LOWER HALF-BREADTH PLAN.
UPPER DIAGONALS
LOWER DIAGONALS

FIG. 5.

fin type. Tank tests have shown that for a given displacement the effect of increase in all dimensions passing from a 35ft. to 46ft. hull, the maximum h.p. required is decreased about 26 per cent. at the hump speed, while the weight of hull increases roughly 75 per cent. assuming the weight of hull increases in proportion to the skin area. Also, with increase of beam only, for a given displacement at all speeds below the hump speed, the resistance is increased, but with very large beams the hump speed occurs earlier and the h.p. required is less. In all cases decrease of beam increases the maximum h.p. required.

On the other hand, considerations such as space required for cargo and passengers would probably increase the dimensions above those obtained from calculations involving only h.p. and hull weights. Also, if the Porte type of hull construction was adopted, the increase in weight would not be proportional to the skin area, as the greater part of the upper portion of the hull may be of very light construction. With good design, greater seaworthiness would be obtained and the increase of weight and air resistance of the larger hull would not be entirely a loss, from a commercial point of view at any rate.

(3) In this case the requirements are to a certain extent conflicting. Other things being equal, the boat with flat sections from keel to chine in the region of the step will take off with the largest lbs./h.p. Landing, however, would be difficult in a moderately rough sea, and at all times would require skilful piloting, to ensure landings without damage to the hull. To obtain good cushioning, the sections should be of " Vee " formation or convex as in the P.S.B. (see Fig. 4). With the high-powered engines now available, the sacrifice in planing efficiency is of little importance.

The suction area aft of step should be reduced to a minimum ; a second step should therefore be fitted well aft of the main step, aft of which the tail portion should rise abruptly to the stern. The step should extend from chine to chine across the form, to ensure adequate ventilation. Air pipes were found useless as a makeshift method of dealing with the suction. The model of the P.S.B. hull was tested in the tank with the step ending at the chine rounding. The riband on the chine prevented free access of air to the step, owing to the water clinging to it. The step was then extended to the full beam over the chine, resulting in a decrease of about 30 per cent. in resistance at the hump speed.

A suitable depth of step for a 45ft. and 60ft. hull was found to be 3in. and 5¼in. respectively.

(4) From just above the hump speed to the getting off speed, that is, when the boat is on its step, the F-boats tend to porpoise, especially in a rough sea. Between these speeds there is generally sufficient elevator control to check these oscillations. In this respect the F-boats are inferior to some forms developed at the tank, in which porpoising has been either eliminated or suppressed until a high speed has been reached. This has been achieved by keeping the running angle small, placing the main step under or slightly forward of the C.G. and the two steps far apart, the angle between the tangent to the main step at the keel and the line joining the keel at step to the chine of the after step being kept as small as is consistent with ability to trim to take off.

(5) Taking any line, usually the top surface on the hull profile, as datum, the angle of the chord of wings relative to this line is fixed within a few degrees by the condition that between top and say cruising speed, the angle between this datum and the flight path should be that giving minimum air drag of the hull.

Also, bearing in mind what has been said above with regard to hull lines and diving at low speeds, the centre of buoyancy should be vertically under the C.G. when the hull is either parallel to the L.W.L. or slightly trimmed by the stern. This is easily obtained by slight modifications to underwater shape, without any sacrifice in other respects.

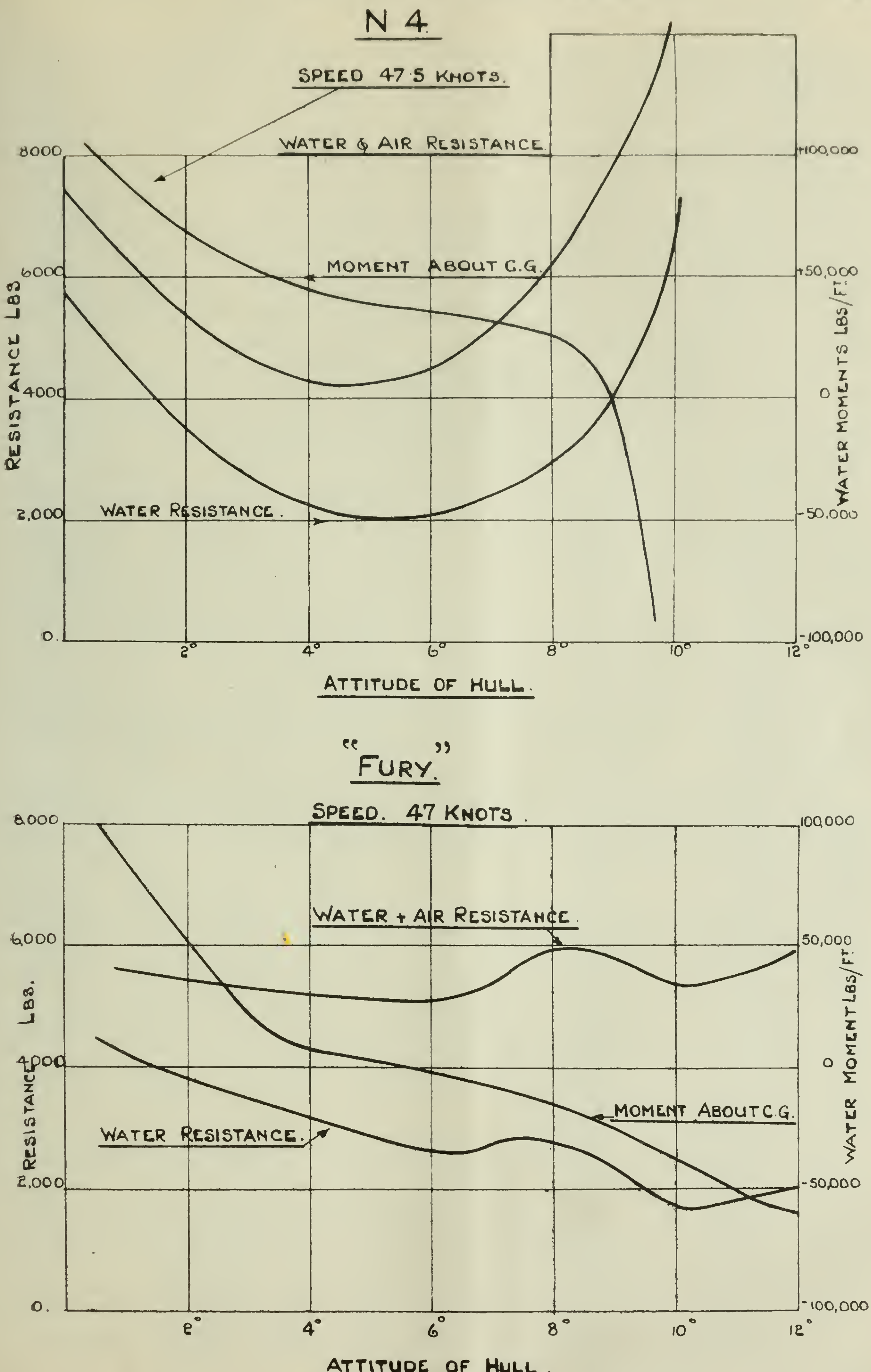

N 4.
SPEED 47·5 KNOTS.
WATER & AIR RESISTANCE.
MOMENT ABOUT C.G.
WATER RESISTANCE.
RESISTANCE LBS
WATER MOMENTS LBS/FT.
8000
6000
4000
2,000
0.
+100,000
+50,000
0
-50,000
-100,000
2°
4°
6°
8°
10°
12°
ATTITUDE OF HULL.
"FURY."
SPEED. 47 KNOTS.
WATER + AIR RESISTANCE.
MOMENT ABOUT C.G.
WATER RESISTANCE.
RESISTANCE LBS.
WATER MOMENT LBS/FT.
8,000
6,000
4,000
2,000
0.
100,000
50,000
0
-50,000
-100,000
2°
4°
6°
8°
10°
12°
ATTITUDE OF HULL.

FIG. 6.

It is now necessary to be able to trim back the boat to take off to such an attitude that the angle of incidence of the wings is that corresponding to the lowest safe speed, usually a few knots above stalling speed, without excessive elevator moments and increase of hull resistance. Also, to enable a safe and easy landing to be made, it should be possible to land trimmed back on either or both steps with only a small area of the forebody, just forward of the main step, touching the water. This is attained if the fore and aft angle between the planing surface at main step and the after step is fairly large, and the tail portion, aft of the second step, swept up abruptly to the stern. This angle is best obtained from tank tests. From Figs. 4 and 5 it will be noticed that the disposition of the steps on the "Fury" N.4 hulls are different. As shown on the "Fury," are typical of all the F-boats, and on the N.4 of forms developed by Baker at the Froude Tank, N.P.L.

The former tend to run at small angles, and at high speeds may be run on either or both steps, the water resistance being least when running on the second step, trimmed well back, the change over taking place at the peak in the resistance curve. Fig. 6 shows the curve of water resistance at about six knots below taking off speed for different hull attitudes.

With the latter, the steps are far apart and the C.G. just over or aft of the main step. At all speeds both steps are in contact with the water, and there is small moment tending to keep the after step in contact with the water. If uncontrolled, these hulls run trimmed back at an angle close to that corresponding to taking off speed. Decrease of resistance is obtained by trim forward (see Fig. 6). From Fig. 6 a rough comparison can be made of the conditions under which each type is running, at or close to the taking off speed. In this connection may be given an extract from the recommendations made by the Sub-Committee on Accidents of the Advisory Committee for Aeronautics :—

"The form should be so designed that with the heaviest possible loading its natural trim (*i.e.*, running angle, hands off) shall maintain the wings at a safe flying angle."

Before reasoning from these curves it should be noticed that aerodynamic balance is assumed, which is reasonable; the water moment vanishes immediately the hull leaves the water, also allowing for slipstream effect on the tailplane, the maximum moment due to the use of full elevator movement will be of the order of 90,000lbs./ft.

Considering the case of the "Fury." The elevator control will be sufficient to trim the boat through nearly the whole range of attitude. At this speed, as will be seen from the total resistance curve, the resistance is nearly constant. At lower speeds the resistance will be less, probably at small altitudes. For this reason the boat will most likely be trimmed at a small attitude, about 4deg. to 6deg. As will be seen from the moment curve, no matter at what attitude the boat is running an air stalling moment has to be applied to trim back to "take off." Thus, unless there is ample power for acceleration or "taking off" speed, say five knots above stalling speed and elevator corrected, there is the possibility of the boat leaving the water stalled.

With the N.4 step arrangement, this condition may be made almost impossible to attain. The natural trim is about 9deg. If, therefore, the hull is so attached to the wings that the incidence at this attitude is say 3deg. below stalling incidence, which will only increase the getting off speed by about 5 per cent., then the boat can be taken off without change of trim. Any tendency to trim back to larger angles is prevented by the rapid increase of water moment. In other words, the conditions present at this attitude are stable.

The minimum resistance occurs at about 5deg., and to maintain this a nose diving moment has to be applied by the elevators; to trim back to "take off" leads again to stable conditions.

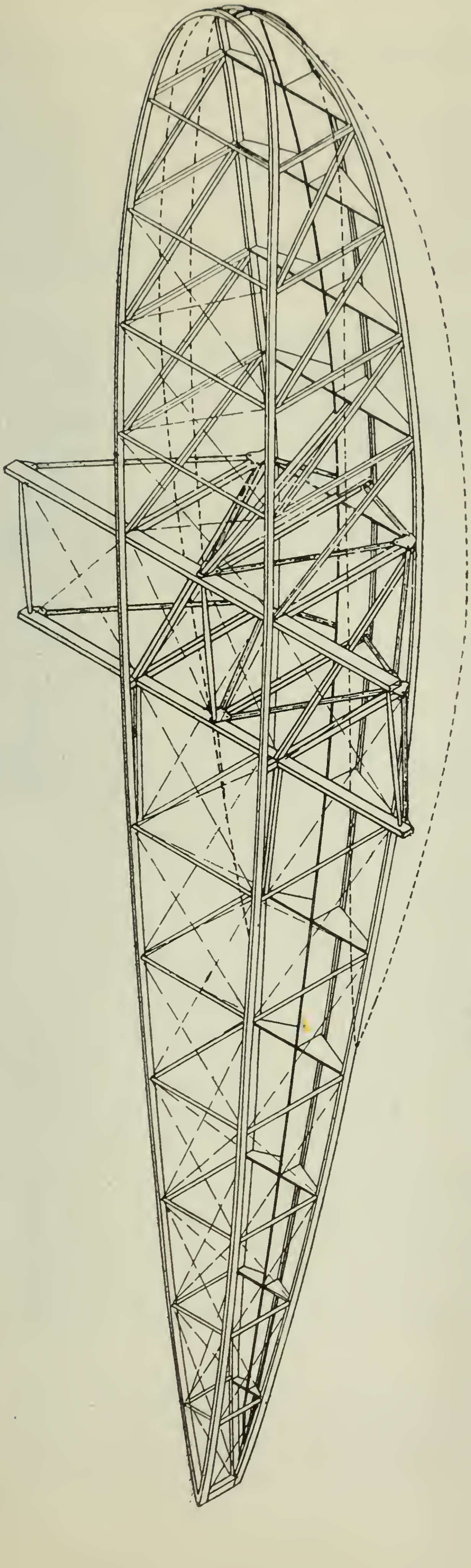


FIG. 7.

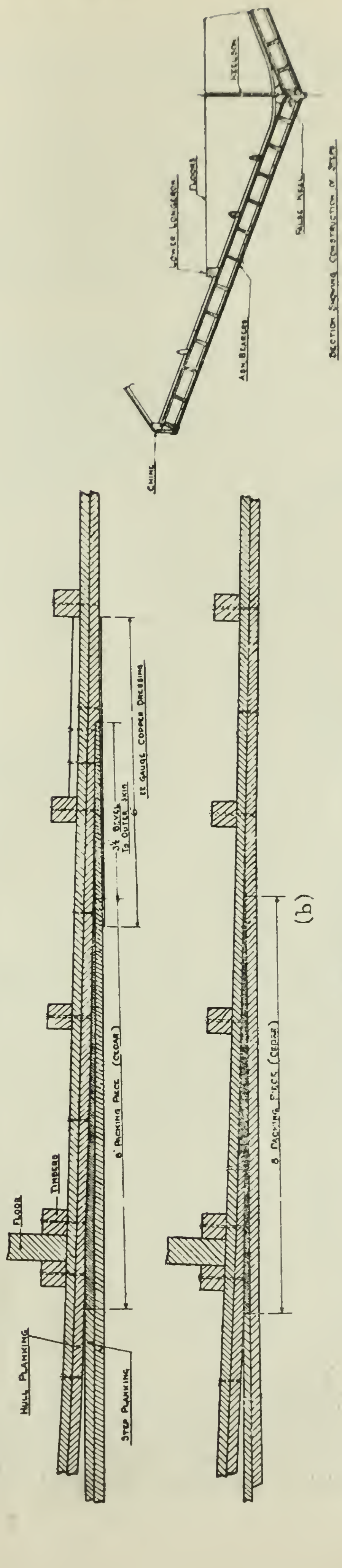

(a)
Hull Planking
Step Planking
Floor
Timbers
(b)
Junction of Step with Hull
Chine
Ash Bearers
Lower Longeron
Floors
Keelson
False Keel
Section Showing Construction of Step

FIG. 8.

From the point of view of safety under full load, the N.4 arrangement of steps is undoubtedly the better.

On the other hand, with the exception of the "Fury," serious accidents due to "taking off" in a stalled condition are rare, as flying boats are taken off generally well above the lowest flying speed, provided sea conditions are suitable. There is, however, a temptation to "take off" nearly stalled in a rough sea accompanied by a strong wind, in order to reduce the run on the water.

There is no reason, constructional or otherwise, why the steps on the "F" boats could not be altered to retain the above stable conditions, should such be desired.

The foregoing brief discussion has been concerned principally with the hull, under water lines, and overall dimensions. With regard to the upper portion, this, as has been mentioned previously, is largely influenced by reserve buoyancy, and space for passengers, cargo, and fuel. Once the dimensions have been settled, the whole hull should be given the best practicable streamline form.

Hull Construction

The various types of construction may be divided broadly into three classes: The "Porte" or fuselage type framework; the "Linton Hope" flexible type, a development of the construction used on fast motor boats and hydroplanes; and lastly, the more or less flat-sided box type.

A short description giving the underlying principles in these types of construction will now be given. It is hoped the detail design will be seen clearly from the illustrations.

The "Porte" or Fuselage Type.—So called because the main framework of the hull is of the usual braced fuselage type. Referring to Fig. 7, which is typical of "F" boat and "Fury" construction. The framework will be seen to consist of two upper and lower longerons, meeting at the sternpost and stem. Aft of the front spar, the sides and the top are braced in the usual manner with struts and wire or tie rods. Forward of the front spar, the sides are "N" girders, the top braced as above. Between and below the level of the lower longerons run the keel and solid keelson without a break, terminating at the stem and stern post. Deep solid floors on a level with top of lower longeron join up the keel, keelson and these longerons. The chine and fin top longitudinals which sweep into the lower longerons forward and aft, now complete the framework.

Continuous timbers run from chine to chine, notched through keelson on keel level and from chine to fin top longitudinal. A timber is fastened on each side of a floor, of which there is usually one at each strut position and one between struts, and two timbers between each floor. To these timbers are fastened the planking. Several fore and aft stringers are fitted, notched out to receive the timbers to which they are fastened. On the original "F" boats the planking on the bottom was double diagonal, continuous from stem to stern, fore and aft, and to chine athwart ship, and the step planking added separately as shown in Fig. 8a. The fin tops and sides to just aft of rear spar were planked with 3-ply, aft of which the sides were covered with doped fabric, a solid mahogany washboard about 1ft deep extending along this length. The top, from aft to pilot's cockpit, was covered with doped fabric laid on fore and aft stringers, supported on formers, forward of which it was planked.

These boats were designed originally to operate from sheltered harbours, such as Felixstowe, for which they were quite suitable. When operations had to be carried out under less favourable sea conditions, several weaknesses became apparent. Water leaked into the joints on the 3-ply on the fin tops and hull sides, rotting and opening up the laminations. Similarly the wash boards split and

the fabric rotted. The fin tops were then planked double diagonal with mahogany and cedar, and the sides either planked single fore and aft and fabric covered or planked with "Consuta."

Owing to the lack of flexibility in the fin construction the joints at the chine and waist line gave trouble. Fig. 9, *b* and *c* respectively. This was overcome on the "Fury" by doing away altogether with these joints, and extending the planking round the chine. Fig. 9 *a* up to the top longeron, supported at the waist line as shown at *d*. Fig. 10 shows the arrangement. Also the skin landings at the keel as shown at *g* was found much more satisfactory than as shown at *f* (Fig. 9).

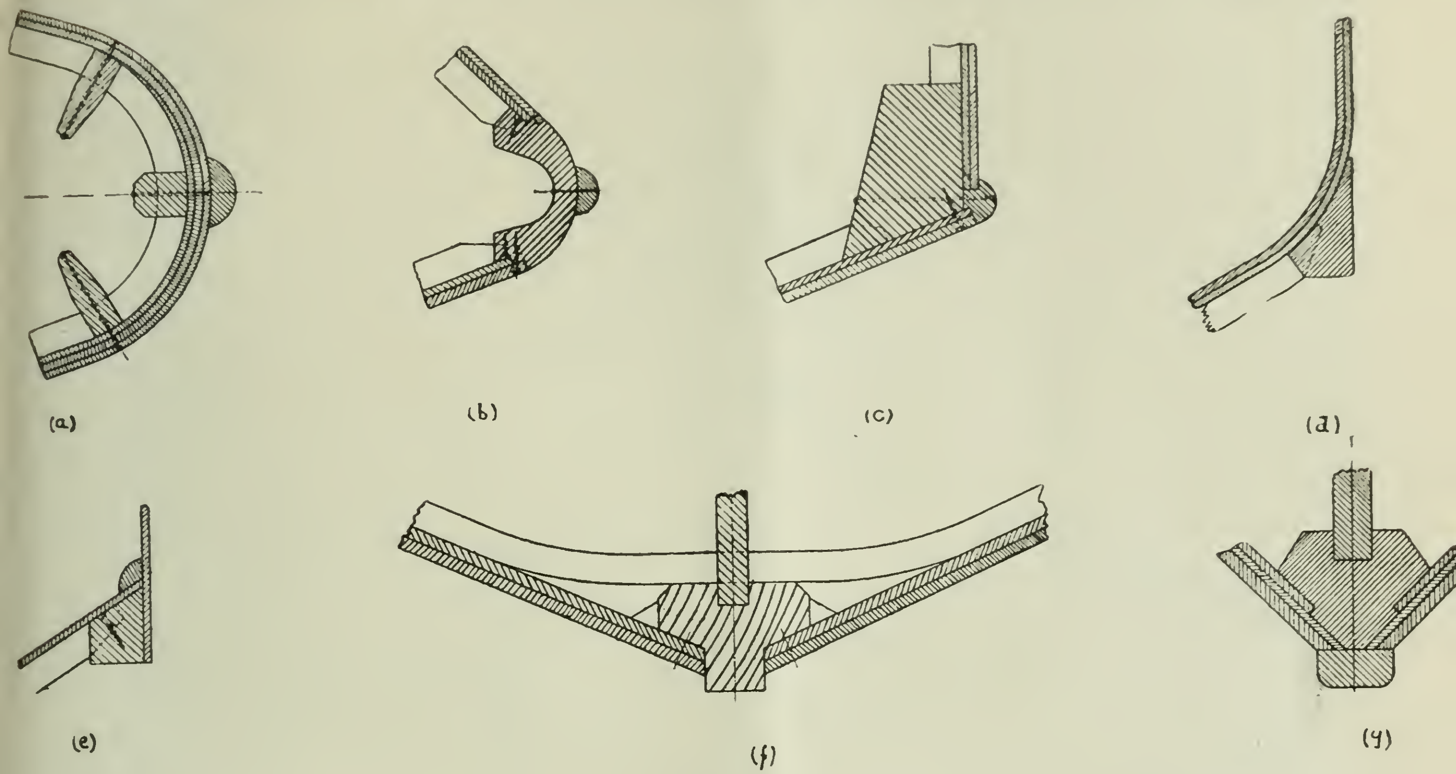


Fig. 9.

The solid transverse mahogany floors were fastened to the lower longerons by metal angle plates and notched out for two-thirds of their depth from the bottom to fit over the keelson, which was notched out one-third of its depth from the top. The corners at the joint were filled with square fillets for the whole depth. This was a weak and uneconomical form of construction, resulting in frequent splittings of the floors. On the "Fury" the keelson and floors were of built-up lattice girder construction, the top rail of the floor continuous over the top rail of keelson (Fig. 10).

Trouble was experienced with the steps, which in some cases were wrenched off the bottom. The steps were of the open type, and the planking fastened with wood screws to the bearers, which were of ash, spaced about 6in. apart and through-riveted to the hull timbers and bottom. Fig. 8 (*b*) shows the arrangement of planking whereby this trouble was eliminated. The inner hull skin is continuous from stem to stern. The outer skin continuous from stem to end of steps, forming the outer skin of the step. The outer skin of the hull bottom in the same way forming the outer skin of the after step.

These modifications were carried out in the design of the "Fury" hull. As was to be expected, she proved to be an excellent seaworthy boat, and in all ways a noteworthy advance on the "F" boat design.

Apart from the hull construction it will be noticed from Fig. 7 that the

wing roots, from the foot of engine bearer struts, with the keel and longerons form a complete structure. This is unique to the "Porte" boats.

"Linton Hope" or Flexible Type of Construction.—In this type the framework is totally different to that of the "Porte" type. It consists (Figs. 11 and 12) of a keel and keelson continuous from stem to stern post, and a large number of fore and aft stringers distributed evenly round the periphery of the hull, which

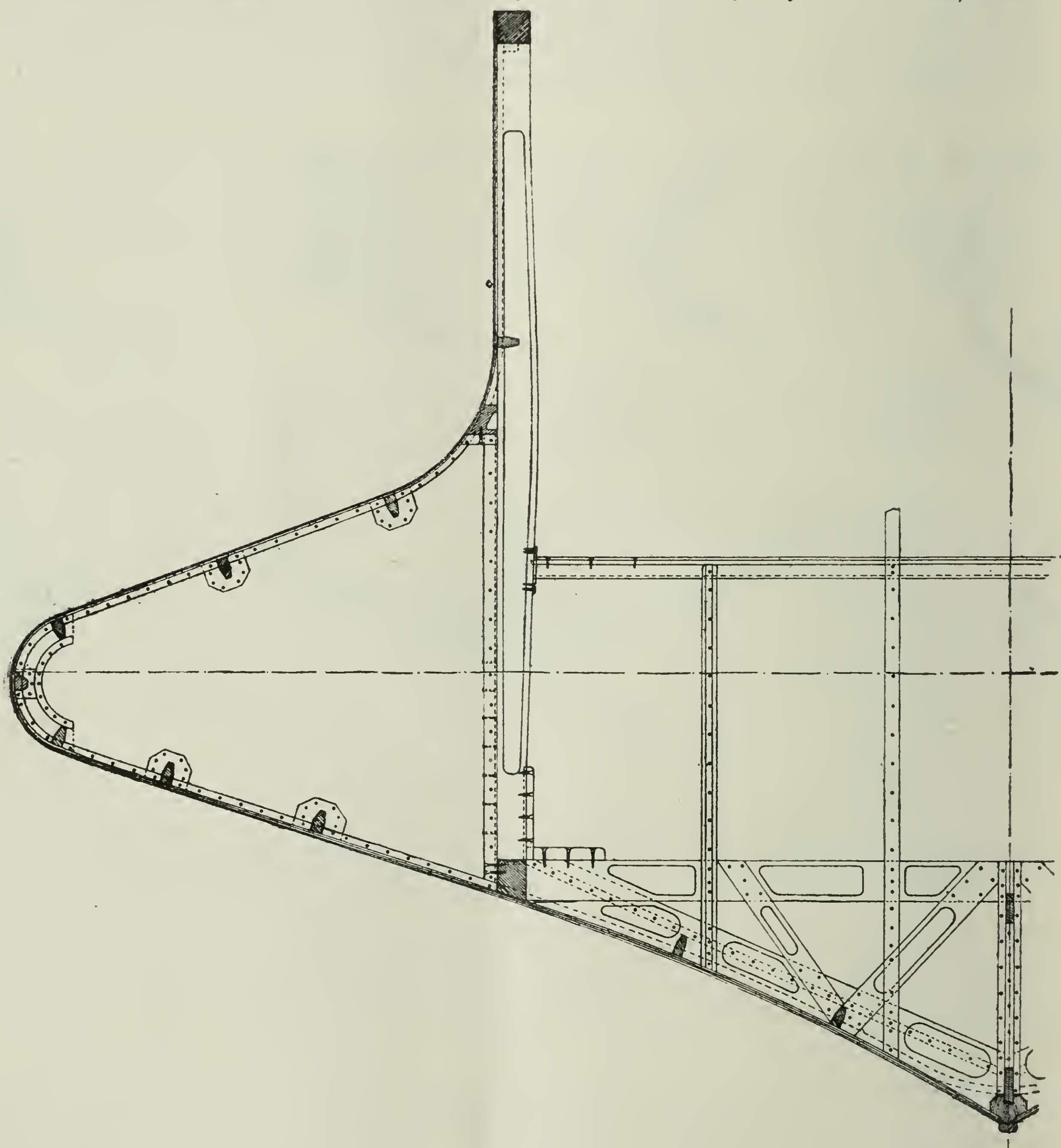
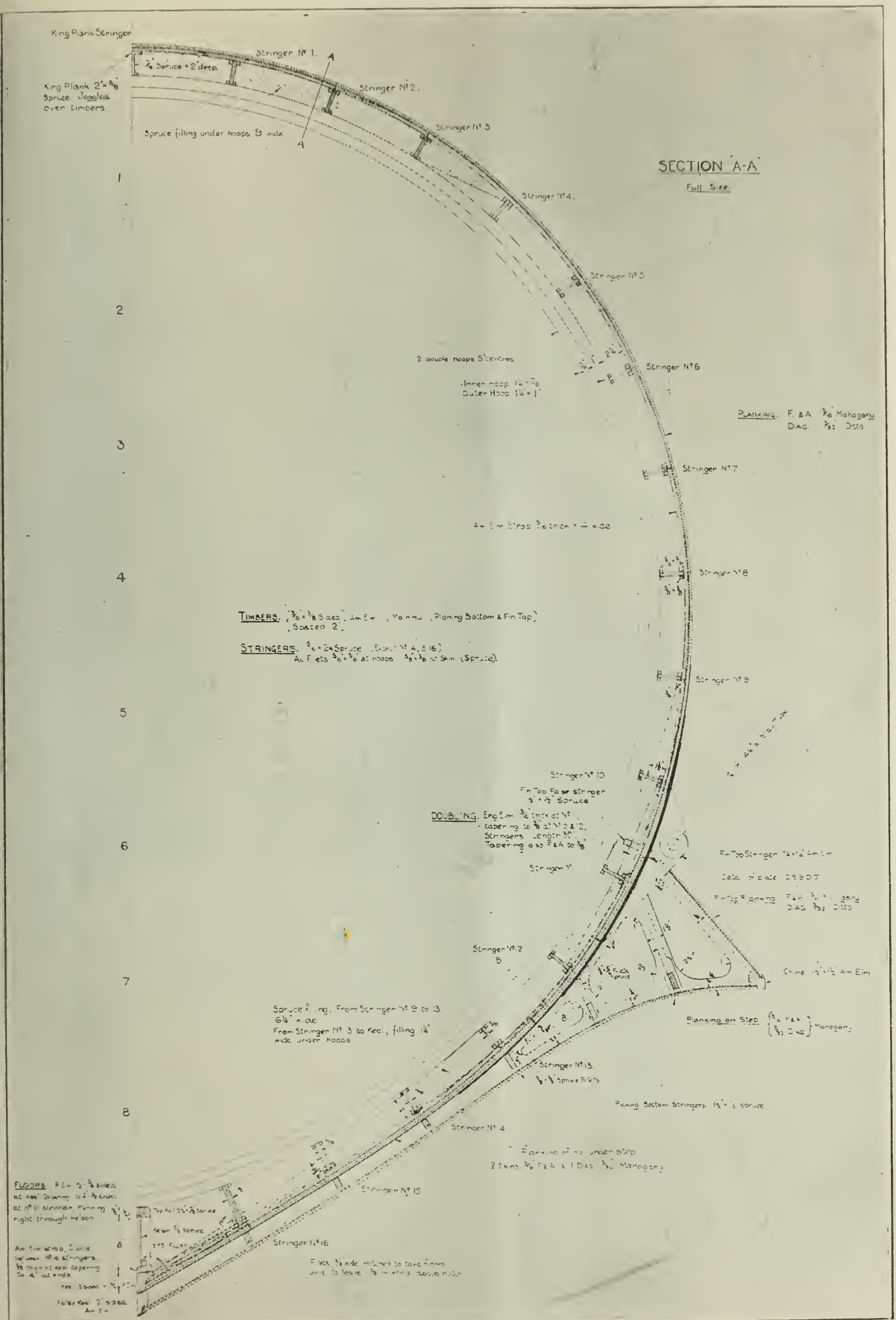


FIG. 10.

is of a streamline form. Small section timbers closely pitched are bent round these stringers, ending at the keel. To these timbers is attached the planking, of which generally the inner is diagonal and the outer fore and aft. Floors spaced at intervals varying from 8in. in forebody and tail to 4in. at the step, run round the lower half of the bottom, continuous through the keelson. At intervals of three or four feet continuous single hoops are fastened to the top of keelson and stringers. Where the wing roots spars enter the hull, double hoops, one each side, are fitted as above. This forms the hull proper. The planing surface, which



SECTION 'A-A'
Full Size
King Plank Stringer
Stringer Nº 1.
Stringer Nº 2.
Stringer Nº 3
Stringer Nº 4.
Stringer Nº 5
Stringer Nº 6
Stringer Nº 7
Stringer Nº 8
Stringer Nº 9
Stringer Nº 10
Stringer Nº 12
Stringer Nº 13.
Stringer Nº 4
Stringer Nº 15
Stringer Nº 16
Spruce filling under hoops 5" wide
2 double hoops 5" centres
TIMBERS.
STRINGERS.
DOUBLING.
PLANKING.
Planking on Step
FLOORS
1
2
3
4
5
6
7
8

FIG. 11.

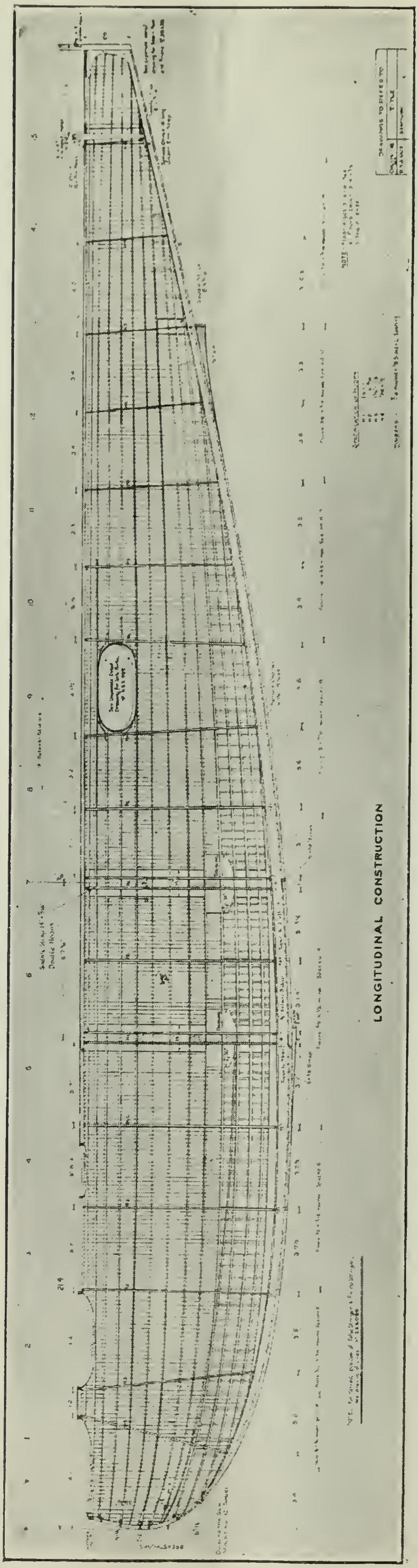


Fig. 12.

extends from the bows to the main step, is attached to 3-ply formers on the hull bottom, and at the step, which is closed, to fore and aft bearers. The fin top is straight and the chine ends at the step. The after step is attached in a similar manner. This construction leads to a double bottom in way of the fins.

In later developments of this type the chine is continuous from stem to after step, as in the "Porte" boats. The planing surface is continuous from stem to after step, at all points kept well clear of the hull, thus forming with the hull and fin tops a complete double bottom. The 3-ply formers are replaced by bent hoops and the top fin curved as shown in Fig. 13. The steps are open, and planking carried out in the same way as in the "Porte" boats, thus avoiding external joints.

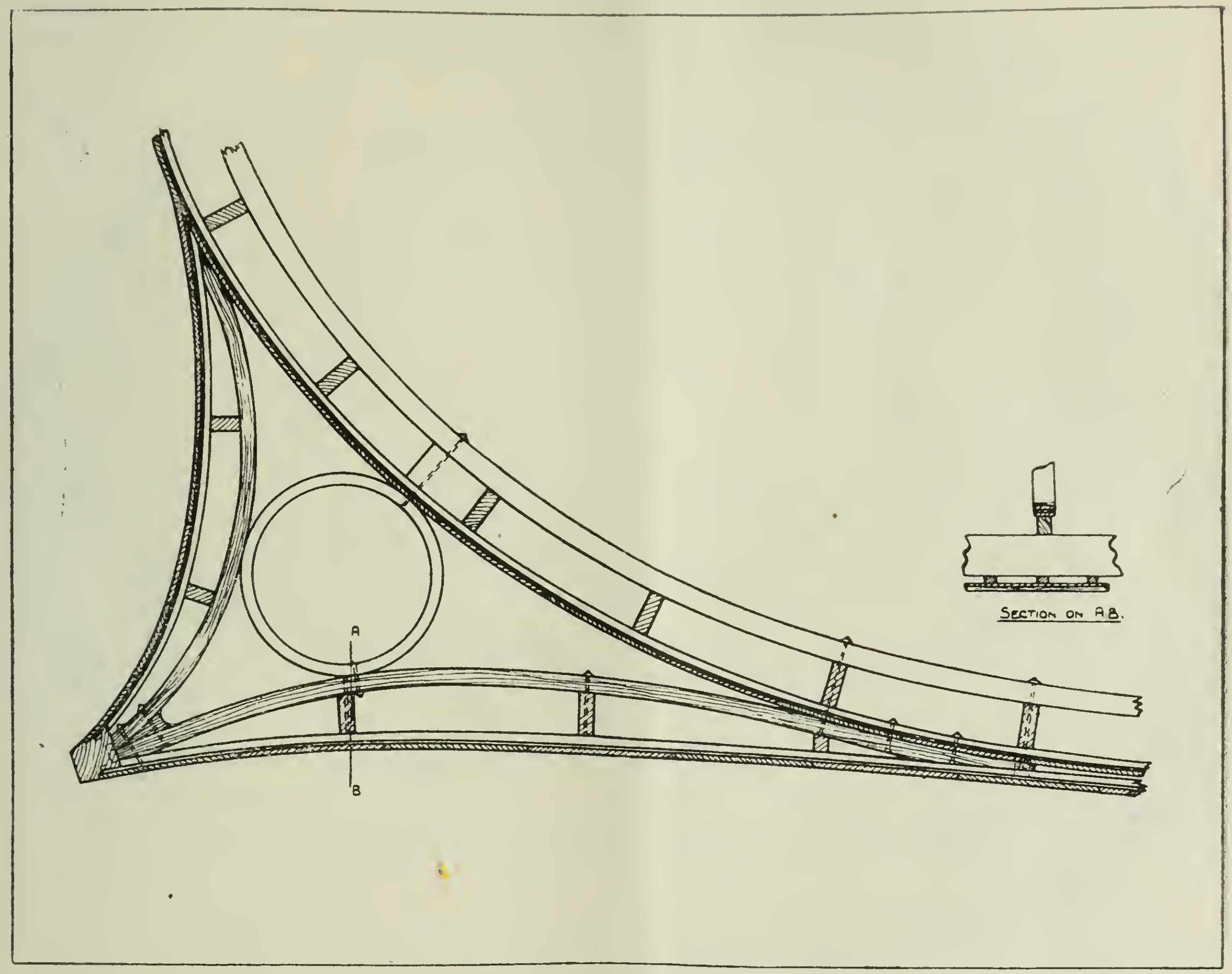


FIG. 13.

It will be noticed there is no similar wing root structure, as in the "Porte" boats.

A further example of a hull following usual boat practice construction is shown in Fig. 14, from which it will be seen the fore and aft stringers are closely spaced, and there is practically no keel and a very light keelson. The timbers are more openly spaced, and in way of and above the floors, run on the inside of the stringers. At intervals there are 3-ply continuous web frames, between which there are 3-ply web floors, stabilised with intercostals as shown. The planking at the bottom is not jointed at the keels, but runs from chine to chine. In this type also there is no wing root structure.

The "Vickers" or Box Type of Construction is shown in Fig. 15, from which it will be seen there are no projecting fins as on the "Porte" or "Linton Hope" type hulls. The framework consists of top and bottom longerons,

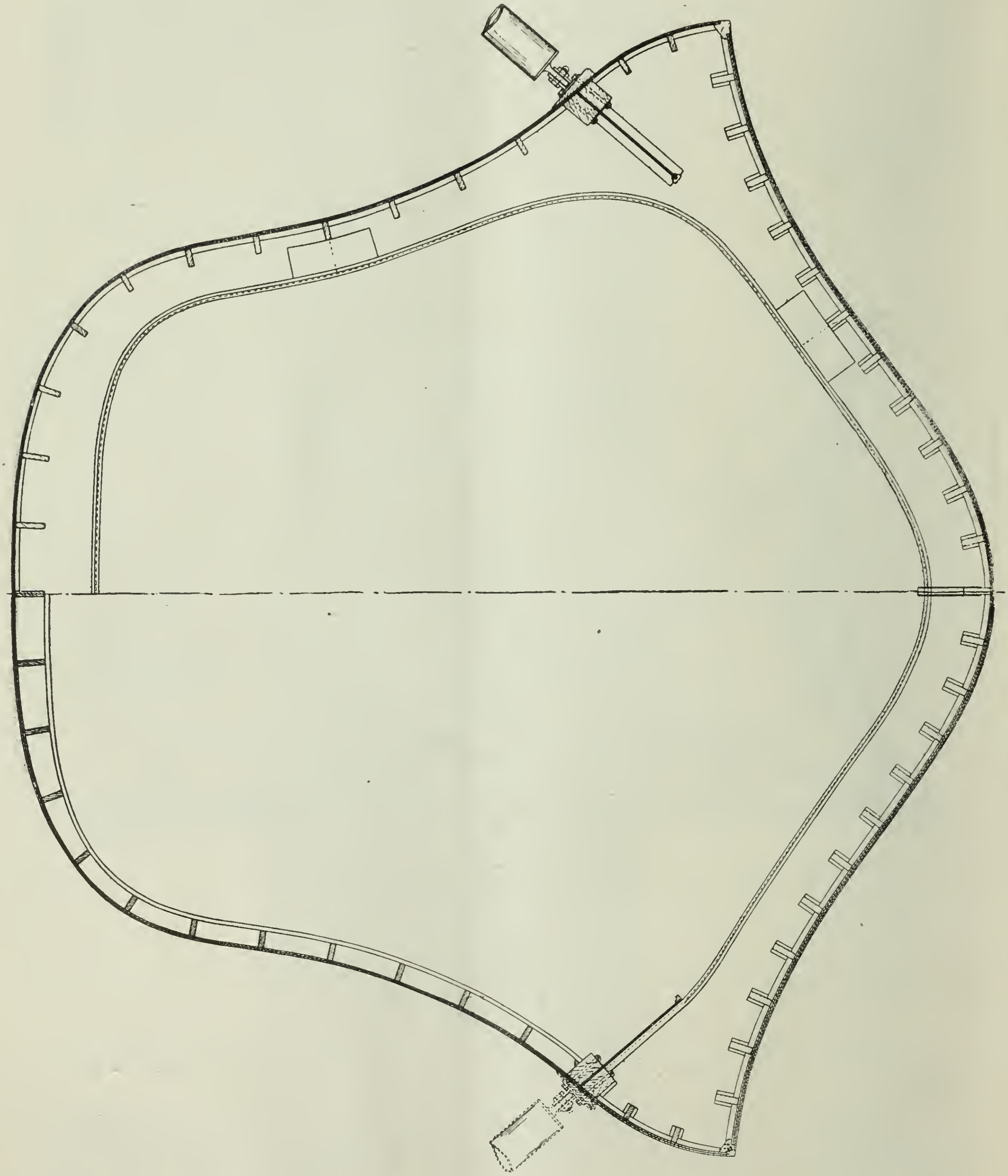


FIG. 14.

a keel and transverse box-shaped diaphragms at intervals of 3ft. Between the diaphragms are two floors, four heavy fore and aft stringers run over these floors and on top side of bottom rail of diaphragms. Timbers run fore and aft, under and fastened to these floors and diaphragms, to which is fastened the

"Consuta" sewn planking. The sides and top, which are flat surfaces, are also covered with "Consuta."

In all these types of hulls, several bulkheads are usually fitted. In the smaller boats, with the exception of, at the most, two complete bulkheads in the tail portion, it is not generally possible on the score of accessibility and weight to fit other than transverse dwarf bulkheads. Of the latter, two should straddle the main step; the disposition of the remainder, if any, will depend upon the internal arrangement of the hull. These are sufficient to limit the travel of any water in the hull, due either to abnormal leakage or slight damage to the bottom, provided such water can be kept under by the bilge pump. In large passenger boats, where safety will be the first consideration, the passenger accommodation will be located between complete bulkheads, fitted with watertight doors.

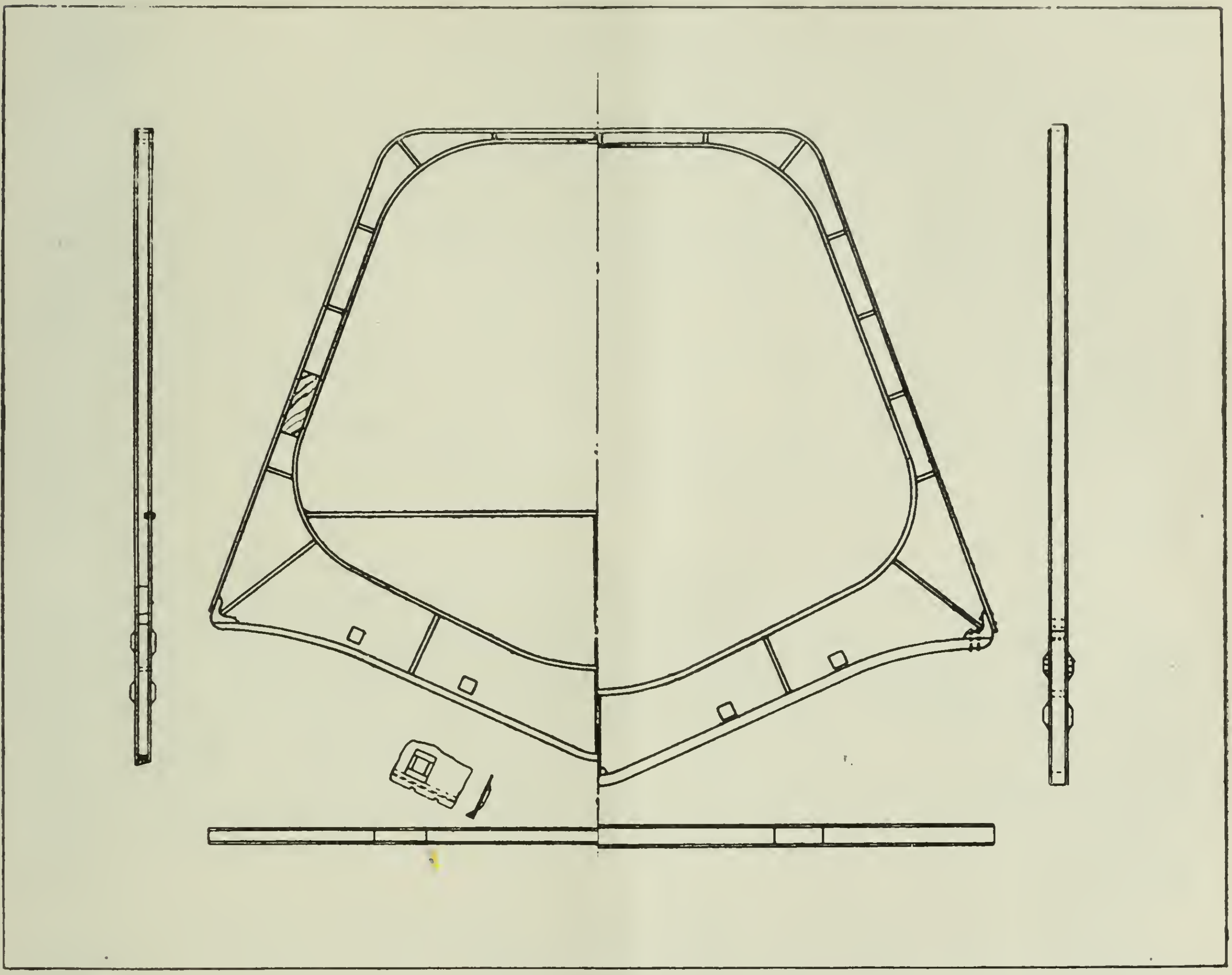


FIG. 15.

In the event of a serious crash, any form of bulkhead would most likely give way against the inrush of water, but, depending upon the extent of the damage sustained, may so reduce the rate of flooding to enable the passengers and crew to get clear.

Unless of flexible construction, the joint between the bulkhead and any part of the hull, which in itself possesses a certain amount of flexibility, should not be rigid, but should be capable of "giving" to the working of the hull. This may be arranged for either by allowing the bulkhead to slide between two faces round its periphery or by interposing a strip of strong Willesden canvas between the bulkhead and the hull shell. Bulkheads may be either of solid construction

or built up of Willesden canvas attached to a spruce framework, a form combining strength and lightness (Fig. 10).

As a further safeguard against flooding, due to extensive under water damage, double bottoms are unquestionably of great value, and in all types of construction should be fitted in way of the passenger accommodation. However, until boats of relatively much greater displacements are contemplated, it would seem unnecessary to have double bottoms extending along the whole L.W.L., as apart from the inevitable increase in weight, the majority of hull breakages occur at the step, the construction of which usually forms a shallow double bottom, and the evidence of serious failures of single bottom hulls is insufficient to insist upon their adoption.

Before discussing the various types of construction outlined above, a few remarks on the question of hull resilience would not be out of place. The greatest loads on the planing surface arise either when " taking off " or landing in a rough sea. Now these loads act for a very small interval of time, normally, in either case, less than 30 seconds. It is therefore preferable to allow the hull to " give " a certain amount, rather than to provide sufficient strength to rigidly withstand these loads. Otherwise the scantlings required would mean a prohibitive increase in weight. Further, a more or less flexible structure absorbs and dissipates energy due to shocks without imposing severe local stresses in any of the members. As the required degree of flexibility is beyond calculation, successful hulls provide the only source of information ; bearing in mind the well-established fact that where a flexible joins a relatively rigid portion of a structure, the change of section should be gradual and not abrupt. In other words, discontinuity of section should be avoided.

It is now proposed to give a brief outline of the main features in the construction of each type described above.

" Porte " Type

(1) The hull framework, wing and tail plane structure, forms a complete and simple braced structure, the overall strength of which is independent of the hull skin. Whether resting on a trolley or in a dry dock, the resultant weight is transmitted to the keel and bottom longerons ; at the wing roots, where it is supported, no other external support is required. No loads are taken by the hull planking, thus avoiding distortion and increased liability of leakage. In flight the weight of hull, passenger cargo, and fuel is transmitted to the point of attachment of the lift wires at the wing roots, through the framework, alone. The only drawback to the wing root structure is the absence of a clear passage through the hull.

The tail setting remains sensibly constant. In the earlier boat-built types, the tail portion sagged under load, due to skin soakage, and the effect of a possible variation in temperature between the top and bottom surfaces. With adjustable tail planes, this is of less importance.

(2) The required degree of strength and flexibility is obtained with an acceptable weight.

(3) As the overall strength of the hull is independent of the skin, cockpits, port holes, etc., present no difficulties and require little compensation.

(4) Over the greater part of the under water surface the only external joint of the skins is at the keel, at which position experience has shown least trouble is to be expected.

(5) The interior of the hull is well lighted, owing to the doped fabric top.

(6) Above the top of the fin, the sides are flat, but if planked with two skins

are amply strong to withstand any seas likely to be met, which would not otherwise seriously damage the boat.

(7) A considerable number of metal fittings, steel or duralumin, are used in the construction. These present no real drawbacks. They are readily inspected, and do not corrode or rust away in a few weeks as some critics would have us believe. With little attention they will outlive the hull.

(8) This type of construction does not readily admit of the fitting of a double bottom.

Linton-Hope, or Flexible Type

(1) Actually these hulls are flexible only in parts. The planing surface ends at the main step, which is closed, forming a rigid belt extending across the form from chine to chine, thus introducing a sharp discontinuity of section, resulting in serious local weakness. Further, the whole planing surface and part of the inner shell to which it is attached, forms a rigid girder, an abrupt discontinuity of section occurring at the waist line. In the latest types the chine is continuous from stem to after step, and the steps open, following the "Porte" type practice.

(2) Three-ply formers are used between the inner shell and planing surface. These break up rapidly under the action of water. Also, where the planing surface washes into the inner shell in the region of the keel, a pocket is formed which cannot be drained. These defects have been more or less eliminated in the latest hulls.

(3) There is no wing root structure as in the "Porte" hulls. On a trolley, or in dry dock, the weight of the wing structure, engines and fuel, if carried on the wings, has to be supported at the engine bearer struts if distortion of the hull is to be avoided. This leads to great inconvenience from the handling point of view, and in other respects may be compared with the "Porte" type under (1).

(4) Possibly of better streamline form than the "Porte" type.

(5) No metal fittings used in the construction.

(6) Lends itself readily to the fitting of a double bottom.

(7) The overall strength depends on continuity of stringers and shell. Openings in shell greatly weaken the hull unless heavily compensated.

(8) Interior of hull very dark.

(9) Skins on the planing surface are jointed at keel and chine, making four more joints than on the "Porte" type.

(10) The curvature of the hull sides and top provides greater protection from damage due to heavy seas breaking on the hull than the flat-sided type does.

The second type of hull of the motor boat or yacht form of construction calls for little comment. The hull is of a weak shape and the keel and keelson, as shown, are of inadequate strength. Three-ply frames and floors are useless. There is no wing root structure. It is a typical example of a design produced by a boat builder possessing no knowledge of flying boat hull requirements.

The Vickers, or Box Construction.

(1) The shape is simple, therefore the cost of construction should be relatively low.

(2) Owing to the absence of projecting fins the air resistance should compare favourably with the other types.

(3) The top longerons are much too light where most strength is required.

(4) No keelson or floors are fitted as in "Porte" type. Panels between

diaphragm are braced with "consuta." Strength depends entirely upon the joints at the longerons. Compensation for openings presents great difficulty.

(5) Owing to the lack of flexibility, to obtain the necessary strength, the weight will be relatively great.

(6) The internal wing root structure is of little use owing to the poor design of the hull framework.

The two latter types of construction would seem to possess all the disadvantages and none of the advantages of the "Linton Hope" or "Porte" types respectively. Sufficient experience with the "Linton Hope" type of construction has not yet been obtained to enable a fair and definite comparison to be made with the "Porte" type. As both types possess features of distinct merit, a suitable combination of these may ultimately lead to a type of construction superior to either.

Hull Weights

It has been customary to adopt as a criterion for the comparison of different types of hulls the percentage weight of the gross weight. This is inadmissible unless the types compared are of equal water performance under the same sea and weather conditions, and of equal strength and durability. Further, such items as wing roots, bulkheads, flooring and seats, having no direct bearing on the type of construction, should be omitted.

Fig. 16 gives in diagrammatic form the detail weights of the F.5 and P.5 flying boat hulls. As will be remembered the F.5 is of the fuselage type of construction, whereas the P.5 is of the Linton-Hope type. The hulls are of the same length, 45ft., and of approximately the same displacement. Both flying boats were constructed to conform to the same specification.

It will be noticed that the weight bare of the F.5 hull is considerably more than that of the P.5. This difference may be accounted for mostly by the larger beam of the F.5, in this instance about 33 per cent. greater than the P.5.

Comparing the percentage weights of the two main items of each type—the framework and skin, these are the same to all intents and purposes. Now the design of these two hulls differs in the position of the load waterline for reasons explained under Hull Lines and Dimensions. Should, therefore, it be found from experience with the Linton Hope types of hulls, as it probably will be, that the load water line is too high for rough sea work, then it may be found the additional weight involved in lowering the low water line the necessary amount would make this difference negligible.

In other words, it is not proved that the "Linton Hope" type of construction is lighter than the "Porte" type, as is generally stated and accepted. Increased air performance can always be obtained by flimsy hull construction. Until the P.5 type, both as regards construction and dimensions for a given displacement, has been proved equal, under the same conditions, ashore and afloat, to the "Porte" type, arguments based on percentage weights are misleading.

Curve Fig. 17 shows the weight of "Porte" type hulls actually constructed, plotted against the length, from which it will be seen that the weight of hulls between 20ft. and 40ft. is nearly proportional to the length, after which and over the range covered the weights vary roughly as the square.

For preliminary design purposes, a rough rule for the weight in lbs. of this type of hull, including bulkheads, wing roots, is given by the square of the length in feet.

HULL WEIGHT ANALYSIS.

F.5. (BARE HULL).

1555 lbs. (100%)

- Frame Work. 724 (46.6%)
  - Longerons 158
    - Keel ... 44
    - Keelson ... 29
    - Sternpost 5
    - 236
  - Bracing.
    - Nose ... 36
    - Wing Roots 20
    - 56
  - Stringers and Timbers.
    - Stringers ... 29.5
    - Floors ... 55
    - Fin Timbers & Bottom Ribs 200
    - 284.5
  - Metal Fittings. 147
- Skin. 563 (36.2%)
  - Sides 130
  - Bottom
    - Inner 158
    - Outer 190
    - Steps 85
    - 433
- Hood. 50 (3.2%)
- Fabric 40 (2.6%)
- Sundries. 177 (11.4%)
  - Fastenings.
  - Varnish
  - Glue
  - Paint

P.5. (BARE HULL).

1230 lbs. (100%).

- Frame Work. 560 (45.6%)
  - INNER HULL
    - Keel and Girders 60
    - Timbers ... 89
    - Floors ... 30
    - Stringers ... 131
    - Stem 3
    - Hoops 60
    - Saddles ... 29
    - Chocks ... }
    - Coambings ... } 55
    - Sternpost ... }
    - 457
  - FORE PLANING SURFACE.
    - Timbers ... 21
    - Frames ... 8
    - Stringers ... 21
    - Chine ... 26
    - Step ... 15
    - 91
  - AFTER STEP.
    - Chine and Mouldings 6
    - Step 4
    - Sundries 2
    - 12
- Skin. 456 (37%)
  - Inner Hull 316
  - Fore Planing Surface 128
  - After Step 12
- Fastenings.
  - 103 (8.4%)
    - Inner Hull 77
    - Planing Surface 24
    - After Step 2
    - 103
  - 66 (5.4%)
    - Varnish
    - Paint
    - Glue
    - Sundries
  - 44 (3.8%)
    - Sheet Brass 8
    - Metal Fittings 25
    - Fabric 11
    - 44

FIG. 16.

Hull Stresses

If the value and disposition of the maximum water forces to which a hull was subjected, under the worst conditions likely to occur during normal operations, were known, simple calculations could be made to ensure adequate over-all strength. Such forces however are not known, even approximately.

The worst loads likely to occur when at rest or taxi-ing at low speeds in a rough sea may be calculated, assuming the boat is supported on a wave crest at each end and the wave hollow touching the keel amidships. The buoyancy

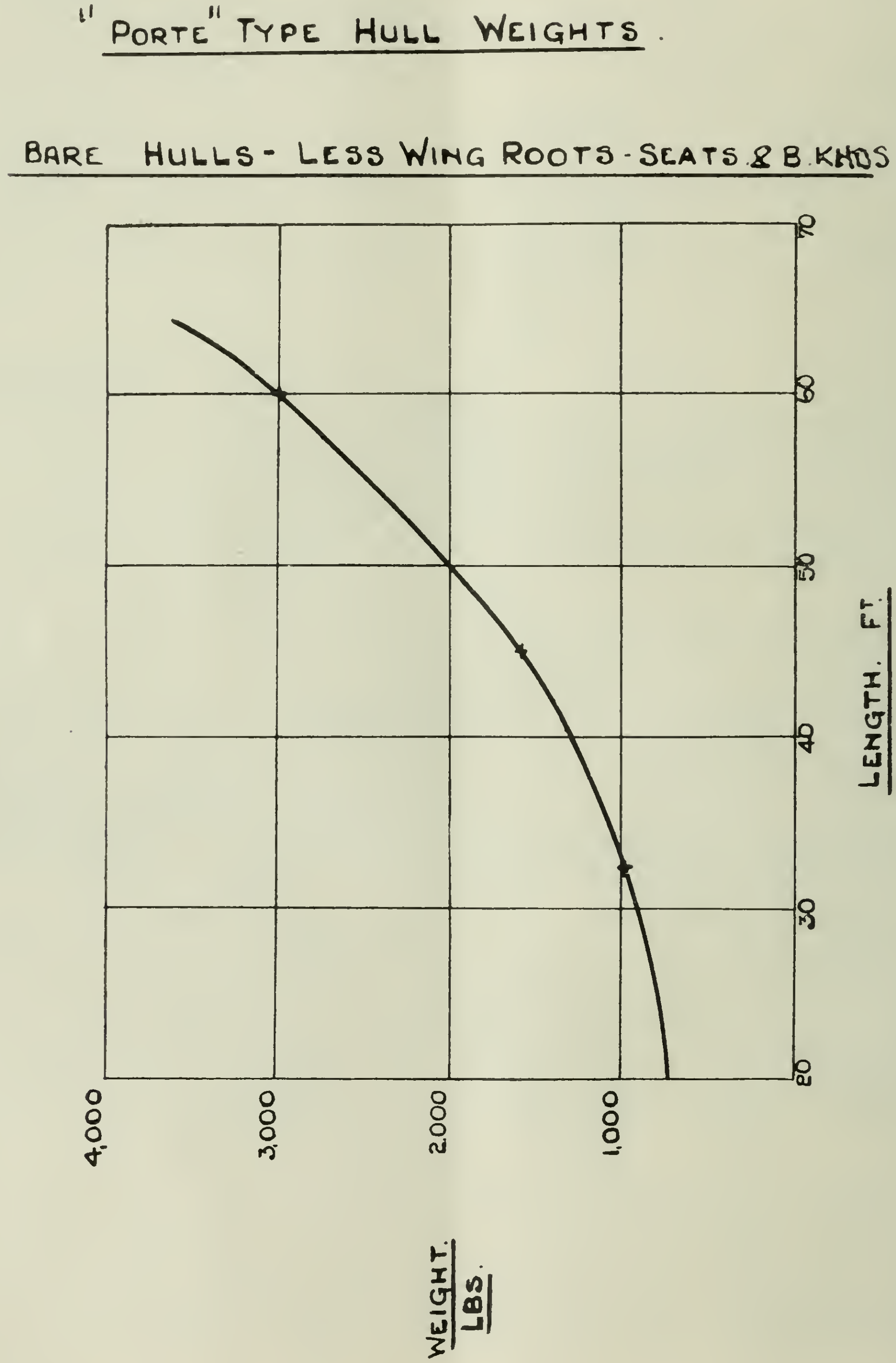


Fig. 17.

and static load curve may then be drawn from the known distribution of the various weights and the bending moments and shear forces obtained.

Unfortunately, almost identical conditions may arise when the boat bounces from one wave to another, the result of leaving the water more or less stalled, when pitching in a rough sea. In this case an estimate of the water forces does not seem to be possible, thus rendering the above method of calculation of little use.

On the other hand, an analysis, on the above lines, of hulls of proved strength might lead to figures useful as a basis of comparison.

When considering a hull from the strength point of view, the important fact should not be overlooked that a flying boat is intended to fly, and to justify its existence as a flying machine from either a military or commercial standpoint, must in addition carry a reasonably useful load. A suitable compromise has therefore to be made between weight and allowable strength. In this respect a flying boat hull is analogous to the landing chassis of an aeroplane. Experience has shown there is no trouble in obtaining sufficient over-all strength for an acceptable weight, and with good design the latter need not exceed 12 per cent. of the gross weight. With the exception of checking the tail portion for the maximum air load likely to occur on the tail plane, no strength calculations were attempted on the "Porte" hulls. Once the type of construction was settled, the necessary strength was eventually obtained by a process of elimination of weaknesses and refinement in detail design.

The fixing up of the scantlings for hulls of other displacements is then a comparatively simple matter, in which the elementary principles involved in the comparison of similar structures give considerable guidance.

All-Metal and Composite Hulls

Hulls may be constructed either entirely of wood or metal or in varying proportions, a combination of these materials. Up to at least 100ft. long a rough calculation shows that for the same strength, the weight of a steel hull would be considerably greater than the corresponding wood or composite built hull. Further, the cost of an experimental all-metal hull would be probably five times and even on a production basis would not likely be below twice that of a similar wood hull.

In the case of all steel hulls, the main disadvantages arise because of the following facts :—

(1) To keep down the weight, the frames and plating would require to be of small gauge of the order of 20 to 22. To prevent local weakness, such as buckling, the plates would need to be corrugated, the rolling and forming of which to the required shape would be very costly.

(2) Unless rusting and corrosion could be prevented the scantlings would have to be increased beyond that necessary for strength.

(3) As it is not possible to caulk the joints in such thin gauge steel plate they would require to be welded, and left in the unannealed state, thus leading to local weakness.

(4) In the event of damage to the hull, repairs would be more difficult, costly and take much longer to execute.

The only apparent gain would be the elimination of hull soakage and possibly abnormal leakage resulting from the planking opening up in tropical countries.

It would seem, therefore, that unless under circumstances which so far have not arisen, the resulting disadvantages are too overwhelming to warrant the use of steel.

By the use of duralumin the scantlings could be increased, and without sacrificing strength the weight might be slightly reduced. Apart from this, and the likelihood that the sea water corrosion would be of little importance, provided the material was properly worked, heat treated and protected by a covering of bituminous varnish or non-metallic paint, a duralumin hull would suffer from the same disadvantage as outlined above.

With a composite hull of "Porte" type construction, when the frame, keelson and floors are of duralumin and the skin wood, as at present, a consider-

able gain in strength and reduction in weight could be obtained at an acceptable increase in cost.

Wing Tip Floats

Unless the hull is of relatively great beam or fitted with extensions in the form of a parallel or tapered thick wing section, added to the chine in the region of the main step, the metacentric height will be negative. Under these conditions the boat, when at rest on the water generally, will not remain on an even keel, but will heel over to port or starboard until the capsizing moment is balanced by the moment due to water, displaced by the float, when equilibrium will be established.

The "Porte Baby" and to a lesser extent the "Fury" remained usually on an even keel when head to wind, but the equilibrium was more or less neutral, a small disturbance bringing on the conditions as above.

The requirements are simple and easily provided for in the design and are as follows:—

(1) When at rest on the water the wing tips should be clear of moderate waves, say 3ft. to 4ft. high. There is really no reason why the wing tips should extend beyond the wing tip floats.

(2) With a cross wind, same as above.

(3) Should be just clear of the water when planing at high speeds.

All the data is easily obtainable, when the necessary float displacement and water line will be fixed.

With regard to the best form for the float; experience showed that the hydroplaning efficiency should be low. It should cut through the waves with small lift and when the boat is rolling should enter the water with as little shock as possible. The plan view should be of streamline shape with pointed bows to reduce air drag. The sides flat. The keel profile more or less parallel to the wing chord and slightly rounded into a straight stem. The chine should start from the foot of the stern post and rise gradually at first, then at the bows swept well up to the stem. This will allow of fine sections forward. The sections throughout may be either convex and fairly flat at the chine or of "Vee" formation. On the larger boats it has always been found possible to attach the floats directly to the wing spars, as within limits the distance of the lower plane from the water does not increase with the size of boat. This is preferable to suspending them by a complicated system of struts and bracing.

The construction is simple and may consist of a keel, two chine and top members, stem and stern post. Several floors and side frames, two of which at least should form three-ply bulkheads, diagonally stiffened. Timber running across the keel from chine to chine and from chine to top members. The sides may be single fore and aft planking covered with fabric, and the bottom either double diagonal, or inner, diagonal and outer fore and aft planking. As the float top is snug up against the lower surface of the wing, fabric covering is sufficient. Finally drain plugs should be fitted to each compartment.

With regard to weight, Mr. A. Thom has given the following formula, which represents the facts fairly well. If W is the weight in lbs. and L the length in feet, then $W = 0.36\ L^{2.5}$.

Water Rudders

On a single-engine flying boat, especially where there is a marked turning tendency due to slipstream effect on the fin and rudder, a water rudder is almost a necessity for manœuvring at slow speeds. To be really of much use, the area

required would seem to be much larger than warranted by the extra air resistance and weight involved. At high speeds the air rudder is the more effective.

With flying boats, twin engine or more, and outboard propellers, water rudders are quite unnecessary, as the air propeller turning movement is greatly in excess of that obtained from a water propeller of reasonable dimensions. Moreover, in any commercial service it would be unnecessary for a large flying boat to be able to manœuvre at slow speeds in restricted water, as the motor-boat tender would then take control, analogous to the case of the large steamship entering or leaving port.

For the same reason, auxiliary propulsive power on the water is superfluous. For example, take the case of a flying boat of 12,500 lbs. displacement; the weight of a petrol engine reversing gear, stern tube and propeller of sufficient power for, say, eight knots, a probable minimum speed to allow for steerage way and adverse currents, would be of the order of 2 per cent. to 3 per cent. of the gross weight. Also a water rudder would be essential, which in combination with the water propeller would lead to a considerable increase in air drag.

Bilge Pumping Arrangements

A certain amount of water leakage takes place in all classes of ships. As would be expected, the flying boat hull, necessarily of a light form of construction and working under severe sea conditions, would prove no exception. The leakage at any time will depend upon the design, workmanship, the materials used in the construction, age, and the treatment it has been subjected to in the past.

In a well-designed hull in good condition the normal leakage is quite small. In the case of the "Fury," when riding at her moorings in a gale which lasted for three days, the leakage was barely a bucketful per day. Under normal conditions the leakage was so slight that it had to be mopped up with "waste."

The arrangements adopted to deal with the leakage depend upon the size of hull, number of bulkheads, whether of single or double bottom construction, and steps opened or closed. In each watertight compartment provision should be made for the water to drain to the lowest point relative to still-water level. To ensure this, the passages should be of ample area to prevent choking up, and be easy of access. Further, in double-bottom hulls, the inner and outer skins at the keel should be kept well apart to avoid the formation of a wedge-shaped pocket which cannot be easily drained. The pump, which may be either of the plunger or semi-rotary type, preferably the latter, should be placed amidships in a position get-at-able by any member of the crew, and as close to the bottom of the hull as can be conveniently operated, to minimise priming troubles.

In a single-bottom open step boat, the bilge suction pipe may be either a flexible rubber hose pipe of sufficient length to reach all the compartments, which are emptied in turn, or permanent duralumin pipes may be laid to each compartment. In both systems the suction end of pipe should be fitted with a shallow strainer, and to advantage a light non-return flap valve. The discharge pipe should be led overboard at a position on the ship's side well above the load water-line.

When there is a double bottom, which may extend from steam to after step and further be sub-divided into a large number of watertight compartments, any system will become more complicated. When this type of hull is stowed ashore, drain plugs on the outer bottom may suffice, but when lying for long periods at moorings, means must be provided for draining the space between the inner and outer bottoms. Otherwise, should leakage happen to be bad, useless weight would be carried in the flight.

Probably the arrangement of least weight would be to have fixed to the inner bottom stand pipes, taking care provision was made to allow the entry of air into the space between bottoms, to which could be attached the flexible rubber hose pipe mentioned above.

However, the simplicity or otherwise of any arrangement adopted will be influenced largely by considerations of weight and the duty for which the boat has been designed.

Power Plant

With the exception of the fuel supply to, and starting up of, the engines, only minor difficulties are experienced in the power installation which are not met with in the corresponding aeroplane.

In the more general case, where the engines are mounted between the planes, the main fuel tanks will be inside the hull and the usual gravity tank in the upper plane. On short range boats it may be possible to carry all the fuel either in tanks on the top plane or in the engine nacelles.

While this is a most desirable arrangement, giving increased cubic capacity in the hull for passengers and cargo and leading to a very simple petrol system, it should be remembered that inertia loads, due either to getting off or landing in a rough sea or with bad landings, are transmitted to the hull via wing root struts which are inclined at about 45 degrees and connect the foot of the engine mounting struts with the keel. There is therefore a strict limit to the amount of fuel which can be carried in this way if the weight of these and related members is not to become excessive. In some landing tests on flying boats carried out in America, the deceleration reached as high a value as 7 " g." With the aeroplane, on the other hand, this arrangement of fuel tanks might be advantageous, as the engines are generally vertically over the landing wheels.

In the general case, that is with tanks inside the hull, the suction head may be as much as 8ft. to 10ft. if the pumps are placed outside the hull in a position free from spray and other interference, such as from the wind screen in the forward cockpit. The delivery head may also range from 10ft. to 18ft.

Assuming the fuel is discharged to the gravity tank from the main tanks and thence by gravity feed to the carburettor, this may be done in three ways, viz., compressed air, centrifugal or plunger pumps.

Tanks under air pressure are excluded, as, apart from other disadvantages owing to the large cubic capacity required and the high delivery head, the weight would be excessive. If compressed air must be used, then a modified " Autovac " system should be adopted.

If centrifugal pumps are used placed outside the hull, then, owing to the large suction head, experience has shown they are unreliable, no matter how carefully all joints on the suction sides are made. For the same reason, priming is a difficult matter. To get over the suction head difficulty, the pumps may be placed inside the hull and level with bottom of tanks, and driven by a windmill through two sets of right angle bevel gearing. Due to the high pump speed, the gearing would probably have to be reduced in the ratio of about 3 : 1 to avoid whirling of the connecting shaft. Alternately, the pumps might be driven by electric motors, but this leads to further troubles. In any case, unless the pumps do not leak either when running or at rest, the petrol fumes due to leakage are a source of danger and discomfort to the crew.

On all counts, a well-designed plunger pump is by far the most satisfactory. It can cope easily with suction heads, without priming, up to at least 20ft. and any delivery head likely to be required. If a simple plunger pump is used driven by a windmill through a crank, and connecting rod, and valves automatic, these valves should be light discs, and given a small lift, and care should be

taken that there is no chance of any air lock on the delivery side inside of the pumps barrel or valve casing, otherwise, unless well primed, the pump will be most inefficient and may fail altogether to discharge.

The Swashplate plunger pump developed at the R.A.E. has an excellent performance, and is peculiarly suitable for use on flying boats. When the weight of fuel carried is of the order of 20 per cent. to 30 per cent., as it usually would be on long range boats, the overflow from the gravity tank should be selective, and under the control of the engineer. Thus by means of a simple indicator operated by the pilot, the change of trim with fuel consumption may be kept within small limits. For example, the "Fury" had tankage for 1,500 gallons of petrol. With tanks full and empty the C.G. travel was from .37 to .39 of the wing chord respectively, within which limits it could always be trimmed by the above arrangement.

When the flying boat services become established, the boats will either ride at moorings or lie alongside suitably designed wet docks. Hand starting gear under these conditions is awkward to operate and likely to cause considerable delay in getting under way. For these reasons it may be looked upon as a stand-by gear in case of emergency. Of all the various methods, mechanical or electrical, which may be proposed, the most promising and suitable to meet flying boat conditions is the small petrol engine starting set, in which the mixture is pumped into the cylinder at a pressure sufficiently high to turn the engine, the magneto on which also igniting the mixture. As weight is not of the same relative importance as in the small aeroplane, the compressor could be made of ample capacity to deal with any normal engine valve leakage. Such a set would not probably weigh more than 40 lbs. It could also be usefully employed in other ways, as driving a W/T generator or the bilge pump.

Operations

As commercial flying boat services, operating boats of suitable displacement and design, have not been established in this country, information as to the running and maintenance costs is not available; therefore a comparison in this respect between flying boat and aeroplane services will not be attempted.

However, a brief discussion of three practicable methods, in two of which a fair amount of experience has been obtained, and of the third, the most likely to be adopted in the future for large flying boats, will give a good indication of the relative costs from the differences in the organisation and equipment found to be necessary.

In the first system to be described, which was used almost exclusively during the late war, the boats when out of flight are stored in sheds in the usual way. Each boat has its own trolley, on which it may be wheeled from the shed, down the slipway to the water edge, and floated off, and *vice versa*. This system, while probably the most suitable and economical for boats of relatively small displacement, experience has shown to be unsatisfactory when the displacement ranges from five to fifteen tons, and to be out of the question for greater displacements.

The increase in initial cost and maintenance of sheds is almost directly proportional to the number of boats in the service, and to the increase in their dimensions, and as boats of fairly large displacement will be used commercially, the cost will be prohibitive. Further, costly slipways and trolleys will form a necessary part of the equipment, and a relatively larger and more skilled handling party is required than found necessary for aeroplanes. Even with careful handling the hull sustains more damage, due to the "floating" off on the trolley, than would take place under average sea conditions, thus unnecessarily reducing its useful life.

In this system the embarking and disembarking of passengers, handling of cargo and refueling is easy and safe.

In the second system the boats may lie at moorings in the most sheltered waters available, and only come ashore for repairs which cannot be conveniently carried out there, and for periodical overhauls. Under these conditions lesser shed accommodation and fewer trolleys would be required than in the first system, and would depend only on the number of boats in use, as one spare would be required for every three in flight. Provided the boats are properly moored, and engines, propellers and cockpits covered over to afford protection from spray, rain, etc., the system works fairly well. Boats not specially designed to meet these conditions have been moored out at the Scillies for a period of fourteen weeks, and were still in good flying condition. The fabric covering on the planes and control surface least of all will withstand such severe weathering and becomes very soggy. Before this system can become commercially successful, therefore, a great deal must be done toward increasing the durability without a serious increase in weight.

The main drawback to this system is the difficulty likely to be experienced in the embarking and disembarking of passengers and cargo. A motor-boat tender has to come alongside, and the transference under moderately rough sea conditions would in consequence be a somewhat hazardous undertaking. Fueling would be carried out from either a fuel boat or the motor-boat tender equipped as such connecting up by means of a flexible rubber pipe, and presents no difficulties.

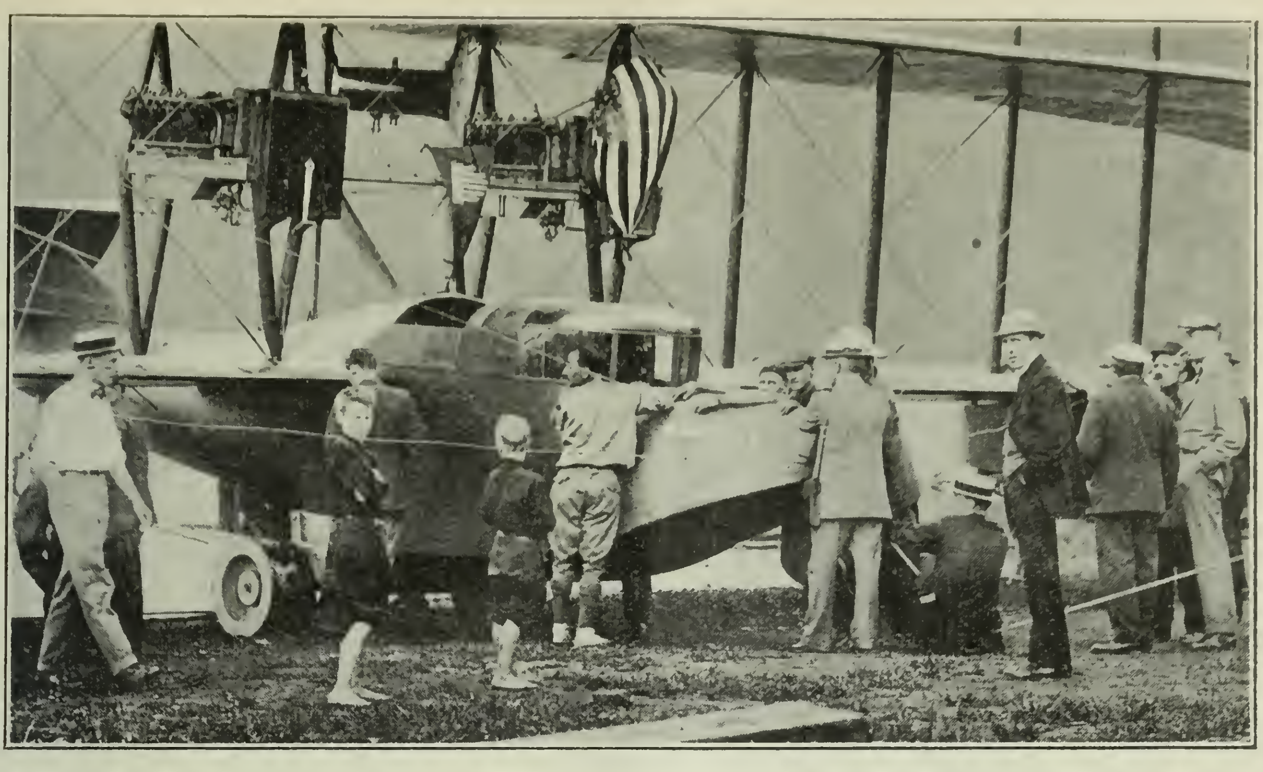
Before describing the third system it may be as well to indicate shortly the general lay-out of a large flying boat to meet the requirements. The experience gained from the "F" boats, and especially the "Fury," showed that so far as durability was concerned, the hull design was satisfactory, but the wing structure was much too fragile and complicated. As the result of the large area required with normal wing sections, further fabric covering must be dispensed with. The specification aimed at was as follows:—Three engines and propellers abreast mounted in the top plane, biplane wing structure, small gap-chord, deep section wing internally braced, span of upper plane considerably greater than that of lower plane, chord tapering from wing tip to centre section. External wing bracing from outboard engines to hull only. Owing to short span of lower plane large wing-tip floats to be fitted, wing tips not to extend beyond floats, wing covering either corrugated double skin wood or duralumin. In this way it is hoped to obtain a robust, compact and durable structure, with little sacrifice in performance.

Assuming boats as outlined above are available, and there are no insurmountable difficulties in the way, then probably the best arrangement would be to have a floating landing stage, with a gangway connecting to the shore; this in the first place would overcome all the tidal difficulties. The landing stage or floating dock, which as a mobile unit was submitted to the Air Ministry by the lecturer early in 1917, would be divided into at least two separate docks—one wet dock for outgoing or incoming boats, and the other, which by simple means would be used as a dry dock, for executing repairs, changing engines, etc. These docks would be probably at right angles to keep down the overall dimensions, and trim maintained by ballast tanks.

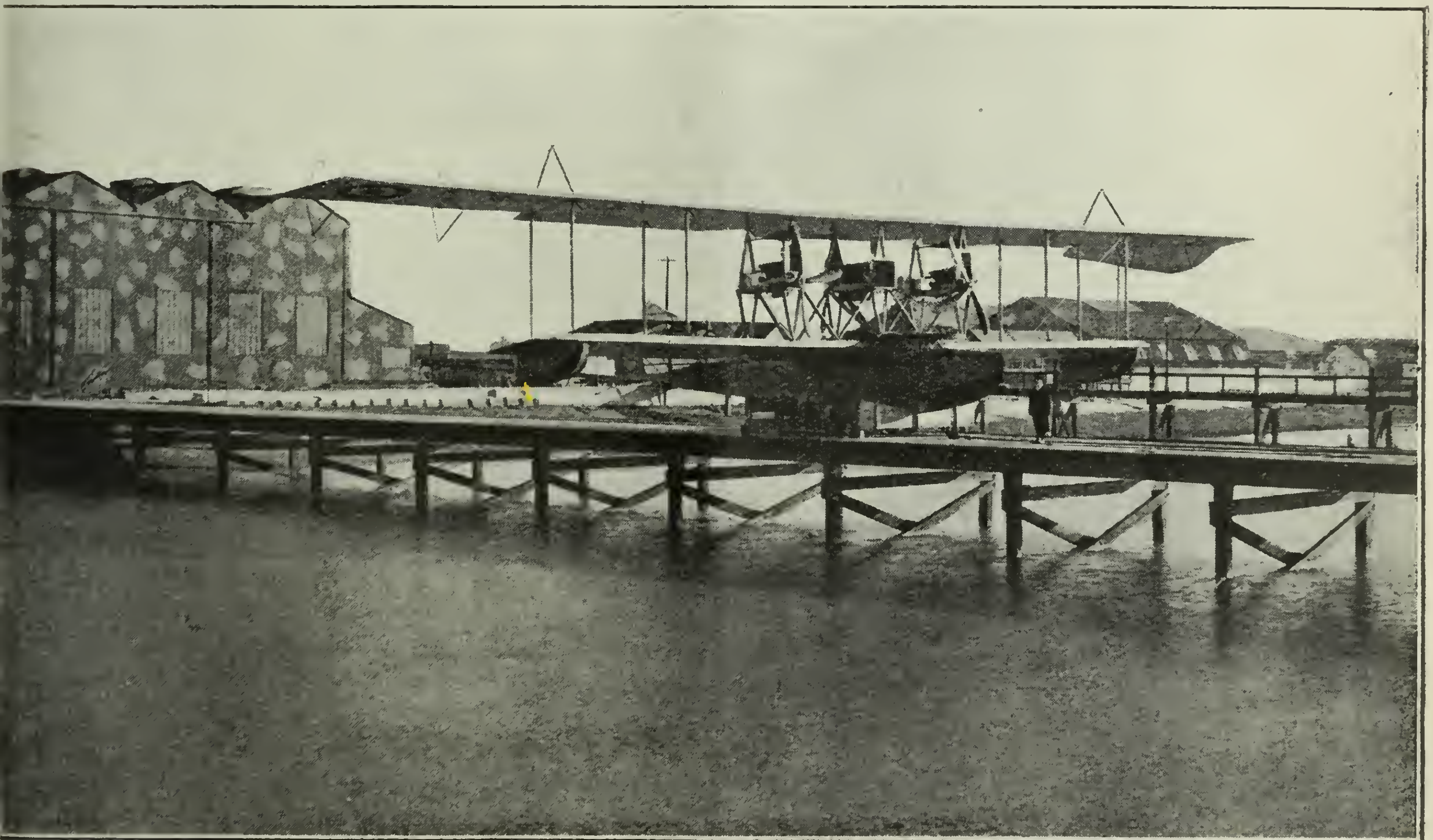
Normally, boats would lie at moorings, and only come into dock to take on board passenger cargo and fuel.

With this system shed accommodation, with the exception of shore workshops, would be unnecessary.

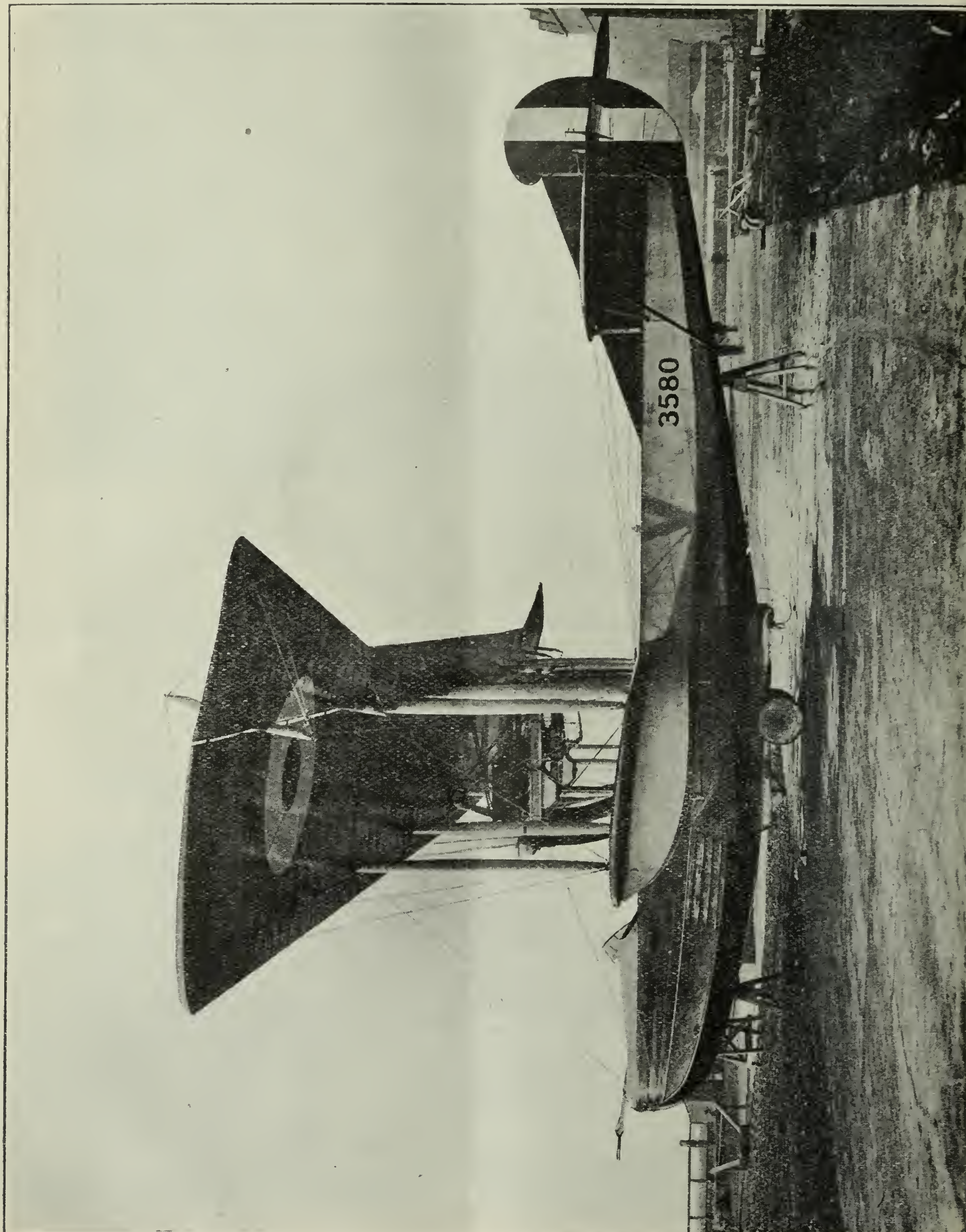
For those who have experience in the organisation and of running costs of aeroplane services the above brief discussion should suffice. The details are easily filled in. Bearing in mind that large flying boats will most likely be used in a commercial service, a rough estimate of the relative operating costs might be obtainable.



The famous "America" on which the late Col. J. C. Porte was going to attempt to fly the Atlantic in 1913. Colonel Porte facing the camera.



Porte "Baby" on Slipway.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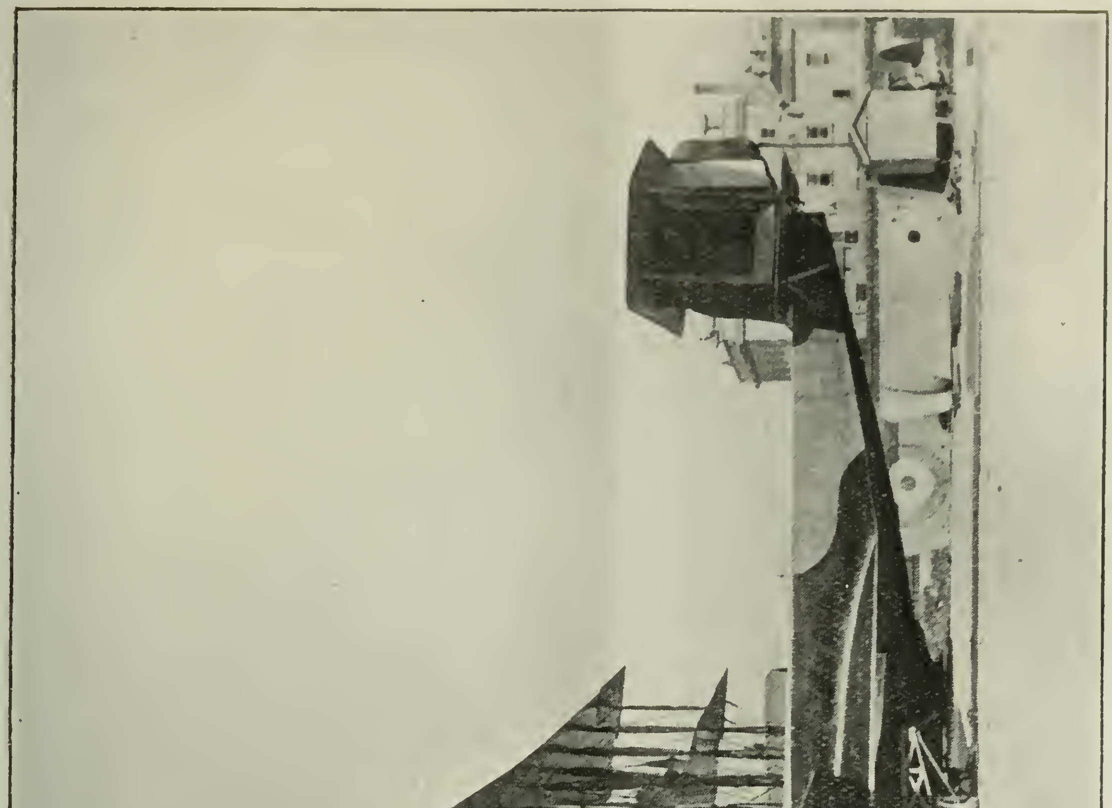


Felixstowe "Fury."

Discussion

Mr. G. S. Baker said he was very glad to see from the Paper that they could reckon Major Rennie as a disciple of tank methods. One came to that conclusion because Major Rennie had based a good many of his arguments on data which had been obtained from the tank. Dealing with the Author's introductory remarks as to the development of the flying boat being at a standstill, Mr. Baker said he did not think they would all agree with the reasons given in the Paper. He himself believed that the development of the flying boat would always be slow until there was a good technical man, a trained naval architect of some sort, holding a responsible position at the Air Ministry in charge of such work. Until that was done it was almost impossible for technical work to proceed properly; secondly, we must avoid concentrating practically all the experimental work on one type of boat, as was done in Porte's time. If we wanted to develop, we must give every type that showed a reasonable chance of success a full-scale experiment to demonstrate its feasibility. Since this had been started he believed it could be said that we had stopped building the " F " type boat, which had been replaced by a number of other types developed by individual builders. He believed he was right in saying that no " F " type boat had been built during the last 12 or 18 months, and possibly during the last two years. The Author, in dealing with hull lines and dimensions, had stated that tank tests were at variance with full-scale tests as regards the loading which it was possible to put upon the hulls. He believed that was the meaning of the sentence referred to, and he asked Major Rennie for the specific data on which he had based that statement. He himself was very keen to get as much comparison as possible between full-scale work and model work, and he knew of no definite data to support the Author's argument. Passing to the statement in the Paper with regard to the " Felixstowe Fury," in the same paragraph, it was desirable to put the facts on record so far as the tank was concerned in that matter. The Author had stated that " the modifications carried out on the new types of hulls evolved at Felixstowe were arrived at from full-scale experimenting, and with the exception of the ' Fury ' hull, tank tests on the corresponding models were not available at the time." The National Physical Laboratory, said Mr. Baker, had nothing whatever to do with the design of the " Fury " hull; it was designed by Colonel Porte, built by him, and sent to the tank in September, 1918, for tank tests after it had been built. The model was then tested at the tank, and it had been shown that its resistance could be lowered 25 per cent. over the whole of the speed range right up to 32 knots, and a letter had been received from Felixstowe stating that some of the modifications suggested by the N.P.L. were being embodied in the hull. The N.P.L. had reported, on the tests made, that the hull was liable to porpoise at 27 knots, and that no change which was feasible on the hull at that time could possibly eliminate that porpoising; the crash which occurred was due to the hull being rocked because of its facility for porpoising. The tests were made at a load which was not the proper load, but it was the load given to them, and the centre of gravity was not the centre of gravity. The impression received from that paragraph, that the N.P.L. were in any way responsible for the " Fury " lines, was not correct, in view of the facts he had given. Passing to the naval architecture side of the Paper, the hull weights and stresses, he had seen the " F " boats in the course of construction, and also what had come to be known as the " Linton-Hope " boats, and he was quite certain that any other naval architect who had seen them would say there was no comparison between them. The Linton-Hope work was really scientific compared with the work which was done on the other boats, and, further, there was no doubt at all about the superiority, from a weight point of view, of the Linton-Hope type of

construction. Also, he believed that Major Hope had had a great deal to do with the big improvement which took place in the construction of the later " F " types of boat. He himself knew that Major Hope had introduced very many changes, and had eliminated the cutting of many of the long strength members, and so on.

With regard to the elasticity referred to, he had noticed that the Author had referred to it as the " so-called " elasticity, and he was glad that he had, because he thought the term was applied a little wrongly to Linton-Hope's work. The real difference between the " F " type and the Linton-Hope type of construction was that in the " F " boat they built up stiff main transverse sections connected with a number of comparatively stiff fore and aft members. Between them were comparatively large areas of bottom more or less unsupported. In the Linton-Hope work practically all the bottom was supported by transverse members only two or three inches apart, which broke the bottom up into small partially-supported pieces all connected to elastic members. In the " F " boat hull, when running on the water, you could see the big patches of bottom panting in and out, and in one or two cases of damage he had seen the damage occurred because of that panting of the bottom between the stiff members. But the real difference between the two types, was not that one was elastic and the other not elastic, but in the manner in which these stresses were spread over and taken up by the main structure. There was another point which had not been touched upon by the lecturer, and it was a real defect on both the " P " and " F " type, namely, that one could not get at them inside. For the impact experiments on the " F " body he had been given a hull which was not quite three years old, and the inner skin was rotten in places, not because of bad work, but because it was almost impossible to get there to keep it clean. One could hardly get at some of the structure inside the boats, and if the hulls were to last more than two or three years they must be accessible inside. He knew of none which were really accessible.

Captain D. Nicolson thanked Major Rennie for having brought the Paper on the subject of flying boats before the Society, as up to the present very few members had tackled the subject. He also thanked Mr. Baker for his remarks, and agreed with him that the reason why flying boats had not advanced much since the war was because the Air Ministry did not employ a competent naval architect to supervise the designing and construction of these boats. He would go further; and stated that the hulls should be built by motor boat and yacht builders, and not by people who had not had previous experience in hull construction.

In answer to one of Mr. Baker's queries, as far as he was aware the Air Ministry had not ordered any of the " F " boats since 1918. Major Rennie had said that a big number of flying boats were a failure. He personally only happened to know of one, and he would like to know the others which turned out to be complete failures.

Major Rennie had also said that the laying off the lines was by no means easy. He entirely disagreed on that point, as the operation presented no difficulty to a naval architect. In fact, he stated that any boy completing his apprenticeship had to draw similar lines for boats and yachts; and as the process was so elementary, but proved difficult to the non-professional man, it was evident that only naval architects should be asked to lay out such lines.

Major Rennie had said that the extraordinary behaviour of the hull of the Porte Super-Baby in rough seas was due to the buoyancy and the lines adopted. Captain Nicolson said in his opinion the obvious increase in size would increase the seaworthiness. He believed that all that part of Major Rennie's description of the trimming moment came down to the fact that if the boat was properly designed it would not nose dive.

He also stated that the steps of the F.5 could have been altered to make this boat stable in the water; he admitted that this was advisable, and had stated that it could easily have been done, and he (Captain Nicolson) would like to ask why it was not done?

With regard to the question of the timbers in the Porte or fuselage type being continuous from chine to chine; this was not the case in the majority of the "F" boats, but was stopped at the keel, thus causing leaky boats, and continual trouble.

Captain Nicolson had to introduce intermediate timbers in the early "F" boats, to prevent the planking from coming away from the keel.

On the subject of bulkheads, Major Rennie had said that he thought these would give way with the inrush of water. He (Captain Nicolson) considered that bulkheads could easily be fitted that would withstand the normal inrush of water. This subject he dealt with fully before the Society, and made many suggestions, among which he proposed a bulkhead committee as set up by the shipbuilders in this and other countries.

Major Rennie had also said that double bottoms should only be fitted in way of the passenger accommodation. He (Captain Nicolson) did not agree, because in ordinary naval architecture they always ran double bottoms the whole way of the water line; and he thought the same should apply to flying boats.

The reason the lecturer had given for the supposed superiority of the Porte type over the Linton-Hope type, was because the boats rested on the keel. He had never yet seen one of these boats resting on the keel when in a trolley; thus causing the planking to spring, with the result that the boats leaked when put in the water. He had been to Felixstowe, and had found that each boat had a particular trolley to fit that particular boat, and even with this precaution the boats did not rest on the keel; this could easily be seen by examining the plans. After his visit to Felixstowe he designed a new false keel to keep the planking up to the original keel; this was bedded in red lead to make a water-tight job.

Another point which the speaker considered showed very bad design in the original F.2A. and F.3, was that all the sides aft were made of fabric; to a naval architect that was not a good job, and all the latest F.5's had diagonal planking, which was of his own design.

With regard to the number of metal fittings used in the construction of the Porte type, in his opinion they were far too numerous, which was from the production point of view very bad; and this might be one of the reasons why the "F" boats are not in the list of boats being built at the present moment.

It was said that the fittings did not rust away in a short time, but he had his doubts about that. If they did rust, it would probably mean pulling down half the boat to get some of the fittings out.

With reference to the streamline form, he had no doubt that the Linton-Hope type was much the superior form.

Regarding the boat which Major Rennie had pointed out was built on the yacht or motor boat form of construction, which he did not like, he (Captain Nicolson) would like to inform the Author that that boat was not built on the yacht or motor boat principle; showing that Major Rennie was not acquainted with motor boat construction.

As to the Author's reference to the use of three-ply for small bulkheads, the speaker did not agree; he did not think that three-ply was suitable for building bulkheads.

In conclusion, he said that although his remarks had not perhaps been such as Major Rennie would have liked to have heard, he hoped they would not be considered to be directed to Major Rennie personally; he was speaking from the point of view of the naval architect.

Mr. Manning joined with Captain Nicolson in his request for particulars of the boats which were said to be unsuccessful. He believed the Author would find that several quite successful machines had been produced in the last two years by private firms. With regard to the table of percentage weights published in the Paper, he would like it to be realised that the figures attributed to the F.5 were not representative of the standard F.5 as produced in large quantities towards the end of the war. The figures referred to a special F.5 built at Felixstowe, and, so far as he knew, only one of this type was ever built. This special F.5 necessarily had to be considerably strengthened and otherwise altered to make it fit for general service, and it was the modified type which was turned out in large quantities. Comparing this machine with one of the "Linton-Hope" type of approximately similar size, P.5, the structure weight of the standard F.5 was 47 per cent., and of the P.5 35.8 per cent.; the useful load percentage of the standard F.5 was 30 per cent., and of the P.5 39.3 per cent.; the weight, empty, of the standard F.5 was 9,100lbs., and of the P.5 7,439lbs., and the useful loads were 3,900lbs. and 4,750 respectively. The speed of the "Linton-Hope" type of machine was about 13 or 14 knots higher than that of the F.5; the speed of the standard F.5 with full load at sea level being 76.5 knots, and that of the P.5 under the same conditions 90 knots, in both cases with Rolls-Royce Eagle engines. The standard F.5 was a very useful flying boat, and did very good work towards the end of the war, but it was clear that its performance as an aeroplane was inferior. He drew special attention to the figures, as the great superiority of the P.5 was largely due to its having a "Linton-Hope" hull. Also the tail area of the P.5 was 196 sq. ft., and not 143 sq. ft., as had been stated. The tail volume coefficient was, therefore, .42 and not .31. The lecturer had given a description of the complicated elevator movements required to get an "F" boat off the water. It was one of the advantages of the "Linton-Hope" type, as developed by Mr. Baker, in the tank at the N.P.L., that none of these movements were necessary. Such a machine could be started from rest on the water, and could be flown off without the elevator being touched. Major Rennie, in his description of the "Linton-Hope" type of construction, had stated that these hulls had three-ply formers in the bottom. He (Mr. Manning) pointed out that the use of three-ply in such positions had been obsolete for several years, and that recent hulls had no three-ply at all in their construction. The lecturer had made a great point of the wing root structure on the "F" boats. It might be necessary in this type, but all the "Linton-Hope" type boats got on perfectly well without it, and this type of boat could be put on a trolley in just the same manner as the "F" type; in fact, they were always handled in this way. The risk of damage was about the same in either type. On the other hand, the wing root structure was very heavy, and obstructed the interior of the hull. The lecturer had also suggested that in hulls of the "Linton-Hope" type the tail setting did not remain constant, owing to variation of temperature, etc. He (Mr. Manning) could assure the lecturer that he was mistaken, and trouble of this type did not occur in the "Linton-Hope" hull. Again, in his description of the "Linton-Hope" type hull, the lecturer had referred to three-ply formers, and to the pocket between the inner and outer skins. As a matter of fact, both these defects had not been "more or less" eliminated, but "completely" eliminated in more recently designed hulls. As to hull weights, the Author's F.5 weight referred to the special F.5 built at Felixstowe, and not to the standard F.5 as used. He did not know the exact weight of the latter hull, but it was approximately 2,500lbs., or 1,000lbs. more than the other. The lecturer had also stated that

the somewhat smaller size of the "Linton-Hope" type would make it less seaworthy than the "F" type. There was no doubt that the "Linton-Hope" type of hull as fitted to the P.5 was considerably more seaworthy than the "F" type hull of similar displacement; this had been proved. It was clear that Major Linton-Hope, when he designed the hull, knew quite well what he was doing, and did not put the load water line too high, as suggested. The speaker had noticed that Major Rennie had recommended three-ply for use on bulkheads of wing-tip floats. This material was quite unsatisfactory for this purpose, and was not used on any modern design. He agreed with Mr. Baker when he had said that the accident to the "Fury" would not have occurred if that machine had been fitted with a hull of the "Linton-Hope" type. The lecturer's curve showing hull weights was interesting, but it should be remembered that, however suitable these hulls were for the special conditions at Felixstowe, they required considerable strengthening if used for general service, which would increase the weight considerably. The fact that the "F" type of hull was unsuitable for the fitting of a double bottom was an exceedingly serious defect. The lecturer had attempted to suggest that a double bottom was not necessary, but he (Mr. Manning) did not think that anybody who had had experience with flying boats would agree with this. A boat which, fully equipped, might be worth, say, £50,000, should not be sent to sea in a condition which would lead to total loss if it landed on a piece of sunken wreck. He thanked the Author for his Paper. The description of the early experiences at Felixstowe was exceedingly interesting, and the collection of experiences of this sort would be very valuable in connection with future design.

Captain G. T. R. Hill, referring to the point raised as to the length of the bodies of seaplanes as compared with those of land-going machines, said it would be noticed that, on the whole, the length of the bodies of the seaplanes was shorter than that of the land machines. The tail volumes were, on the whole, about the same, so it must mean that the seaplanes had larger tails, and thus heavier elevator control. The fact that they had shorter bodies seemed to lead to the serious disadvantage of greater wing span; he could see from the tables that this was so, and that the aspect ratios of the seaplanes were in excess of those of the land machines. The bodies were apparently built short in order to keep down the weight, so that the whole design was inferior to that of the land machines in that the serious disadvantages of the extra span had to be put up with in order to keep the weights down to the same sort of figure as on the land machines.

Again, the wing weights apparently were slightly less on the seaplanes than on the land machines. From the higher aspect ratio it might be inferred that lower factors of safety were worked to on seaplanes, and he would like the Author's opinion as to whether that was so or not.

With regard to the trouble of getting the machine trimmed fore and aft, he had often wondered why, in these experimental machines, the designers did not allow for a small sweep backwards or forwards of the wings, to be obtained by packing out the front or rear spars at the roots, thereby altering the centre of gravity position so that the required amount of stability fore and aft might be obtained on trial. The same applied to land machines.

He would also like to know whether there was any possibility of designing seaplanes which could enter the water at a comparatively steep angle; *i.e.*, whether a hull could be designed which, while having the necessary qualities of seaworthiness, would allow the water to absorb the energy of the machine falling with a speed whose vertical component was 10, 15 or even 20ft. per second. It seemed that seaplanes would be much safer if they could come down at a steep angle when alighting on a small area of water in emergency. Did the Author

think it possible to allow a machine to sink, say, 4 or 5ft. after first touching the water, in order that the vertical velocity might be destroyed by the pressure of water on the bottom.

With regard to controls, there seemed to be still a certain amount of discussion as to the exact cause of the accident to the "Felixstowe Fury." He believed that accident, and accidents to other large machines, had been due really to the lack of proper control. On large machines nowadays the control surfaces were first designed of such size that the machine would have reasonable control. Then it was found that the pilot was not strong enough to operate the surfaces of that size, and they were cut down until he could do so, which necessarily resulted in machines having inefficient control. The result of this lack of knowledge as to how to provide adequate control on large aeroplanes might be likened to feeding a man on raw potatoes because one did not know how to make the kitchen range work; it was not only very unpleasant, but also highly dangerous.

Major Buchanan considered it only fair to say that the "F" type boats were used by the Admiralty throughout the war, and had performed excellent service for the Admiralty and the R.A.F. He did not wish to diminish what had been done by the late Major Linton-Hope, with whom he had been in close contact at the Air Ministry and at the Admiralty. While he agreed generally with the naval architects on the question of the design of the "F" type boats, at the same time, he thought it was going rather a long way to claim that the present type of so-called flexible hull was a complete solution. His reason for saying that was that, whilst we had a large experience of the "F" type boat, our experience of the "Linton-Hope" type of hull was very limited indeed, and therefore he felt it would be rash to say that the latter constituted a complete replacement of the "F" type of machine. With regard to useful loads, the Author had given the average useful loads taken by the aeroplane and the seaplane. He believed the Author had taken the average of certain things which were not comparable, and in conclusion he had said that the average useful load of the flying boat was 40.5 per cent., against only 38.5 per cent. for the aeroplane. In the first place, the figures were not quite correct, and he felt that the conclusion that the flying boat carried the same useful load as the aeroplane, was scarcely justified. In fact, he considered that a very strong case could be made out the other way. He could not agree with the Author that it appeared logical that the most suitable type of aircraft to develop in large sizes was the flying boat. One of the reasons was that already stated, namely, the useful load carried. He pointed out that for the flying boat greater engine weight had to be provided. He was also not sure that he agreed with the lecturer in his condemnation of the single-engine flying boat. It had certain definite and useful functions, and he did not think it was fair to say that the single-engine flying boat was not satisfactory. For the purposes for which it was used, and could be used, he believed that on the whole it was doing reasonably good work. In conclusion, he thanked Major Rennie for his very interesting paper.

Mr. W. A. Wright (British Electric Co.) raised the problem of the cost of construction of these boats. Major Rennie had shown that in the latest type of boat developed by Colonel Porte the planking and timbers were carried round in a rather complicated form. He was inclined to think that the cost of construction in that case must have been very high, and that was a point which would seriously affect the development of flying boats in the future. In the old "F" type of boats there was a large number of fittings, which would increase cost unless they could be produced in very large quantities, but even if they could be produced in large quantities at a reduced price he was inclined to think that the "Linton-Hope" type of hull could be produced, size for size, cheaper than the "F" type. Major Buchanan had also mentioned that the principal duty of a

flying boat was to fly. He believed they had always agreed with that. He himself was always trying to reduce the weight of the hulls, but one could not overlook the fact that large flying boats had to be efficient boats. The difficulty of housing machines when not in use was very great, and the type of machine that had been considered in the Paper was not of much use unless it could remain afloat for weeks on end when not required for flying. That led him to say that, on the question of whether these boats should be designed by aircraft designers or naval architects, after all said and done, the most useful thing was to have co-operation between the two. So far as he could see, that had resulted in by far the best results obtained up to the present. He thanked Major Rennie for his Paper.

Mr. E. Gibson Knight (*communicated*): From the manufacturing point of view, one of the most essential differences between the Felixstowe type of construction and the Linton-Hope construction is the fact that while the former type employs a large number of metal fittings, the Linton-Hope type, with the exception of towing plates and strut attachments, requires no metal fittings in its construction. The metal fittings in the F.5 flying boats cost about £200 per machine to produce when manufactured in quantities of some hundred sets, which enabled almost all parts to be stamped out by means of dies, and I think it is no exaggeration to say that to produce a similar set of fittings to-day, in the quantities of a few sets at a time, would cost not less than £400 per set. The cost of the boat-building work on the two types is about equal and it therefore appears that the Felixstowe type will—size for size—always cost more than the Linton-Hope type by the cost of the metal fittings and, judging from experience on the F.5, this would mean an addition to the cost of about 25 per cent. unless they can be built in very large quantities.

The later type of Felixstowe construction in which the planking joints at the chines and fin tops were eliminated by bending the timbers and planking round these points would be even more expensive, as a very elaborate bending jig would be necessary for the bending of all the timbers, every one of which requires to be slightly different, and every timber must be steamed. In addition, while it is understood that the outer planking was put on diagonally, thus enabling it to be bent on the actual hull itself, this necessitates the steaming of every plank as it is fitted into place. One hesitates to give any idea as to the extra cost of this method of construction, but it must be absolutely prohibitive when compared with the Linton-Hope construction, in which no steaming is required on either the timbers or the planking, with the possible exception of a few timbers right at the tail end of the boat. Further, the sole object of this later type of Felixstowe construction was to eliminate the planking joints at the fin chines and fin tops owing to the large amount of trouble experienced with the Felixstowe types through unsatisfactory joints at these points. In the Linton-Hope type of construction this trouble is not experienced and there is, therefore, no object in adopting a costly and elaborate system of obviating these joints.

Major Buchanan mentions that the principal duty of a flying boat is to fly, and in this I absolutely agree with him, and think there is some tendency on the part of the Air Ministry to insist on too great a perfection in the hull construction at the expense of weight and consequent deterioration of air performance. On the other hand, one has to realise that the difficulty of housing large flying boats is very great and they should undoubtedly be capable of being moored out for some weeks when not required for flying.

While one cannot agree with a very large number of the Lecturer's opinions, one certainly agrees that it would be unwise for the Air Ministry to devote themselves entirely to one type of construction to the exclusion of the others. It must be pointed out, however, that while the "F" boats have already been produced in enormous quantities and have had several hundreds of modifications

and improvements effected in them, the resultant machine was even then inferior to the first machine of this size fitted with a Linton-Hope hull, and on which practically no modifications were carried out whatsoever, and there is little doubt that when the improved type of machines now under construction are tested, the superiority of the Linton-Hope over the Felixstowe construction will be even more marked. In conclusion, one would thank Major Rennie for a most interesting, if somewhat controversial, paper.

The Chairman, dealing with aerodynamic structure, quoted the Author's remark that "It has been said frequently of the 'F' boats that the use of stabilisers on the top plane is a very inefficient aid to lateral stability. They were never really intended as such, as it was well-known an increase of dihedral angle would be much more effective." He had never understood what stabilisers were for, and if Major Rennie could give him some idea as to why they were put on it might help in the future.

Major Rennie intimated that he would prefer to reply to the criticisms raised in writing.

A hearty vote of thanks was accorded the Author for his "very interesting and apparently very controversial Paper," and the proceedings terminated.

Major Rennie's Reply

In reply to Professor Bairstow, with regard to the effect of vertical fins on the top plane of flying boats, I am afraid I haven't the courage to explain the aerodynamic effect of a vertical fin above the C.G. in the case of, say, sideslipping to such a well-known authority on aerodynamics. These fins originated with the early Curtis boats, and have been retained for reasons given in my paper.

In reply to Mr. Baker, he says they could reckon me as a disciple of tank methods, because a good deal of my arguments were based on data obtained from the tank. With the exception of the curves showing the water performance of a hull, my arguments are based entirely on the results of full-scale experimenting. I think I have made my position in this respect quite clear in the opening paragraph under "Hull Lines and Dimensions."

He says that in Porte's time experimental work was confined entirely to his type of boat. This is not the case, as one firm at least, The Norman Thompson Flight Co., were concentrating on flying boats at the same time. They had little success, as their hulls were of the old Curtis type, which had many serious defects.

Again, he says that for the last 12 or 18 months, no "F" type boats had been built, as they had been replaced by other types developed by individual builders. I am afraid Mr. Baker is unacquainted with the true position. During the war about 500 "F" boats were ordered from various firms. Of these, say, 200 were delivered and used up to the Armistice, after which a certain number were cancelled and the remainder delivered to stores. Some of the latter are still being reconditioned and delivered to the Service. There is no R.A.F. squadron equipped with any other type of flying boat, and no other type has, so far, got further than the experimental stage.

It may be of interest to state that the U.S. Naval Air Service are using F.5's exclusively, and have found them to be most satisfactory.

With regard to hull loading, I think it is quite clear what I mean. I have no specific data. The conclusions are based on the experience gained with a large number of boats, under all sorts of conditions. All data cannot be expressed in mathematical symbols, as Mr. Baker may know. If Mr. Baker will read more carefully the part dealing with the Felixstowe "Fury" I think he will realise there was no intention of giving the tank credit for the hull lines. While quite realising the position of the steps on the "Fury" could have been altered to help towards the elimination of porpoising, the lines generally were found to be most satisfactory, but could be further improved as explained in my paper.

I think I have acknowledged the assistance obtained from the tank in suggesting modifications to reduce the water resistance.

With regard to the crash of the "Fury," it is quite obvious many important facts are not generally known. Prior to the accident she had done about 30 hours flying, and in every way was found most satisfactory. Under the control of expert boat pilots, such as Colonel Porte, Majors Hallam, Hobbs, Wright, and Cooper, porpoising to any serious extent was absent when taking off. The tendency was there, but sufficient elevator control was available to keep it down. Owing to the tail plane being well covered by the propeller slipstream, there is more elevator control, over the range of speed between the hump and taking off, than probably is generally realised.

The C.G. position had been determined by weighing in the usual way, contrary to what is stated in the report on the accident.

Several months before the accident Colonel Porte and myself were demobilised, there was therefore no technical officer in charge. No one knew where the C.G. ought to be or took the trouble to find out. The result was that the boat was loaded up with spares, etc., and the fuel, of which there was tankage for 1,500 gallons, most likely distributed in the tanks such that the final C.G. position was at least at .5 of the chord. Further, from a reliable and intelligent member of the crew, the late Major Moon attempted to take her off, as he had done on a previous occasion, before the minimum safe flying speed had been reached. As loaded, she was underpowered, there was therefore little available h.p. for acceleration, once clear of the water, with the inevitable result. While possibly this might not have occurred had the arrangement of steps given stability on the water and the water moment such that the air control was insufficient to allow of the boat becoming air borne in a stalled condition, it may be mentioned that a stable hull is only such under the conditions for which it has been designed. While small variations in loading and C.G. position are of little importance, sufficient deviation from those conditions may easily arise in actual practice, such that an otherwise stable hull may tend to porpoise. Also, for example, in a long heavy swell it is quite possible for any boat, no matter what step arrangement is adopted, to attain an angle of incidence corresponding to the stalled condition.

While Mr. Baker has done a great deal towards the elimination of porpoising, the problem is by no means completely solved.

Before replying further to Mr. Baker, it may be as well to say a few words with regard to the part in the design and construction of a flying boat which could come within the province of the naval architect. From Mr. Baker's and Captain Nicolson's remarks one may rightly infer that unless one is a naval architect one can never hope to design a successful flying boat hull. With this I am in disagreement, as is also Major Buchanan. In the first place, it should be understood clearly that a naval architect in the true sense does not include a ship's draughtsman or small boat builder. I take it Mr. Baker is hinting at the latter when he says the Air Ministry should employ a competent naval architect.

The knowledge of naval architecture required in the design of a flying boat hull is of a quite elementary nature, and from the purely theoretical side is well within the capabilities of the average aircraft designer. Apart from a certain amount of the detail design, the hull design cannot be separated from the aerodynamic or structural design of the complete boat, and up to the present this knowledge has not been possessed by those dealing with the hull design alone.

Mr. Baker says, "There was no doubt at all about the superiority of the Linton-Hope type of hull from the weight point of view." I am afraid a mere statement like this is not very convincing. The weights given of the F.5 were actual and not estimated, and the hull of this particular F.5 was, with the exception of that of the "Fury," the best hull designed and built at Felixstowe. This is not just

my own opinion. Experience showed it to be such. Major Linton-Hope had nothing whatever to do with the improvements incorporated in the design of this hull. The Porte I., the prototype of all the "F" boats, was under way before he joined the Air Dept., Admiralty.

With regard to Mr. Baker's contribution to my paper, setting out more fully the differences in the construction of the "F" and "P" type boats. The fact remains that the Porte hulls stood up and are still standing up to the work for which they were designed, in spite of all the adverse criticism and condemnation which they have been and are subjected to. Admittedly there are many faults and weaknesses in the Porte hulls, but there are just about as many in the Linton-Hope hulls. The naval architect is no more infallible than the engineer, as, for example, witness the accident to the rigid airship R.38. Again, the Porte hulls were built on a production basis, and given out to firms, in many cases, with no experience of boat building. Also a lot of unskilled labour was employed. Whereas the P.5 hulls were built by a "pukka" firm in this class of work.

The difficulty of access to the hull bottom inside the hull has always been realised, and when a large quantity of fuel is carried, seems inevitable. As mentioned in the paper, the fuel may be carried in tanks on the top plane, thus clearing the hull. The same trouble is likely to be experienced when hulls of moderate displacement are fitted with double bottoms.

In reply to Captain Nicolson, the chief reason for the stoppage of "F" boat construction is given in my reply to Mr. Baker. Further to which I might add that few, if any, Air Ministry officials were aware of the improvements incorporated in the design of the "Fury" hull. As these improvements led to a hull greatly superior to that of the "F" boat hulls, it is most unfortunate that the development of this type of hull has not been proceeded with, more especially when one realises the achievements and performance of these boats.

With regard to the failures of other hulls, Captain Nicolson will realise I am unable to make public their names, but I will be pleased to give him privately the names of at least six.

I disagree most emphatically with Captain Nicolson when he says, as he did at the discussion, that the setting out of the lines of a hull could be done by the office boy. I should have thought that a man who has been more or less connected with ship work would have realised that the setting out of the lines is divided into two parts. Firstly, knowing what to set down, and secondly the mechanical process of drawing them on paper.

If the former can be done by the office boy, then evidently the tank is unnecessary and experience of no account. In other words, we are all wasting time and money chasing the obvious. The latter is comparatively simple, requiring only an elementary knowledge of geometry and accurate draughtsmanship. On the other hand, such work, in my experience, is given to an experienced draughtsman, as it will be realised that a fair amount of work is detailed in the D.O. before the lines are laid down, faired and checked in the mould loft.

Again, Captain Nicolson said he didn't know of any ship in which the centre of buoyancy wasn't vertically under the C.G. With this I agree, but the trim in still water may be very different to that required. The desired L.W.L. is then obtained by, usually, a combination of a small shift of the C.G. and a slight alteration to the under-water lines.

The increased seaworthiness of the "Fury" hull over the smaller "F" boats was obviously partly due to the increase in dimensions. However, the marked improvement in this respect was due to the reasons given, which are based on experience and not on opinions.

I regret I am unable to reply to Captain Nicolson's comment on what I have

said about trimming moments, as I do not know to what he refers. I am afraid there is more in this world than meets his philosophy. The position of the steps on the "F" boats was obtained by Colonel Porte as the result of a great deal of experience taking off and landing flying boats under very varied weather and sea conditions. He probably did not realise that a more stable arrangement was possible. Just before the Armistice work was put in hand to fit steps on the F.5 in accordance with the arrangement developed at the tank, but the work had to be stopped owing to demobilisation troubles. It must also be remembered that the experimental F.5 had been flown before the design of the P.5 had begun, and before there was any full-scale test of the tank arrangement of steps.

It is obvious bulkheads could be fitted which would withstand a 12in. shell. It is entirely a question of weight. For an acceptable weight I very much doubt if bulkheads can be made to stand up to a serious crash in boats of the displacements in present use.

As flying boats become of relatively greater displacements, the fitting of a double bottom will be simpler, as the design will lend itself more readily to its adoption without a serious increase in weight. In any case, we are dealing with flying boats and not ships, the latter are not called upon to fly; the conditions are therefore not similar. The "F" boats were so designed that the hull, when on the trolley, rested on the keel, and was steadied at the bottom longerons. At the experimental station this always was the case, as the trolleys were fitted to the hull to ensure this. If such was not the case in the war flights at Felixstowe and elsewhere, then the fault lay with the trolley and not with the boat.

Captain Nicolson complains of the bad design in having the sides aft covered with fabric. Does he seriously believe Colonel Porte and his staff were unaware of this? As I have said, these boats were designed originally to operate from Harwich Harbour. They were used for submarine patrol and ship escorting. The maximum possible military load had to be carried, and the air endurance was continually being increased. Many sacrifices and risks had to be made to help in this direction, as the one and only object in view was to beat the Hun. This particular compromise was justified, as to give one example, the saving in weight over planked sides would add about half an hour to the air endurance at cruising speed. As may be seen, it wasn't a question of naval architecture.

Mr. Manning challenges the percentage weights of the F.5, and says the hull had to be considerably strengthened to make it fit for general service. As I was chief technical officer at Felixstowe, and therefore in a position to know accurately the history of this boat, I must contradict him. As I have already pointed out, this boat had been used extensively, and was well-known to be the best hull turned out at Felixstowe.

The F.5 was never put into production, which was a great blunder on the part of the Production Dept., Ministry of Munitions. Instead, the F.3 wing structure, the weight considerably increased to facilitate production, and adapters fitted to take either streamline wires or stranded cables, also permanent slinging gear incorporated, was fitted to a mongrel hull, a cross between the F.5 and F.3, and the resulting boat called the F.5. This was done solely because the F.3 was already in production (it never should have been), and the Ministry of Munitions were against a further change as jigs, templets, etc., were already made for the F.3. Therefore Mr. Manning's figures for weights of the F.5 are not accepted for two reasons; firstly, they do not represent the F.5, and secondly, the hull weight includes bulkheads, seats, etc., and consequently is not comparable with the figures given in my paper.

With regard to air performance, the following figures extracted from test station reports are of some interest. As the only complete tests of the P.5 available are those light and overload, these will be compared with the F.5 in the same condition.

LIGHT TRIALS.

	P.5. Two Rolls Royce Eagle VIII. Engines, 352 H.P. each.	F.5 (Felixstowe). Two Rolls Royce Eagle VIII. Engines, 345 H.P. each.
Weight, empty	7,437lbs.	8,023lbs.
Total weight	9,210lbs.	9,630lbs.
Weight/sq. ft.	7.12	6.83
Speed at 2,000ft.	91 knots	88 knots
Rate of climb at 2,000ft. ...	672ft./min.	701ft./min.
Service ceiling	15,100ft.	17,400ft.

OVERLOAD TRIALS.

Total weight	12,511lbs.	13,306lbs.
Speed at 1,000ft.	80 knots	87½ knots
Rate of climb at 1,000ft. ...	353ft./min.	352ft./min.
Service ceiling	6,400ft.	9,200ft.

Without analysing these trials to obtain a better aerodynamic comparison, I think it is fairly obvious that the P.5 performance is only very slightly superior to that of the F.5. Further, the P.5 failed to take off with a load of 12,800lbs., showing that the F.5 may be overloaded to a much greater extent, which is not without its advantages. It should be mentioned that the wing section on the Felixstowe F.5 was different to that of the production F.5, which was R.A.E.14.

I have to thank Mr. Manning for correcting my figures for the P.5 tailplane. With regard to "taking off," while I am quite well aware of the fact that the P.5 has been taken off "hands off," this is not the usual practice, for reasons I have outlined elsewhere.

As the object of my paper was to set out some of the work carried out at Felixstowe, which came to an end early in 1919, when discussing hull construction, it would seem fair to compare the Porte type of hull with other types actually built prior to that date. At the same time, I took care to indicate improvements which subsequently have been found necessary on these types.

Three-ply formers between the planing bottom and shell were used on the N.4, which boat so far has not yet been tested. With regard to the pocket between the inner and outer skins on the P.5, I said this defect had been more or less eliminated. This is quite true, the many improvements carried out on these hulls have not yet been tested, and are either in an incomplete state in the shops or still on the drawing board. It is unwise to argue on hopes, no matter how promising they may be. On the question of tail sagging on boat-built hulls, I think if Mr. Manning will read carefully my paper, he will find that I never mentioned the Linton-Hope type of hulls, but referred to the earlier boat-built hulls, namely those of Curtis. Quite possibly the L.-H. hulls may exhibit this weakness when subjected to Service use.

Taking now the wing root structure, sufficient experience has not yet been obtained to say definitely that this structure is unnecessary in the L.-H. hulls. Experience has been gained with over 200 "F" boats, operating under Service conditions varying from stations in the North of Scotland to the Mediterranean. In all two P.5 boats have been built and used and have not been out of the hands of either the test station or Development Flight.

The curve of hull weights as shown provides hulls of ample strength to meet all reasonable requirements. This is not a question of my own opinion, but it is supported by the results of experience with a large number of hulls, extending over a period of several years.

I agree that double bottoms extending the whole length of the L.W.L. are desirable, but it has not been proved that with boats of the present displacements

they are essential and the increase of weights warranted. With boats much larger than so far built, as Mr. Manning suggests, at a cost of £50,000, I agree double bottoms should be incorporated, for reasons already given.

To say that "the fact that the 'F' type of hull was unsuitable for the fitting of a double bottom was an exceedingly serious defect" is a statement without the slightest justification. As I said in my paper, the "Porte" type of hull did not lend itself readily to the fitting of a double bottom. On the other hand, experience with these boats has shown that damage due to flotsam when taking off or landing is a most infrequent occurrence. I do not know of any flying boat yet flown which has a double bottom along the L.W.L. The Linton-Hope hulls certainly have not. In these hulls, the planing surface from the stem to the main step is additional to the hull proper, the skins of which wash into the hull about half way across the form; thus there is less than 50 per cent. of the area of planing bottom when there is a double bottom in which the two skins are separated to any extent, the remainder having only more skin thickness. At the step the two skins are separated from chine to chine as in the Porte hulls. In any case, blocks of timber are the most dangerous from this point of view. Empty beer bottles do not float, as Mr. Manning may easily verify, and I doubt very much if passengers on excursion steamers are allowed to take empty beer bottles on deck to throw overboard for amusement. Further, on this same point, Mr. Manning says "that anybody who had had experience with flying boats wouldn't agree that double bottoms are unnecessary." I think I am right in claiming such experience, as with the exception of about $1\frac{1}{4}$ years on full-scale research, I have been employed entirely on flying boat work since early in 1915. As a matter of interest I shall be very glad to compare my flying hours, time stationed on air stations, and sea experience generally with Mr. Manning and Captain Nicolson.

I think I am right in stating that Mr. Manning's flying boat experience has been confined to the P.5, of which so far two have been built. It is therefore of some interest to trace briefly the history of the P.5.

Colonel Porte had shown, by the end of 1916, that it was possible to design, build and successfully fly a twin-engined flying boat of at least 12,000lbs. displacement. In other words, the pioneer work up to this size was done. Porte led the way, others followed. Having the advantage of this knowledge, and the experience gained with the Porte boats, the Air Ministry in 1917 decided to build a boat of about the same displacement—leading to the P.5. The hull lines were set out by Major Linton-Hope, and a model tested at the tank, where certain modifications were suggested as the result of experience with previous models tested. The hull construction was to Linton-Hope's design and was built by Messrs. May, Harden and May. The hull was then handed over to the Phœnix Dynamo Co. to fit the superstructure, etc., which firm were then building F.3's and F.5's.

Owing to the high L.W.L. and lack of experience, the planes were set too low on the hull, and had to be considerably raised after preliminary trials had been carried out. The various hull defects have already been indicated in my paper.

In reply to Captain Hill, I regret I am unable to follow his argument on the disadvantages to be expected from a high aspect ratio, unless he is referring to manœuvrability. From the aerodynamic point of view I am inclined to think that the aspect ratio commonly used on flying boats leads to greater efficiency than that used on the corresponding aeroplanes, thus partly accounting for the better performance of the former.

The short tailplane arm makes for less damping of the longitudinal oscillations, giving a more sensitive machine fore and aft. The control appears to be quite satisfactory. I agree that allowance for sweep back is most useful, but owing to

the wing tip floats and covered-in pylons, would lead to some complication in the case of the boats under discussion.

I am not at all clear on what he means with regard to the connection between aspect ratio and factors of safety. Maybe there is an error in the reporting of his remarks.

While I have no definite data to go on, I think a vertical velocity of 15ft. per when landing is quite reasonable, and could be obtained without much sacrifice in other directions.

From the time the keel first touches the water when landing, the distance the boat sinks into the water will depend upon the displacement of the boat, the under-water lines and the vertical velocity.

The accident to the " Fury " was certainly not the result of having insufficient or inefficient control. The control proved to be ample, and was well within the power of the pilot.

In reply to Major Buchanan, I would like it to be known that his remarks on the " F " boats carry considerable weight, because as head of the Aircraft Design Dept., Directorate of Research, Air Ministry, he is closely in touch with all the latest developments in aircraft and has an accurate knowledge of the work carried out by these boats; and also having the good fortune to be an engineer and not a naval architect, his opinions may be looked upon as unbiassed.

The weights used in compiling Table II., with the exception of those of the P.S.B. and the F.5, for which I can vouch, were taken from Air Ministry weight sheets, which I had assumed to be reasonably correct. I am still of the opinion, and feel sure future designs will show to be the case, provided tank tests are not taken too seriously, that a large flying boat can be designed to carry a useful load at least equal to, if not greater than, the corresponding aeroplane. The air performance on general considerations must be superior. With a propeller suitably designed to meet the water conditions, and light hull loading, I think it will be found unnecessary to provide greater engine weight.

While the single engine flying boat may fulfil certain Service requirements, I doubt very much if it ever will be a commercial proposition, except when operating under conditions outlined in my paper. I think Major Buchanan will agree that there is ample scope for improvement in the aerodynamic and detail design of the small boats in use at present.

In reply to Mr. Knight, the question of the cost of construction is beyond the scope of this paper. With regard to the working of the planking round the chine on the " Fury " hull, I would say this was a comparatively simple operation. The timbers were bent round the stringers in the usual way, and the inner skin, which was fore and aft, fastened on. The two diagonal skins were then laid over, steamed at the bend. The cost would not be at all prohibitive.

ON THE STABILITY OF AERO ENGINES

BY CAPT. J. MORRIS, B.A., A.F.R.AË.S.

Introduction

The question of the stability of an aero engine is one of the utmost importance. An aerodynamically unstable aeroplane does not necessarily involve failure or disaster, but a dynamically unstable engine in an aeroplane is fraught with the gravest consequences. Frequent cases of failure have undoubtedly occurred in practice through ignorance or avoidance of this important problem. At least one Zeppelin was forced to land in this country during the war through failure of a shaft by whirling or torsional resonance and considerable trouble was experienced with the early N.S. airships from this cause. According to the Report of the Advisory Committee for Aeronautics for the year 1918-1919: There have been many failures of the crankshafts in aero engines, and the question is one of some moment. . . . In certain types of engines the airscrew bosses have been found to heat and even burn.

The main problem involved in the stability of aero engines is this: The system as a whole will vibrate during rotation with certain frequencies; in addition, particular items may oscillate independently of the whole system. All possible frequencies, whether for the system or independently for members of the system, should be examined and estimated and compared with the frequency of the engine impulses. At such speeds where resonance occurs, that is, when the natural frequency of oscillation is equal to the engine impulse frequency, failure will occur if that speed is maintained. In addition, failure may occur by whirling, that is, when the speed of rotation is so great that centrifugal force overcomes the tendency of the stiffness of a member to maintain that member in vibration. Whirling is more likely to occur with long shafts supported in bearings at distances from one another; with short shafts and close bearings failure from this cause is unlikely. Failure through torsional resonance is most probably the more frequent phenomenon. The following investigation aims at envisaging the problem in a perfectly general manner. The mathematics involved will be found to be reasonably simple and use of the results only involves an elementary knowledge of algebra.

Within the author's knowledge the treatment is entirely original.

Part I.

General Principles—Resonance, Whirling—The Airscrew and the Flexibility of the Blades.

1. To illustrate the principles involved in the treatment of the stability of rotating shafts, consider a light straight shaft (not necessarily uniform in section) carrying a single concentrated load and running in any bearings.

Let the load be W_1 lbs. and let P_1 be its point of attachment.

Let y_{11} ins. be the static deflection of the shaft at P_1 due to unit load at that point, then if Y_1 is the static deflection at P_1, due to the load W_1, we have

$$Y_1 = W_1 y_{11} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now let W_1 be pulled aside and then let go, the subsequent motion taking place in a horizontal plane.

At time t let Y_1 be the lateral displacement of the load W_1.

Its acceleration will be

$$d^2Y_1/dt^2 \text{ or } \ddot{Y}_1 \text{ radially outwards.}$$

The inertia force at P_1 is

$$-W_1\ddot{Y}_1/g \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Hence the equation of motion of W_1 will be

$$Y_1 = -(W_1\ddot{Y}_1/g)\,y_{11} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

or

$$(m_1y_{11}D^2 + 1)\,Y_1 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where $m_1 = W_1/g$, g being in ins./sec.² and D is the operator d/dt.

The motion is *simple harmonic*.

The frequency N_v—the number of double vibrations per second—will be given by $k/2\pi$ where

$$k^2 = 1/m_1y_{11} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Hence

$$N_v = (1/2\pi)\sqrt{(g/W_1y_{11})} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Now let the shaft rotate as well as vibrate.

If Y_1 is the deflection of the shaft at P_1, the two component accelerations of W_1 will be:

(i.) $\ddot{Y}_1 - \Omega^2Y_1$ radially outwards and

(ii.) $2\Omega\dot{Y}_1$ at right angles to (i.),

where Ω is the angular velocity of rotation of the shaft.

$\ddot{Y}_1 - \Omega^2Y_1$ will in general be large compared with $2\Omega\dot{Y}_1$.

We will suppose that the shaft is bent in one plane.

The force bending the shaft is

$$-W_1(\ddot{Y}_1 - \Omega^2Y_1)/g + W_1\cos\Omega t \quad . \quad . \quad . \quad (7)$$

The first member of (7) being an inertia force and the second the component of the weight.

The equation of motion of W_1 will be

$$Y_1 = [-W_1(\ddot{Y}_1 - \Omega^2Y_1)/g + W_1\cos\Omega t]\,y_{11} \quad . \quad . \quad (8)$$

or

$$(D^2 + g/W_1y_{11} - \Omega^2)\,Y_1 = g\cos\Omega t \quad . \quad . \quad . \quad (9)$$

Thus

$$Y_1 = g\cos\Omega t/(g/W_1y_{11} - 2\Omega^2) + A\cos[\sqrt{(g/W_1y_{11} - \Omega^2)}\,t + \epsilon] \quad . \quad (10)$$

where A and ϵ are arbitrary constants.

The motion of W_1 is made up of two harmonics:

(i.) A forced vibration of frequency $\Omega/2\pi$ due to the revolution of the weight.

(ii.) A vibration of frequency $(1/2\pi)\sqrt{(g/W_1y_{11} - \Omega^2)}$ due to the centrifugal force of W_1.

Instability will arise when

$$2\Omega^2 = g/W_1y_{11} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

At this speed the amplitude of the forced vibration will become infinite. It occurs when

$$\Omega/2\pi = (1/2\pi)\sqrt{(g/W_1y_{11} - \Omega^2)} \quad . \quad . \quad . \quad (12)$$

i.e., when the frequency of the forced vibration due to the revolution of the weight coincides with the natural frequency of the shaft when rotating with angular velocity Ω. Such a speed is called a *critical speed of resonance*.

Instability will also arise when

$$\Omega^2 = g/W_1y_{11} \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (13)$$

At this speed the centrifugal force of the load will overcome the stiffness of the shaft and vibration will cease, the displacement tending to increase indefinitely.

Such a speed is called a *critical speed of whirling.*

In the above case whirling takes place when

$$\Omega/2\pi = (1/2\pi)\sqrt{(g/W_1y_{11})} \qquad . \quad . \quad . \quad . \qquad (14)$$

i.e., when the frequency of rotation of the shaft—the number of revolutions per second—coincides with the frequency of vibration of W_1, when the shaft is not rotating.

It should be observed that if the shaft is vertical there will not be a forced vibration due to the weight of W_1 and consequently there will be no critical speed of resonance, although the critical speed of whirling will be unaffected.

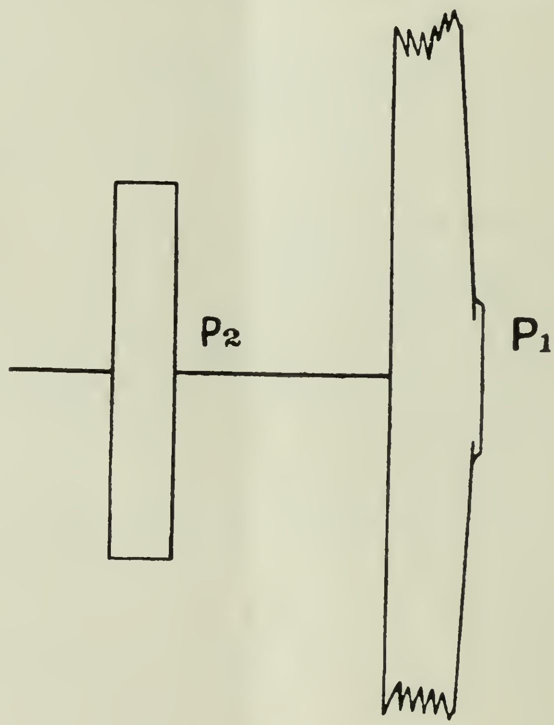


Fig. 1.

2. Consider next an airscrew P_1 driven by a gear wheel P_2 (see Fig. 1). P_2 is subjected to an applied torque T_2 and T_1 is the air torque opposing the rotation of P_1. There are supposed to be bearings in the neighbourhood of both P_1 and P_2, the frictional torques being t_1 and t_2 respectively.

At time t let θ_1 θ_2 be the angular displacements of P_1 and P_2 respectively. Then the equations of motion of P_1 and P_2 are

$$p_1\ddot{\theta}_1 = c_{12}(\theta_2 - \theta_1) - (T_1 + t_1) \qquad . \quad . \quad . \quad . \qquad (1)$$

$$p_2\ddot{\theta}_2 = T_2 - t_2 - c_{12}(\theta_2 - \theta_1) \qquad . \quad . \quad . \quad . \quad . \qquad (2)$$

where $p_1 = I_1/g$, $p_2 = I_2/g$; I_1 and I_2 being the moments of inertia of P_1 and P_2 about the axis of the shaft; c_{12} is the torsional stiffness of the shaft between P_1 and P_2 ($= CI/L$ for a uniform shaft, L being its length, I the polar moment of inertia of its cross section and C its modulus of rigidity).

From (1) and (2) we obtain

$$\theta_{12} = -c_{12}(1/p_1 + 1/p_2)\,\theta_{12} + (T_1 + t_1)/p_1 + (T_2 - t_2)/p_2 \quad . \quad (3)$$

where

$$\theta_{12} = \theta_2 - \theta_1$$

Thus

$$[D^2 + c_{12}(1/p_1 + 1/p_2)]\,\theta_{12} = (T_1 + t_1)/p_1 + (T_2 - t_2)/p_2 \quad . \quad (4)$$

If T_1, T_2, t_1, t_2, are constant we find from (4)

$$\theta_{12} = [(T_1 + t_1)\,p_1 + (T_2 - t_2)\,p_2]/c_{12}(1/p_1 + 1/p_2) + A\cos(kt + \epsilon) \quad . \quad (5)$$

where

$$k^2 = c_{12}(1/p_1 + 1/p_2)$$

and A and ϵ are arbitrary constants. In this case T_1, T_2, t_1, t_2, only affect the mean angle between P_1 and P_2 which remains constant. They do not affect the period of vibration which is the same as the free period when the system is not power driven.

If the mean motion is uniform

$$T_2 - t_2 - T_1 - t_1 = 0$$

so that

$$\theta_{12} = (T_2 - t_2)/c_{12} + A\cos(kt + \epsilon) \quad . \quad . \quad . \quad . \quad (6)$$

In an internal combustion engine, however, T_2 (and consequently T_1) is not constant. T_2 and T_1 in general are periodic functions involving a series of harmonics, the fundamental frequency being the number of firing impulses per second.

If N is the number of revolutions per minute of the crankshaft and n the number of cylinders, the frequency of the firing impulses is

$$nN/120 = f/2\pi \text{ (say)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$(T_1 + t_1)/p_1 + (T_2 - t_2)/p_2$ will in general consist of a Fourier series

$$T + \sum_{q=1}^{q=\infty} (a_q \cos qft + b_q \sin qft) \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where T, a_q, b_q, are constant at any particular speed.

Equation (4) under these conditions becomes

$$(D^2 + k^2)\,\theta_{12} = T + \sum_{q=1}^{q=\infty} (a_q \cos qft + b_q \sin qft) \quad . \quad . \quad (9)$$

The solution of which is

$$\theta_{12} = T/k^2 + \sum_{q=1}^{q=\infty} (a_q \cos qft + b_q \sin qft)/(k^2 - q^2f^2) + A\cos(kt + \epsilon) \quad (10)$$

From (10) we observe that

(i.) There will be imposed upon the system a series of forced vibrations, viz.

$$\sum_{q=1}^{q=\infty} (a_q \cos qft + b_q \sin qft)/(k^2 - q^2f^2) \quad . \quad . \quad . \quad . \quad (11)$$

(ii.) The system will have a natural frequency

$$k/2\pi = (1/2\pi)\sqrt{[c_{12}(1/p_1 + 1/p_2)]} \quad . \quad . \quad . \quad (12)$$

From (II.) we note that the angular difference between P_2 and P_1 will involve an harmonic of infinite amplitude when

$$k^2 = q^2 f^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

or

$$k/2\pi = qf/2\pi \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

i.e., when the natural frequency of vibration of the system is an exact multiple of the frequency of the firing impulses.

At such an engine speed we have instability due to *torsional resonance* and the speed at which it occurs is called the critical speed of torsional resonance.

In the case considered (14) can be written

$$(1/2\pi)\sqrt{[c_{12}(1/p_1 + 1/p_2)]} = qnN/120 \quad . \quad . \quad . \quad (15)$$

N being the r.p.m. of the crankshaft; n the number of cylinders and q a whole number.

If p_1 is large and p_2 is relatively small a first approximation to (15) is

$$(1/2\pi)\sqrt{(c_{12}/p_2)} = qnN/120 \quad . \quad . \quad . \quad . \quad (16)$$

In addition the forces due to reciprocating parts and gravity will be periodic, the frequency being

$$\Omega/2\pi \text{ or } N/60 \quad . \quad . \quad . \quad . \quad . \quad (17)$$

where Ω is the angular velocity of the system and N the corresponding r.p.m.

Thus resonance will occur when

$$k/2\pi = qN/60 \quad . \quad . \quad . \quad . \quad . \quad (18)$$

where $k/2\pi$ is the frequency of the system or an individual member of the system.

The importance of values of q above unity in the case of (15) can only be assessed by experiment. Failure will certainly occur when $q = 1$ and vibration may be violent when $q = 2$, becoming less marked as q increases.

It remains to try out by experiment the resonance as indicated by (18).

3. In the preceding problem the airscrew was regarded as a rigid body. This, of course, is not the case in practice. In general the blades of an airscrew are flexible and this flexibility may have an appreciable influence on the periods of vibration of a system of which an airscrew is a part. Owing to the complexity of the section of an airscrew it would prove extremely difficult to estimate this effect by analysis.

If the blades could be regarded as light flexible arms carrying loads at the tips the problem would be greatly simplified. In order to investigate the possibility of an actual airscrew being replaced by such a system consider first the following problem (see Fig. 2):

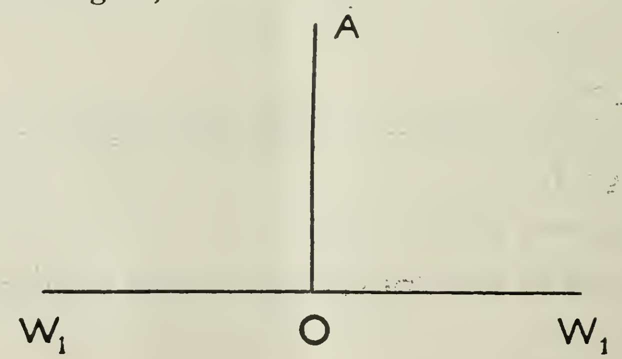


FIG. 2.

AO is a vertical bar fixed at A. $W_1\ O\ W_1$ is a pair of equal light flexible arms carrying tip loads W_1. The arms are displaced in a horizontal plane and then released so that the system on the whole executes pure torsional oscillations about AO.

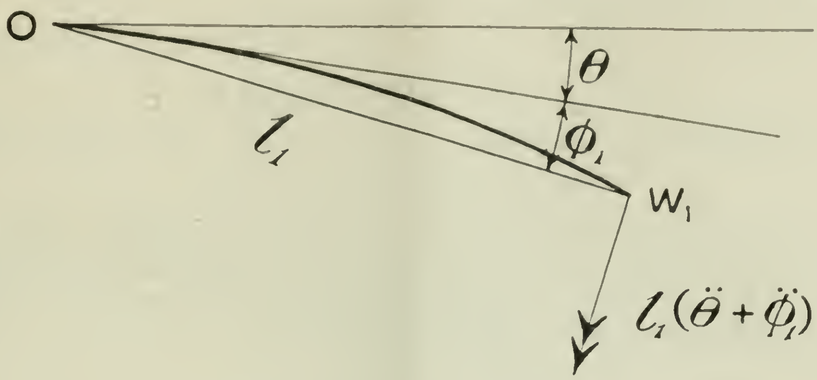


Fig. 3.

Fig. 3 is a plan view of one of the displaced arms. θ is the angular displacement of the normal line of the arms and ϕ_1 the angular displacement of W_1 due to the flexibility of the arm.

The acceleration of W_1 as shown is

$$l_1\,(\ddot{\theta} + \ddot{\phi}_1) \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

Thus the arm regarded as a cantilever has a deflection $l_1\phi_1$ due to an inertia force

$$-W_1 l_1\,(\ddot{\theta} + \ddot{\phi}_1)/g \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

Hence the equation of motion of W_1 is

$$l_1\phi_1 = -W_1 l_1\,(\ddot{\theta} + \ddot{\phi}_1)\,y_{11}/g \quad . \quad . \quad . \quad . \qquad (3)$$

where y_{11} is the deflection of the arm, regarded as cantilever, due to unit load at rest.

Similarly for the other arm

$$l_1\phi_2 = -W_1 l_1\,(\ddot{\theta} + \ddot{\phi}_2)\,y_{11}/g \quad . \quad . \quad . \quad . \qquad (4)$$

Also by moments about the axis of OA

$$-W_1 l_1^2\,(\ddot{\theta} + \ddot{\phi}_1)/g - W_1 l_1^2\,(\ddot{\theta} + \ddot{\phi}_2)/g = c_{12}\theta \quad . \quad . \qquad (5)$$

where $c_{12} = CI/L$ for the bar.

Writing

$$c_1 = l_1^2/y_{11} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

and

$$p_1 = W_1 l_1^2/g \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

(3), (4) and (5) can be written

$$c_1\phi_1 = -p_1\,(\ddot{\theta} + \ddot{\phi}_1) \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

$$c_1\phi_2 = -p_1\,(\ddot{\theta} + \ddot{\phi}_2) \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

or

$$(D^2/c_1 + 1/p_1)\,[c_1\phi_1 \text{ or } c_1\phi_2] = -D^2\theta \quad . \quad . \quad . \qquad (10)$$

and

$$c_1\,(\phi_1 + \phi_2) = c_{12}\theta \quad . \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

will be the bending moment at O.

From (10) we find

$$(D^2/c_1 + 1/p_1)\,c_1\,(\phi_1 - \phi_2) = 0 \quad . \quad . \quad . \qquad (12)$$

Thus one of the frequencies of the arms will be

$$(1/2\pi)\sqrt{(c_1/p_1)} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (13)$$

which is the same as that for the free vibration of one of these arms.

From (10) and (11) we find

$$2D^2\theta + (D^2/c_1 + 1/p_1)\, c_{12}\theta = 0 \quad . \quad . \quad . \quad . \qquad (14)$$

or

$$[(1/c_{12} + 1/2c_1)\, D^2 + 1/2p_1]\, \theta = 0 \quad . \quad . \quad . \quad . \qquad (15)$$

From which we obtain the frequency

$$1/2\pi\sqrt{[2p_1\,(1/c_{12} + 1/2c_1)]} \quad . \quad . \quad . \quad . \quad . \qquad (16)$$

4. Next let the arms be replaced by an actual airscrew. Let θ be the displacement of the normal line of the airscrew blades and let y_1 be the displacement from the normal line of the airscrew blades of an element dx on one side, at a distance x from the centre line of the vertical bar.

The acceleration of this element will be

$$d^2y_1/dt^2 + x\,(d^2\theta/dt^2) \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

so that the equation of motion will be

$$(d^2/dx^2)\,(EI_x\, d^2y_1/dx^2) = -(\rho A_x/g)\,(d^2y_1/dt^2 + x\, d^2\theta/dt^2) \quad . \qquad (2)$$

where A_x is the area of cross section at the element, I_x the appropriate moment of inertia of this section, ρ the density of the material and E its Young's modulus. Similarly for the complementary element on the other blade

$$(d^2/dx^2)\,(EI_x\, d^2y_2/dt^2) = -(\rho A_x/g)\,(d^2y_2/dt^2 + x\, d^2\theta/dt^2) \quad . \qquad (3)$$

From (2) and (3) we have

$$(d^2/dx^2)\,(EI_x\, d^2y/dx^2) = -(\rho A_x/g)\, d^2y/dt^2 \quad . \quad . \quad . \qquad (4)$$

where

$$y = y_1 - y_2 \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

The solution of (4) will be the same as that for the free vibration of one of the blades. Hence the blades will have in addition to any others the frequencies which each would have in free vibration.

If the blades were of uniform section (4) becomes

$$EI\,(d^4y/dx^4) = -(w/g)\, d^2y/dt^2 \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

where w is the weight per unit length.

Let

$$y = XT$$

where X is a function of x only and T a function of t only. Then (6) becomes

$$EI\,(d^4X/dx^4)/X = -(w/g)\,(d^2T/dt^2)/T \quad . \quad . \quad . \qquad (7)$$

Let now

$$d^4X/dx^4 = \lambda^4X \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

and

$$d^2T/dt^2 = -k^2T \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

so that

$$EI\lambda^4 = wk^2/g \quad . \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

and

$$y = \cos\,(kt + \epsilon)\,[\alpha\cosh\lambda x + \beta\sinh\lambda x + \gamma\cos\lambda x + \delta\sin\lambda x] \quad . \qquad (11)$$

where α, β, γ, δ and ϵ are arbitrary constants.

When $x = 0$, $y_1 = 0$, $y_2 = 0$; $dy_1/dx = 0$, $dy_2/dx = 0$ for all t so that

$$\alpha + \gamma = 0 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (12)$$

and

$$\beta + \delta = 0 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (13)$$

When $x = l_1$, $d^2y_1/dx^2 = 0$, $d^2y_2/dx^2 = 0$; $d^3y_1/dx^3 = 0$, $d^3y_2/dx^3 = 0$ for all t so that

$$\alpha(\cosh\lambda l_1 + \cos\lambda l_1) + \beta(\sinh\lambda l_1 + \sin\lambda l_1) = 0 \quad . \quad . \quad (14)$$

and

$$\alpha(\sinh\lambda l_1 - \sin\lambda l_1) + \beta(\cosh\lambda l_1 + \cos\lambda l_1) = 0 \quad . \quad . \quad (15)$$

From (14) and (15) we find

$$\cosh\lambda l_1 \cos\lambda l_1 + 1 = 0 \quad . \quad . \quad . \quad . \quad . \quad (16)$$

The lowest value of λl_1 which satisfies (16) is

$$\lambda l_1 = 1.8731 \quad . \quad . \quad . \quad . \quad . \quad (17)$$

or

$$\lambda^4 l_1^4 = 12.46 \quad . \quad . \quad . \quad . \quad . \quad (18)$$

Now

$$EI\lambda^4 = wk^2/g \quad . \quad . \quad . \quad . \quad . \quad (19)$$

$$\therefore\ k^2 = 12.46\, EIg/wl_1^4 \quad . \quad . \quad . \quad . \quad (20)$$

or

$$k^2 = (3EI/l_1)/(Wl_1^2/4.11\, g) \quad . \quad . \quad . \quad . \quad (21)$$

where $W = wl_1$, the weight of one blade.

Thus

$$k^2 = c_1/p_1 \quad . \quad . \quad . \quad . \quad . \quad (22)$$

where

$$c_1 = 3EI/l_1 \text{ and } p_1 = Wl_1^2/4.11\, g$$

A sufficiently close approximation is

$$k^2 = 4gc_1/Wl_1^2 \quad . \quad . \quad . \quad . \quad . \quad (23)$$

Hence the equivalent tip load for free vibration of the blades when of uniform section is $W/4$.

Unfortunately the tip load for free vibration does not necessarily give the correct bending moment at the root and in an endeavour to overcome this difficulty we will investigate this bending moment.

The equations of motion of the blades are

$$EId^4(y_1 \text{ or } y_2)/dx^4 = -(w/g)[d^2(y_1 \text{ or } y_2)/dt^2 + xd^2\theta/dt^2] \quad . \quad (24)$$

or

$$EI(d^4y/dx^4) = -(w/g)[d^2y/dt^2 + 2xd^2\theta/dt^2] \quad . \quad . \quad (25)$$

where

$$y = y_1 + y_2$$

A solution of (25) is

$$y + 2x\theta = \cos(kt + \epsilon)[\alpha\cosh\lambda x + \beta\sinh\lambda x + \gamma\cos\lambda x + \delta\sin\lambda x] \quad . \quad (26)$$

where

$$EI\lambda^4 = wk^2/g$$

When $x = 0$, $y_1 = 0$, $y_2 = 0$; $dy_1/dx = 0$, $dy_2/dx = 0$ for all t so that

$$\alpha + \gamma = 0 \quad . \quad . \quad . \quad . \quad . \quad (27)$$

and

$$2\theta = (\beta + \delta)\lambda\cos(kt + \epsilon) \quad . \quad . \quad . \quad . \quad (28)$$

When $x = l_1$, $d^2y_1/dx^2 = 0$, $d^2y_2/dx^2 = 0$; $d^3y_1/dx^3 = 0$, $d^3y_2/dx^3 = 0$ for all t so that

$$\alpha(\cosh\lambda l_1 + \cos\lambda l_1) + \beta\sinh\lambda l_1 - \delta\sin\lambda l_1 = 0 \quad . \quad . \quad (29)$$

and

$$\alpha(\sinh\lambda l_1 - \sin\lambda l_1) + \beta\cosh\lambda l_1 - \delta\cos\lambda l_1 = 0 \quad . \quad . \quad (30)$$

Thus

$$2\theta = 2\alpha\cos(kt+\epsilon)[\lambda\cosh\lambda l_1\cos\lambda l_1 + 1]/(\cosh\lambda l_1\sin\lambda l_1 - \cos\lambda l_1\sinh\lambda l_1) \quad (31)$$

Also by moments

$$EI\,(d^2y_1/dx^2)_{x\,=\,0} + EI\,(d^2y_2/dx^2)_{x\,=\,0} = c_{12}\theta \quad . \quad . \quad (32)$$

or

$$EI\,(d^2y/dx^2)_{x\,=\,0} = c_{12}\theta \quad . \quad . \quad . \quad . \quad (33)$$

the c_{12} referring to the bar, or

$$2EIa\lambda^2 \cos(kt + \epsilon) = c_{12}\theta \quad . \quad . \quad . \quad . \quad (34)$$

Hence from (31) and (34) we find

$$2c_0/3c_{12} = (\cosh \lambda l_1 \cos \lambda l_1 + 1)/\lambda l_1 (\cosh \lambda l_1 \sin \lambda l_1 - \cos \lambda l_1 \sinh \lambda l_1) \quad (35)$$

where

$$c_0 = 3EI/l_1 \text{ and } c_{12} = CI/L$$

the former referring to the blades and the latter to the bar.

As a first approximation we find

$$1/c_0\lambda^4 l_1^4 = (2/9)\,[(1/c_{12}) + (99/140)\,(1/2c_0)] \quad . \quad . \quad (36)$$

and since

$$EI\lambda^4 = wk^2/g$$

we have approximately

$$k^2 = 3g/8W_0 l_1^2\,(1/c_{12} + 3/8c_0) \quad . \quad . \quad . \quad (37)$$

where W_0 is the equivalent tip load of one blade for free vibration (*i.e.*, $W/4$, where W is the weight of the blade).

If the blades were light, of flexural coefficients c_1 and had tip loads W_1.

$$k^2 = g/2W_1 l_1^2\,(1/c_{12} + 1/2c_1) \quad . \quad . \quad . \quad . \quad (38)$$

Thus, comparing (37) with (38), the frequencies with the uniform loading will be approximately the same as if the blades were light, the tip being loaded with $1/3$ the weight of each blade, or $4/3$ the equivalent tip load in free vibration and c_0 increased in the same proportion $4/3$ times. It should be observed that the tip loads $W/3$ will give the same moment of inertia of the airscrew about its axis of revolution as its own weight.

5. From §3 and §4 the treatment suggested in the case of an actual airscrew is as follows:—

Let W_0 be the equivalent tip load for one blade in free vibration and let c_0 be its flexural coefficient. Next let W_0 and c_0 be increased in the same ratio to W_1 and c_1 so that $W_1/W_0 = c_1/c_0$ is the necessary factor in order that the bending moment at the root of the blade should be correct. This increase will not affect the period of free vibration. The blade is thus reduced to a light arm of known stiffness loaded at the tip.

Similarly, quantities are found for the case when the blades vibrate in a fore and aft direction.

These quantities can be found by experiment, but before indicating an experimental method for finding them we will investigate the bending of a light cantilever of uniform section under two end loads, one vertical and the other inclined at a small angle to the horizon (see Fig. 4). The object of this investigation is to examine how the centrifugal force of the equivalent tip loads influences the main problem under consideration.

RP is the cantilever bent under the vertical load W and the load F at an angle ψ to RQ. The line of action of F meets the vertical at R at the point S, so that $RS = \xi$ and $RQS = \theta$, PQ being vertical and $= \eta$.

N.B.—P is the point where the lines of action of W and F meet.

If l is the length of the cantilever

$$l\psi = \xi + \eta \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The bending moment at a distance x from R the point of fixture is

$$EI\,(d^2y/dx^2) = (W + F\psi)\,(l - x) - F\,(\eta - y) \quad . \quad . \quad (2)$$

or

$$EI\,(d^2y/dx^2) - Fy = (W + F\psi)\,(l - x) - F\eta \qquad (3)$$

where EI is the flexural rigidity of the cantilever.

$$\therefore\ y = A \sinh\sqrt{(F/EI)}\,x + B\cosh\sqrt{(F/EI)}\,x - (W/F + \psi)\,(l - x) + \eta \qquad (4)$$

When

$$x = 0,\ y = 0\ \therefore\ B - (Wl/F + \xi) = 0 \qquad (5)$$

when

$$x = 0,\ dy/dx = 0\ \therefore\ A\sqrt{(F/EI)} + [W/F + (\xi + \eta)/l] = 0 \qquad (6)$$

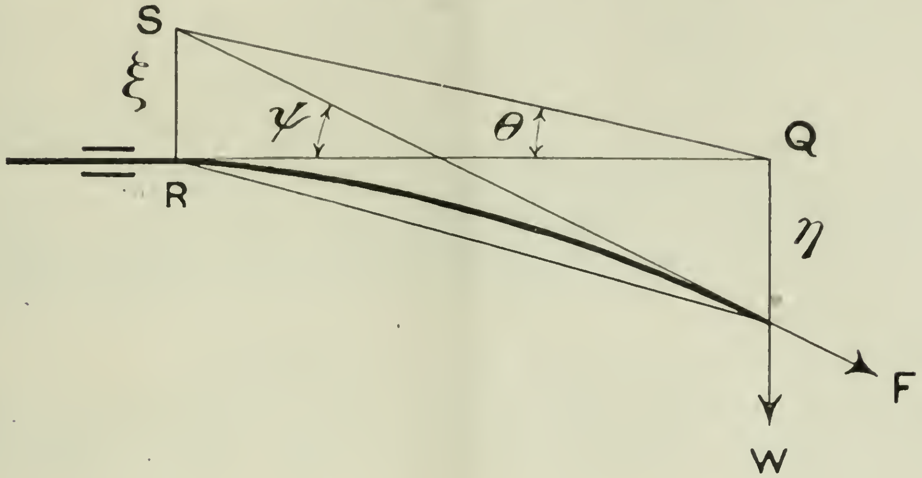


Fig. 4.

Also when $x = l,\ y = \eta$

$$\therefore\ \eta = -[W/F + (\xi + \eta)/l]\sqrt{(EI/F)}\sinh\sqrt{(F/EI)}\,l + (Wl/F + \xi)\cosh\sqrt{(F/EI)}\,l + \eta \qquad (7)$$

leading to

$$\eta = (W + F\theta)\,\Phi(\alpha)\,l^3/3EI \qquad (8)$$

where $\Phi(\alpha)$ is the Berry function,

$$3\,(\alpha\coth\alpha - 1)/\alpha^2 \qquad (9)$$

and

$$\alpha = \sqrt{(F/EI)}\,l \qquad (10)$$

When F is upwardly inclined θ will be negative.

It is to be observed that when α is small, *i.e.*, when Fl^2/EI is small, $\Phi(\alpha)$ will be practically unity.

In cases which we shall deal with F will usually be centrifugal force and having regard to the practical speeds and the stiffness of the members considered α will in general be small, so that approximately

$$\eta = (W + F\theta)\,l^3/3EI \qquad (11)$$

The fixing couple will be

$$(W + F\theta)\,l \qquad (12)$$

We also find that the slope at P is

$$(W + F\theta)\,\Psi(\alpha)\,(l^2/2EI) \qquad (13)$$

where

$$\Psi(\alpha) = (\tanh\tfrac{1}{2}\alpha)/\tfrac{1}{2}\alpha \qquad (14)$$

and which is practically unity when α is small.

If F was absent the deflection and slope at P, due to the load W, would be $Wl^3/3EI$, $Wl^2/2EI$, respectively.

Thus the effect of the lateral load F on the deflection and slope of the shaft at P is the same as if W were increased to $W + F\theta$ and there was no lateral load.

When $\theta = 0$, *i.e.*, when the line of action of F passes through the point of fixture of the cantilever the deflection is

$$Wl^3/3EI \qquad (15)$$

approximately, the slope is

$$Wl^2/2EI \qquad (16)$$

approximately, and the fixing couple is

$$Wl \qquad (17)$$

Under these circumstances the lateral load has practically no influence on the deflection of the shaft due to the vertical load.

6. We will now consider an experimental determination of the equivalent tip loads and flexural coefficients of the blades of an airscrew.

Let the airscrew be attached to one end of a straight uniform bar of circular section, the other end of the bar being fixed. In the normal position the bar is vertical and the plane of rotation of the airscrew is horizontal.

The system is given a torsional displacement about the axis of the vertical bar and then let go so as to execute, as far as can be observed, pure torsional oscillations. The bar is of such length that the periods of these oscillations can be readily measured.

Suppose in the first case the airscrew is two-bladed.

Let W_1 be the equivalent tip load and c_1 the equivalent flexural coefficient for each blade. We have already proved in §3 that such a system will have frequencies as follows:

(i.) For the system

$$1/2\pi\sqrt{[2p_1(1/c_{12} + 1/2c_1)]} \qquad (1)$$

and (ii.) for the blades in addition

$$(1/2\pi)\sqrt{(c_1/p_1)} \qquad (2)$$

(i.) will be the fundamental frequency for the system with the prearranged conditions.

If T be the period of oscillation of the fundamental

$$T^2/4\pi^2 = 2p_1(1/c_{12} + 1/2c_1) \qquad (3)$$

If $T^2/4\pi^2$ be plotted against $1/c_{12}$ we obtain a straight line. The slope of this line will be

$$2p_1 \text{ or } 2W_1l_1^2/g \qquad (4)$$

and the intercept in the $1/c_{12}$ axis will be

$$1/2c_1 \qquad (5)$$

Thus we find both p_1 and c_1 and consequently we know the other frequency of the blades which is $k/2\pi$ where

$$k^2 = c_1/p_1 \qquad (6)$$

Now c_0, the actual flexural coefficient of a blade, can be found by direct measurement. Suppose a load W, attached at the tip of one blade bent in the plane of rotation of the airscrew, produces a deflection d_1 then

$$c_0 = Wl_1^2/d_1 \qquad (7)$$

Since $W_0/W_1 = c_0/c_1$ we also have W_0 the actual equivalent tip load in free vibration.

In the case of a four-bladed airscrew we find for the tip loads

$$(D^2/c_1 + 1/p_1)c_1\phi_r\,(r = 1, 2, 3 \text{ or } 4) = -D^2\theta \qquad (8)$$

and by moments

$$c_1(\phi_1 + \phi_2 + \phi_3 + \phi_4) = c_{12}\theta \qquad (9)$$

From which we obtain

(i.) A frequency of the system

$$1/2\pi\sqrt{[4p_1(1/c_{12} + 1/4c_1)]} \qquad (10)$$

(ii.) A blade frequency in addition

$$(1/2\pi)\sqrt{(c_1/p_1)} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

Thus

$$T^2/4\pi^2 = 4p_1(1/c_{12} + 1/4c_1) \quad . \quad . \quad . \quad . \quad . \qquad (12)$$

From which we can obtain p_1 and c_1 as before.

7. Next, to ascertain W'_1 and c'_1 the equivalent tip load and flexural coefficient of each blade in planes perpendicular to that of rotation of the airscrew.

A two-bladed airscrew could be turned through 90° about the original axis of the blades and the same procedure as in §6 will give the quantities required.

This, however, may not be convenient and is certainly impracticable in the case of a four-bladed airscrew.

The following method is suggested:

Let the system be so displaced that the bar and the blades vibrate in the vertical plane, the oscillation on the whole being lateral. At time t let Y_1 be the displacement of the mass centre of the airscrew from the vertical and let Φ_1 be the slope of the bar at that point (see Fig. 5).

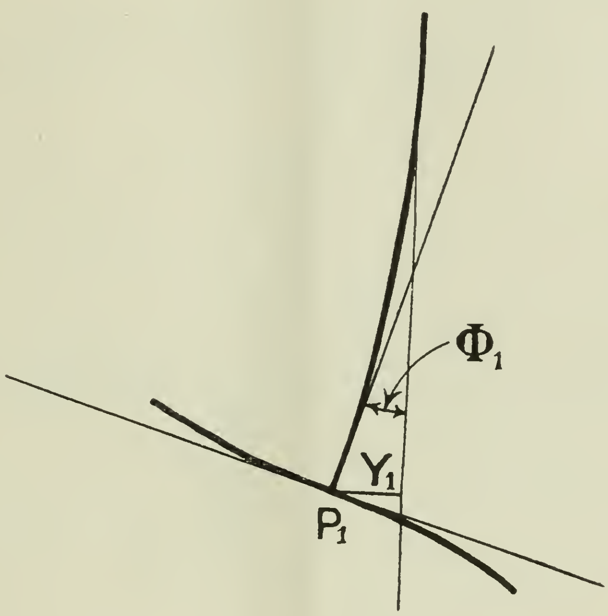


FIG. 5.

Let ϕ'_1, ϕ'_2, be the angular displacements of the tip loads due to the bending of the blades.

For the equations of motion of the tip loads we have

$$(D^2/c'_1 + 1/p'_1)[c'_1\phi'_1 \text{ or } c'_1\phi'_2] = -D^2\Phi_1 \quad . \quad . \quad . \qquad (1)$$

where

$$p'_1 = W'_1 l_1^2/g$$

There will be acting on the bar at P_1

(i.) An inertia force

$$-2W\ddot{Y}_1/g \text{ or } -2m\ddot{Y}_1 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

where $m = W/g$, W being the weight of each blade.

(ii.) An inertia couple

$$c'_1(\phi'_1 + \phi'_2) \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

Hence for the bar

$$Y_1 = -2m\ddot{Y}_1 y_{11} + c'_1 (\phi'_1 + \phi'_2) z_{11} \quad . \quad . \quad . \quad (4)$$

$$\Phi_1 = -2m\ddot{Y}_1 \psi_{11} + c'_1 (\phi'_1 + \phi'_2) \phi_{11} \quad . \quad . \quad . \quad (5)$$

where y_{11} = displacement of P_1 due to unit load at rest.

z_{11} = displacement of P_1 due to unit couple at rest.

ψ_{11} = slope at P_1 due to unit load at rest.

and ϕ_{11} = slope at P_1 due to unit couple at rest.

From (4) and (5) we have

$$Y_1\phi_{11} - \Phi_1 z_{11} = -2m\Delta_1 \ddot{Y}_1 \quad . \quad . \quad . \quad . \quad (6)$$

or

$$(2m\Delta_1 D^2 + \phi_{11}) Y_1 = \Phi_1 z_{11} \quad . \quad . \quad . \quad . \quad (7)$$

where

$$\Delta_1 = y_{11}\phi_{11} - z_{11}\psi_{11}$$

Also from (1) and (4) we obtain

$$(D^2/c'_1 + 1/p'_1)(2my_{11}D^2 + 1) Y_1 = -2z_{11}D^2\Phi_1 \quad . \quad . \quad (8)$$

Thus from (7) and (8) we find

$$[2D^2(2m\Delta_1 D^2 + \phi_{11}) + (D^2/c'_1 + 1/p'_1)(2my_{11}D^2 + 1)]\,[Y_1 \text{ or } \Phi_1] = 0 \quad (9)$$

From which we obtain two frequencies $k_1/2\pi$, $k_2/2\pi$, where k_1^2, k_2^2 are roots of the equation in k^2.

$$2m(\Delta_1 + y_{11}/2c'_1)k^4 - (\phi_{11} + 1/2c'_1 + my_{11}/p'_1)k^2 + 1/2p'_1 = 0 \quad . \quad . \quad (10)$$

or

$$(1/2p'_1)(T^2/4\pi^2)^2 - (\phi_{11} + 1/2c'_1 + my_{11}/p'_1)(T^2/4\pi^2)$$
$$+ 2m(\Delta_1 + y_{11}/2c'_1) = 0 \quad . \quad . \quad . \quad . \quad (11)$$

where T (the period) = $2\pi/k$.

Now y_{11}, ϕ_{11} and Δ_1 depend on the bar and are known. Also m is known. By varying the length of the bar we can obtain mean values for T and from two appropriate relations (11) we can find p'_1 and c'_1.

In the case of a four-bladed airscrew we find

$$(1/2p'_1)(T^2/4\pi^2)^2 - (\phi_{11} + 1/2c'_1 + 2my_{11}/p'_1)(T^2/4\pi^2)$$
$$+ 4m(\Delta_1 + y_{11}/2c'_1) = 0 \quad . \quad . \quad . \quad . \quad (12)$$

In addition there will be the frequency

$$(1/2\pi)\sqrt{(c'_1/p'_1)}$$

c'_0 can be found by direct measurement and

$$W'_0/W'_1 = c'_0/c'_1$$

where W'_0 is the actual equivalent tip load for free vibration in the direction considered.

For a uniform bar used as a cantilever as in the above experiments

$$y_{11} = L^3/3EI$$
$$z_{11} = \psi_{11} = L^2/2EI$$
$$\phi_{11} = L/EI$$

and

$$\Delta_1 = y_{11}\phi_{11} - z_{11}\psi_{11} = L^4/12(EI)^2$$

where L is the length of the bar and EI its flexural rigidity for bending.

8. To return to the problem of §2 in which an airscrew and gear wheel are both fixed to the same shaft, the airscrew being driven by the gear wheel.

The blades of the airscrew are regarded as light flexible arms of equivalent flexural coefficients c_1 and carrying equivalent tip loads W_1. The air loading of

the blades will be of a periodic nature of fundamental period, that of the engine period.

Fig. 6 is an end view. θ_2 is the displacement of the pulley P_2. The normal line of the blades is supposed displaced through an angle θ_1 and ϕ_1, ϕ_2 (supposing there are two blades) are the angular displacements of the equivalent tip loads from the normal lines of the blades. The air loading is taken as a series of air forces f_1, f_2, f_3, f_4, . . . at points 1, 2, 3, 4, . . . along the blade.

We have for the displacement at W_1

$$l_1\phi_1 = -W_1 l_1 (\ddot{\phi}_1 - \ddot{\theta}_1) y_{11}/g + f_1 y'_{11} + f_2 y'_{21} + \ldots f_n y'_{n1} + \ldots \qquad (1)$$

Where y'_{11} is the displacement at point 1, due to unit static load, and y'_{n1} is the displacement at the point 1, due to unit static load, at the point n, the blade

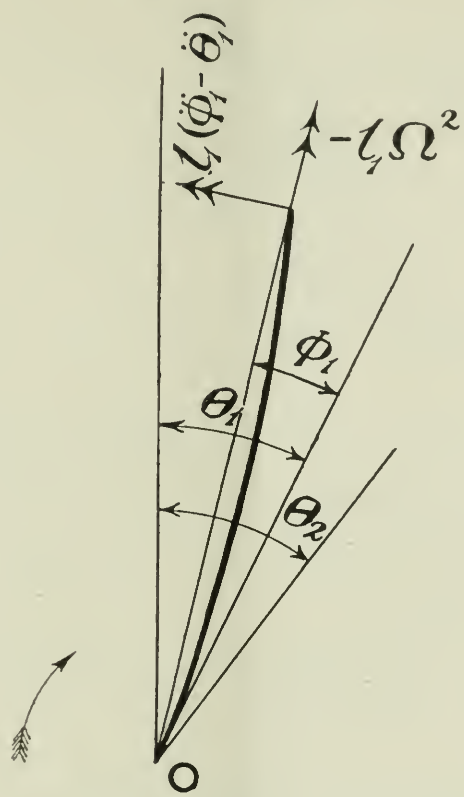


FIG. 6.

being regarded as a cantilever; and y_{11} is the displacement at point 1, due to unit static load, of the equivalent blade.

Thus (1) can be written

$$c_1\phi_1 = -p_1(\ddot{\phi}_1 - \ddot{\theta}_1) + (l_1/y_{11})\,\Sigma f_n y'_{n1} \qquad (2)$$

Now the y''s on the right hand side of (2) are elastic constants depending on the stiffness of the blade and the f's are periodic functions of fundamental frequency, that of the engine impulses.

Hence

$$c_1\phi_1 = -p_1(\ddot{\phi}_1 - \ddot{\theta}_1) + T'_1 \qquad (3)$$

or

$$(D^2/c_1 + 1/p_1)\,c_1\phi_1 = D^2\theta_1 + T'_1/p_1 \qquad (4)$$

where T'_1 is a Fourier's series.

Similarly

$$(D^2/c_1 + 1/p_1)\,c_1\phi_2 = D^2\theta_1 + T''_1/p_1 \qquad (5)$$

We have taken T''_1 different from T'_1 to allow for any inequality of the blades in motion.

Also by moments

$$p_1(\ddot{\phi}_1 - \ddot{\theta}_1) + p_1(\ddot{\phi}_2 - \ddot{\theta}_1) = T_1 - c_{12}(\theta_2 - \theta_1) \quad . \quad . \quad (6)$$

where T_1 is the nett air torque and c_{12} is the coefficient of torsional flexure of the shaft between the airscrew and P_2.

Using (4) and (5), (6) can be written

$$-c_1(\phi_1 + \phi_2) + T'_1 + T''_1 = T_1 - c_{12}\theta_{12} \quad . \quad . \quad . \quad (7)$$

or

$$-c_1(\phi_1 + \phi_2) + c_{12}\theta_{12} = T_1 - (T'_1 + T''_1) = T'''_1 \text{ (say)} \quad . \quad (8)$$

where

$$\theta_{12} = \theta_2 - \theta_1$$

Hence

$$-(D^2/c_1 + 1/p_1)c_1(\phi_1 + \phi_2) + c_{12}(D^2/c_1 + 1/p_1)\theta_{12} = (D^2/c_1 + 1/p_1)T'''_1 \quad (9)$$

and by the use of (4) and (5)

$$-2D^2\theta_1 - (T'_1 + T''_1)/p_1 + c_{12}(D^2/c_1 + 1/p_1)\theta_{12} = (D^2/c_1 + 1/p_1)T'''_1 \quad (10)$$

or

$$-2D^2\theta_1 + c_{12}(D^2/c_1 + 1/p_1)\theta_{12} = T \text{ (say)} \quad . \quad . \quad . \quad (11)$$

We have also

$$p_2\ddot{\theta}_2 = T_2 - c_{12}\theta_{12} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Hence from (11) and (12)

$$2D^2\theta_{12} + (D^2/c_1 + 1/p_1 + 2/p_2)c_{12}\theta_{12} = 2T_2/p_2 + T \quad . \quad (13)$$

or

$$[(1/2c_1 + 1/c_{12})D^2 + 1/2p_1 + 1/p_2]\theta_{12} = (2T_2/p_2 + T)/2c_{12} \, . \quad (14)$$

Hence the ordinary frequency of the system will be:

$$(1/2\pi)\sqrt{[(1/2p_1 + 1/p_2)/(1/2c_1 + 1/c_{12})]} \quad . \quad . \quad . \quad (15)$$

and torsional resonance will occur when

$$(1/2\pi)\sqrt{[(1/2p_1 + 1/p_2)/(1/2c_1 + 1/c_{12})]} = qnN/120 \quad . \quad (16)$$

From (4) and (5) we find

$$(D^2/c_1 + 1/p_1)c_1(\phi_1 - \phi_2) = (T'_1 - T''_1)/p_1 \quad . \quad . \quad (17)$$

From which we find that the blades will have in addition to the frequency (15) another of frequency

$$(1/2\pi)\sqrt{(c_1/p_1)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

that is, the ordinary frequency of a separate blade.

Where T'_1 is not $= T''_1$ there may be resonance when

$$(1/2\pi)\sqrt{(c_1/p_1)} = qnN/120 \quad . \quad . \quad . \quad . \quad (19)$$

The above investigation leads to the following conclusions:

Any part of the system may fail by resonance as given by (16). The airscrew, or the shaft by the resulting increasing oscillation of the airscrew blades, may fail by resonance as given by (19).

9. Assuming for the present that the airscrew shaft at the point of attachment of the airscrew maintains its original direction during rotation, each blade can vibrate in a plane perpendicular to that of rotation as shown in Fig. 7.

If ϕ'_1 is the angular displacement of one particular tip load W'_1 in the direction considered, the accelerations will be as shown in the figure.

We have for this particular blade

$$l_1\phi'_1 = [-W'_1l_1\ddot{\phi}'_1/g - W'_1l_1\Omega^2\phi'_1/g]\,y'_{11} + \Sigma f'_n y''_{n1} \quad . \quad . \quad (1)$$

or

$$c'_1\phi'_1 = -p'_1(D^2 + \Omega^2)\phi'_1 + (l_1/y'_{11})\Sigma f'_n y''_{n1} \quad . \quad . \quad (2)$$

where

$$c'_1 = l_1^2/y'_{11} \text{ and } p'_1 = W'_1 l_1^2/g$$

and the f's and y's have corresponding meanings to those in §8.

Thus

$$(D^2 + \Omega^2 + c'_1/p'_1)\,\phi'_1 = (l_1/p'_1 y'_{11})\,\Sigma f'_n y''_{n1} \quad . \quad . \qquad (3)$$

or

$$(D^2 + \Omega^2 + c'_1/p'_1)\,\phi'_1 = T'_1$$

where T'_1 is a Fourier series of fundamental frequency that of the engine impulses.

Hence the frequency of the blade for the motion considered will be

$$(1/2\pi)\sqrt{(\Omega^2 + c'_1/p'_1)} \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

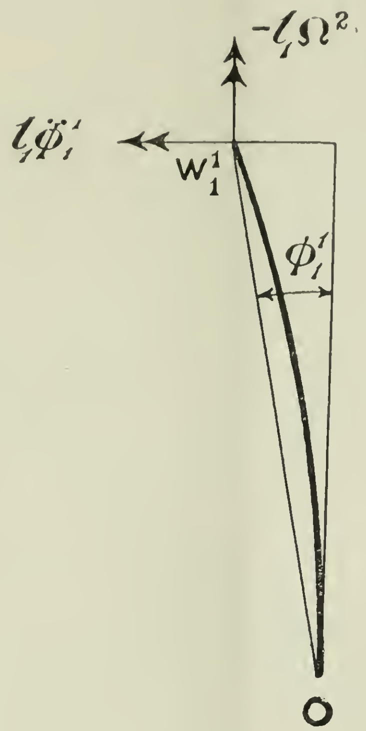


Fig. 7.

and resonance may occur when

$$(1/2\pi)\sqrt{(\Omega^2 + c'_1/p'_1)} = qnN/120 \quad . \quad . \quad . \qquad (5)$$

10. Take next the case when the airscrew is close up to a short bearing so that the airscrew shaft at the point of attachment of the airscrew can adopt any slope. The gear wheel will be supposed negligible in comparison with the airscrew both as regards weight and moments of inertia.

The airscrew is regarded as being so close up to the nearest bearing that only angular deflection is appreciable.

To consider the motion as shown in Fig. 8, assuming that the shaft and the two conjugate blades of the airscrew are bent in one plane.

The applied periodic forces are neglected as they only affect the forced vibrations.

For the arms of P_1 in the plane of bending we have

$$c'_1\phi'_1 = -p_1(D^2 + \Omega^2)(\Phi_1 + \phi'_1) \quad . \quad . \quad . \qquad (1)$$

or

$$[(D^2 + \Omega^2)/c'_1 + 1/p'_1]\,c'_1\phi'_1 = -(D^2 + \Omega^2)\,\Phi_1 \quad . \quad . \qquad (2)$$

and

$$c'_1\phi'_2 = -p_1(D^2 + \Omega^2)(\Phi_1 + \phi'_2) \quad . \quad . \quad . \qquad (3)$$

or

$$[(D^2 + \Omega^2)/c'_1 + 1/p'_1]\,c'_1\phi'_2 = -(D^2 + \Omega^2)\,\Phi_1 \quad . \quad . \qquad (4)$$

The inertia couple bending the shaft at P_1 will be

$$c'_1 (\phi'_1 + \phi'_2) \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

In the case of a four-bladed airscrew the other pair of blades will have no effect in the motion considered. Thus for the shaft at P_1

$$\Phi_1 = c'_1 (\phi'_1 + \phi'_2) \phi_{11} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

where ϕ_{11} is the slope due to unit couple.

From (2) and (4) we have for the blades a frequency

$$(1/2\pi) \sqrt{(\Omega^2 + c'_1/p'_1)} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

From (2), (4) and (6) we find

$$[D^2 + \Omega^2 + c'_1/p'_1 (1 + 2c'_1\phi_{11})] \Phi_1 = 0 \quad . \quad . \quad . \quad . \qquad (8)$$

leading to a frequency for the system

$$(1/2\pi) \sqrt{[\Omega^2 + c'_1/p'_1 (1 + 2c'_1\phi_{11})]} \quad . \quad . \quad . \quad . \qquad (9)$$

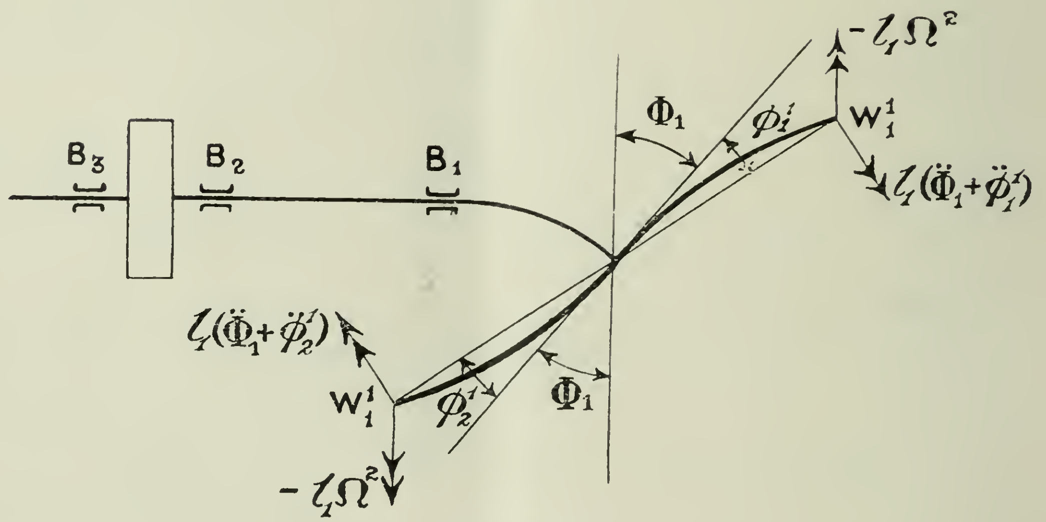


Fig. 8.

If there are three bearings, B_1, B_2, B_3, and $B_1B_2 = l_1$ and $B_2B_3 = l_2$,

$$\phi_{11} = l_1 (3l_1 + 4l_2)/12EI (l_1 + l_2) \quad . \quad . \quad . \qquad (10)$$

If there is no third bearing B_3

$$\phi_{11} = l_1/3EI \quad . \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

EI being the flexural rigidity of the shaft in the bending considered.

There will be resonance between the frequency (7) and the frequency of the firing impulses when

$$(1/2\pi) \sqrt{(\Omega^2 + c'_1/p'_1)} = qnN/120 \quad . \quad . \quad . \qquad (12)$$

or

$$N = (60/\pi) \sqrt{[c'_1/p'_1 (q^2n^2 - 4)]} \quad . \quad . \quad . \qquad (13)$$

since

$$\Omega = \pi N/30$$

N is in r.p.m., n the number of cylinders and q any whole number.

Similarly, there will be resonance for the frequency (9) when

$$N = (60/\pi) \sqrt{[c'_1/p'_1 (1 + 2c'_1\phi_{11}) (q^2n^2 - 4)]} \quad . \quad . \qquad (14)$$

The torsional oscillations have already been dealt with in §8.

Suppose next that the gear wheel is of appreciable inertia, and that B_2 is a short bearing so that the bending of the shaft to the left of B_2 affects the bending

of the shaft to the right. P_2 will be regarded as so close to the bearing B_2 that only angular deflection there is appreciable. If Φ_2 is the angular displacement of the gear wheel in the plane of bending there will be an inertia couple at P_2

$$-(p''_2 D^2 + p'_2 \Omega^2)\,\Phi_2 \qquad (15)$$

where $p''_2 = I''_2/g$, I''_2 being the moment of inertia of P_2 about a line through its mass centre perpendicular to the plane of bending and $p'_2 = (I_2 - I_3)/g$ the difference divided by g between its polar moment of inertia and the moment of inertia about the line through its mass centre and in the plane of bending.

If gp_2 is the polar moment of inertia of the gear wheel

$$p'_2 = \tfrac{1}{2}p_2 \text{ approximately.}$$
$$p''_2 = \tfrac{1}{2}p_2 \text{ approximately.}$$

For the equations of motion we have

$$\Phi_1 = c'_1(\phi'_1 + \phi'_2)\,\phi_{11} - \tfrac{1}{2}p_2(D^2 + \Omega^2)\,\phi_{12}\Phi_2 \qquad (16)$$

$$\Phi_2 = c'_1(\phi'_1 + \phi'_2)\,\phi_{12} - \tfrac{1}{2}p_2(D^2 + \Omega^2)\,\phi_{22}\Phi_2 \qquad (17)$$

where ϕ_{11} = slope at P_1 due to unit couple at P_1.
ϕ_{22} = slope at P_2 due to unit couple at P_2.
ϕ_{12} = slope at P_2 due to unit couple at P_1.
= slope at P_1 due to unit couple at P_2.

From (16) and (17) we have

$$\Phi_2\phi_{11} - \Phi_1\phi_{12} = -\tfrac{1}{2}p_2(D^2 + \Omega^2)\,\Delta_{12}\Phi_2 \qquad (18)$$

or

$$[\tfrac{1}{2}p_2(D^2 + \Omega^2)\,\Delta_{12} + \phi_{11}]\,\Phi_2 - \phi_{12}\Phi_1 = 0 \qquad (19)$$

where

$$\Delta_{12} = \phi_{11}\phi_{12} - \phi_{12}^2$$

For the blades of the airscrew we have

$$[(D^2 + \Omega^2)/c'_1 + 1/p'_1]\,[c'_1\phi'_1 \text{ or } c'_1\phi'_2] = -(D^2 + \Omega^2)\,\Phi_1 \qquad (20)$$

Using the equation (20), (17) becomes

$$[(D^2 + \Omega^2)/c'_1 + 1/p'_1]\,[\tfrac{1}{2}p_2(D^2 + \Omega^2)\,\phi_{22} + 1]\,\Phi_2$$
$$+\, 2(D^2 + \Omega^2)\,\phi_{12}\Phi_1 = 0 \qquad (21)$$

From (19) and (21) we obtain

$$\{\,[(D^2 + \Omega^2)/c'_1 + 1/p'_1]\,[\tfrac{1}{2}p_2(D^2 + \Omega^2)\,\phi_{22} + 1]$$
$$+\, 2(D^2 + \Omega^2)\,[\tfrac{1}{2}p_2(D^2 + \Omega^2)\,\Delta_{12} + \phi_{11}]\,\}\,[\Phi_1 \text{ or } \Phi_2] = 0 \qquad (22)$$

Thus there will be two harmonics of frequencies $k_1/2\pi$, $k_2/2\pi$ where k_1^2, k_2^2 are roots of the equation in k^2.

$$r(k^2 - \Omega^2)^2 - s(k^2 - \Omega^2) + t = 0 \qquad (23)$$

where $r = p'_1 p_2(\phi_{22} + 2c'_1\Delta_{12})$
$s = 2p'_1 + c'_1(4p'_1\phi_{11} + p_2\phi_{22})$
and $t = 2c'_1$

For resonance we have

$$k^2 - \Omega^2 = \pi^2 N^2 (q^2 n^2 - 4)/3600 \qquad (24)$$

where N is r.p.m., n the number of cylinders and q any whole number.

Equation (23) in k^2 can thus be converted into one for N^2 to obtain the resonance speeds.

In addition, there will be the fore and aft frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_1/p'_1)} \qquad (25)$$

for each blade as previously found. The torsional motion will also be the same as previously considered.

PART II.

Investigation of the Principal Types of Engine

1. Fig. 9 is a diagrammatic outline of the main rotating parts of a single-throw crank radial engine with the airscrew coupled direct to the crankshaft. P_1 is the airscrew; W_2 the estimated concentrated load at the crankpin; W_3, W_4 balance loads; B_1, B_2, B_3 bearings.

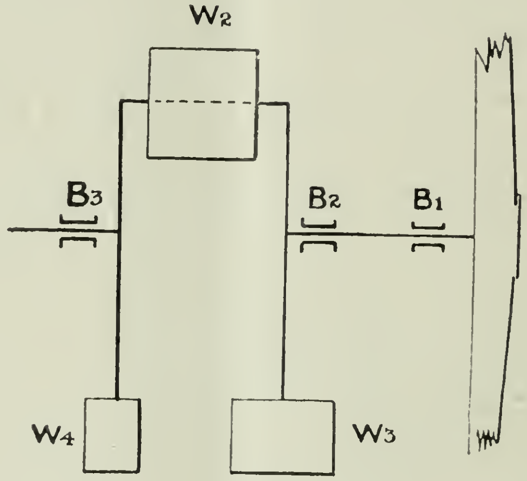


FIG. 9.

As a first approximation P_1 and the cranks are regarded as rigid.

We have already found (Part I., §2) that such a system has a frequency

$$(1/2\pi)\sqrt{[c_{12}(1/p_1 + 1/p_2)]} \quad . \quad . \quad . \quad . \quad (1)$$

where $p_1 = I_1/g$ and $p_2 = I_2/g$; I_1 being the moment of inertia of the airscrew about its axis of revolution and I_2 the moment of inertia of the crank and its loads (the load at the crankpin being regarded as concentrated) about the axis of the crankshaft; and c_{12} is the torsional stiffness of the shaft between P_1 and P_2.

For torsional resonance we have

$$(1/2\pi)\sqrt{[c_{12}(1/p_1 + 1/p_2)]} = qnN/120 \quad . \quad . \quad . \quad (2)$$

As a next approximation the blades of the airscrew are regarded as flexible, but the crank is still regarded as rigid.

The motion of such a system has already been investigated in Part I., §8.

For a two-bladed airscrew there are two frequencies, viz.:

$$(1/2\pi)\sqrt{(c_1/p_1)},\ (1/2\pi)\sqrt{[(1/2p_1 + 1/p_2)/(1/2c_1 + 1/c_{12})]} \quad . \quad (3)$$

the former referring to the blades only, where p_1 is $W_1 l_1^2/g$, W_1 being the equivalent tip load for each blade; c_1 is the equivalent flexural coefficient for each blade and p_2 and c_{12} are the same as for (2) above.

In addition each blade has a frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_1/p'_1)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

for fore and aft oscillations.

2. We next take into consideration the flexibility of the crank webs as well as the flexibility of the blades of the airscrew.

The crank with pin is taken as one flexible arm and the balance cranks are taken as another separate flexible arm. We have then a system as shown diagrammatically in Fig. 10.

The system P_2 is replaced by two loads, W_2, W_3, at the ends of collinear flexible arms of lengths l_2, l_3.

Suppose in the first case the airscrew is two-bladed.

In order to obtain the frequencies of such a system we will neglect the applied torques and friction at the bearings as these only affect the forced vibrations.

Let θ_1 be the angular displacement of the normal line of the blades of the airscrew and θ_2 that of the normal line of the arms l_2, l_3.

For the blades of the airscrew we have, with the general notation

$$(D^2/c_1 + 1/p_1)\,[c_1\phi_1 \text{ or } c_1\phi_2] = -D^2\theta_1 \quad . \quad . \quad . \quad . \quad (1)$$

From which we have at one of the frequencies of the blades

$$(1/2\pi)\sqrt{(c_1/p_1)} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

For the pair of blades we have by moments

$$c_1\,(\phi_1 + \phi_2) + c_{12}\theta_{12} = 0 \quad . \quad . \quad . \quad . \quad (3)$$

For the arms l_2; l_3, we have

$$(D^2/c_2 + 1/p_2)\,c_2\phi_5 = -D^2\theta_2 \quad . \quad . \quad . \quad . \quad (4)$$

$$(D^2/c_3 + 1/p_3)\,c_3\phi_6 = -D^2\theta_2 \quad . \quad . \quad . \quad . \quad (5)$$

and by moments

$$c_2\phi_5 + c_3\phi_6 - c_{12}\theta_{12} = 0 \quad . \quad . \quad . \quad . \quad (6)$$

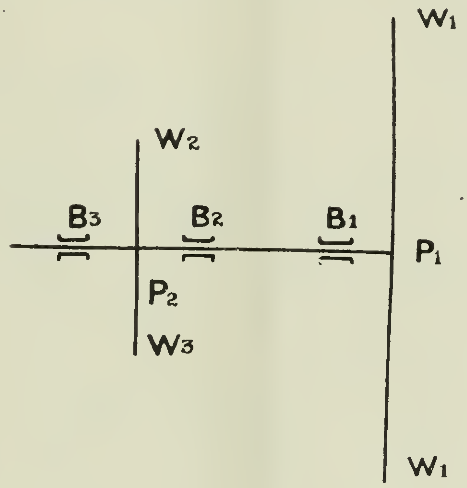


Fig. 10.

From (1) and (3) we obtain

$$2D^2\theta_1 - c_{12}\,(D^2/c_1 + 1/p_1)\,\theta_{12} = 0 \quad . \quad . \quad . \quad (7)$$

and from (4), (5) and (6)

$$D^2\,[(1/c_2 + 1/c_3)\,D^2 + 1/p_2 + 1/p_3]\,\theta_2$$
$$+ c_{12}\,(D^2/c_2 + 1/p_2)\,(D^2/c_3 + 1/p_3)\,\theta_{12} = 0 \quad . \quad . \quad (8)$$

By (7) and (8) we find two frequencies $k_1/2\pi$, $k_2/2\pi$, where k_1^2, k_2^2, are roots of the equation in k^2.

$$rk^4 - sk^2 + t = 0 \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where

$$r = (1/2c_1 + 1/c_{12})\,(1/c_2 + 1/c_3) + 1/c_2c_3$$

$$s = (1/2c_1 + 1/c_{12})\,(1/p_2 + 1/p_3) + (1/c_2)\,(1/2p_1 + 1/p_3) + (1/c_3)\,(1/2p_1 + 1/p_2)$$

and

$$t = (1/2p_1)\,(1/p_2 + 1/p_3) + 1/p_2p_3$$

In addition, each blade will have a frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_1/p'_1)} \qquad . \quad . \quad . \quad . \quad . \qquad (10)$$

for fore and aft oscillations.

For a four-bladed airscrew we find

(1) The blade frequency

$$(1/2\pi)\sqrt{(c_1/p_1)} \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

(2) Two frequencies $k_1/2\pi$, $k_2/2\pi$, where k_1^2, k_2^2, are roots of the equation in k^2.

$$rk^4 - sk^2 + t = 0 \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (12)$$

where

$$r = (1/4c_1 + 1/c_{12})(1/c_2 + 1/c_3) + 1/c_2c_3$$

$$s = (1/4c_1 + 1/c_{12})(1/p_2 + 1/p_3) + (1/c_2)(1/4p_1 + 1/p_3) + (1/c_3)(1/4p_1 + 1/p_2)$$

and

$$t = (1/4p_1)(1/p_2 + 1/p_3) + 1/p_2p_3$$

In addition, each blade will have a frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_1/p'_1)} \qquad . \quad . \quad . \quad . \quad . \qquad (13)$$

for fore and aft oscillations.

Similarly, the arms l_2, l_3, will have frequencies

$$(1/2\pi)\sqrt{(\Omega^2 + c'_2/p'_2)},\ (1/2\pi)\sqrt{(\Omega^2 + c'_3/p'_3)} \qquad . \quad . \qquad (14)$$

for fore and aft oscillations.

3. As a final refinement we regard each balance crank as an independent arm, as in Fig. 9.

The system P_2 is now replaced by three flexible arms l_2, l_3, l_4, each carrying a load at its extremity. For the airscrew blades (supposed two)

$$(D^2/c_1 + 1/p_1)\,[c_1\phi_1 \text{ or } c_1\phi_2] = -D^2\theta_1 \qquad . \quad . \quad . \quad . \qquad (1)$$

and for the pair by moments

$$c_1(\phi_1 + \phi_2) + c_{12}\theta_{12} = 0 \qquad . \quad . \quad . \quad . \quad . \qquad (2)$$

For the arms l_2, l_3, l_4, we have

$$(D^2/c_2 + 1/p_2)\,c_2\phi_5 = -D^2\theta_2 \qquad . \quad . \quad . \quad . \quad . \qquad (3)$$

$$(D^2/c_3 + 1/p_3)\,c_3\phi_6 = -D^2\theta_2 \qquad . \quad . \quad . \quad . \quad . \qquad (4)$$

$$(D^2/c_4 + 1/p_4)\,c_4\phi_7 = -D^2\theta_2 \qquad . \quad . \quad . \quad . \quad . \qquad (5)$$

and by moments

$$c_2\phi_5 + c_3\phi_6 + c_4\phi_7 - c_{12}\theta_{12} = 0 \qquad . \quad . \quad . \quad . \quad . \qquad (6)$$

From (1) we obtain the blade frequency

$$(1/2\pi)\sqrt{(c_1/p_1)} \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

From (1) and (2) we have

$$-2D^2\theta_1 + c_{12}(D^2/c_1 + 1/p_1)\,\theta_{12} = 0 \qquad . \quad . \quad . \quad . \qquad (8)$$

and from (3), (4), (5) and (6) we have

$$\begin{aligned} D^2\,[(D^2/c_2 + 1/p_2)(D^2/c_3 + 1/p_3) + (D^2/c_2 + 1/p_2)(D^2/c_4 + 1/p_4) \\ + (D^2/c_3 + 1/p_3)(D^2/c_4 + 1/p_4)]\,\theta_2 \\ + c_{12}(D^2/c_2 + 1/p_2)(D^2/c_3 + 1/p_3)(D^2/c_4 + 1/p_4)\,\theta_{12} = 0 \end{aligned} \qquad . \qquad (9)$$

(8) can be written

$$-D^2\theta_2 + c_{12}\,[(1/2c_1 + 1/c_{12})\,D^2 + 1/2p_1]\,\theta_{12} = 0 \qquad . \quad . \qquad (10)$$

From (9) and (10) we obtain three frequencies, $k_1/2\pi$, $k_2/2\pi$, $k_3/2\pi$, where k_1^2, k_2^2, k_3^2, are roots of the equation in k^2.

$$rk^6 - sk^4 + tk^2 - u = 0 \qquad . \quad . \quad . \quad . \quad . \qquad (11)$$

where

$$r = (1/2c_1 + 1/c_{12})(1/c_2c_3 + 1/c_2c_4 + 1/c_3c_4) + 1/c_2c_3c_4$$

$$\begin{aligned} s = {} & (1/2c_1 + 1/c_{12})[(1/c_2)(1/p_3 + 1/p_4) + (1/c_3)(1/p_2 + 1/p_4) \\ & + (1/c_4)(1/p_2 + 1/p_3)] + (1/c_2c_3)(1/2p_1 + 1/p_4) \\ & + (1/c_2c_4)(1/2p_1 + 1/p_3) + (1/c_3c_4)(1/2p_1 + 1/p_2) \end{aligned}$$

$$\begin{aligned} t = {} & (1/2c_1 + 1/c_{12})(1/p_2p_3 + 1/p_2p_4 + 1/p_3p_4) \\ & + (1/c_2)[(1/2p_1)(1/p_3 + 1/p_4) + 1/p_3p_4] \\ & + (1/c_3)[(1/2p_1)(1/p_2 + 1/p_4) + 1/p_2p_4] \\ & + (1/c_4)[(1/2p_1)(1/p_2 + 1/p_3) + 1/p_2p_3] \end{aligned}$$

$$u = (1/2p_1)(1/p_2p_3 + 1/p_2p_4 + 1/p_3p_4) + 1/p_2p_3p_4$$

In addition there will be for each blade a frequency

$$(1/2\pi)\surd(\Omega^2 + c'_1/p'_1)$$

for fore and aft oscillations and similarly frequencies

$$(1/2\pi)\surd(\Omega^2 + c'_2/p'_2),\ (1/2\pi)\surd(\Omega^2 + c'_3/p'_3),\ (1/2\pi)\surd(\Omega^2 + c'_4/p'_4),$$

for the fore and aft oscillations of the arms l_2, l_3, l_4, respectively.

In the case of a four-bladed airscrew we write $2c_1$ for c_1 and $2p_1$ for p_1 in (11).

4. Consider next a two-throw crankshaft engine as depicted diagrammatically in Fig. 11.

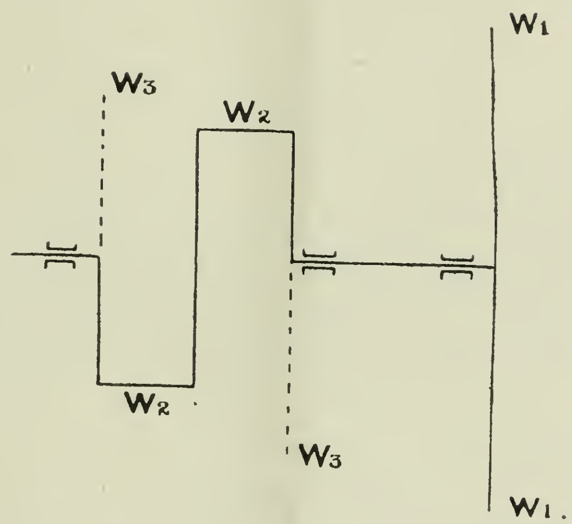


FIG. 11.

We assume that the throws are equal in all respects, that the concentrated loads at the crankpins are equal and that in the first place there are no balance loads.

We have for the blades of the airscrew

$$(D^2/c_1 + 1/p_1)\,[c_1\phi_1 \text{ or } c_1\phi_2] = -D^2\theta_1 \quad . \quad . \quad . \quad (1)$$

and by moments for the two blades

$$c_1(\phi_1 + \phi_2) + c_{12}\theta_{12} = 0 \quad . \quad . \quad . \quad . \quad (2)$$

Similarly for the two cranks

$$(D^2/c_2 + 1/p_2)\,[c_2\phi_5 \text{ or } c_2\phi_6] = -D^2\theta_2 \quad . \quad . \quad . \quad (3)$$

and by moments for the two cranks

$$c_2(\phi_5 + \phi_6) - c_{12}\theta_{12} = 0 \quad . \quad . \quad . \quad . \quad (4)$$

From (1) we have as one of the frequencies of the blades

$$(1/2\pi)\surd(c_1/p_1) \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Similarly from (3) we have as one of the frequencies of the cranks

$$(1/2\pi)\sqrt{(c_2/p_2)} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

From (1) and (2) we obtain

$$-2D^2\theta_1 + c_{12}(D^2/c_1 + 1/p_1)\,\theta_{12} = 0 \quad . \quad . \quad . \quad (7)$$

and from (3) and (4) we obtain

$$-2D^2\theta_2 - c_{12}(D^2/c_2 + 1/p_2)\,\theta_{12} = 0 \quad . \quad . \quad . \quad (8)$$

From (7) and (8) we find

$$[(1/2c_1 + 1/2c_2 + 1/c_{12})\,D^2 + 1/2p_1 + 1/2p_2]\,\theta_{12} = 0 \quad . \quad . \quad (9)$$

and thus we obtain the frequency

$$(1/2\pi)\sqrt{[(1/2p_1 + 1/2p_2)/(1/2c_1 + 1/2c_2 + 1/c_{12})]} \quad . \quad . \quad (10)$$

In addition the blades of the airscrew will have a frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_1/p'_1)} \quad . \quad . \quad . \quad . \quad (11)$$

and the cranks a frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_2/p'_2)} \quad . \quad . \quad . \quad . \quad (12)$$

for fore and aft oscillations.

If the airscrew is four-bladed we write $2c_1$ for c_1 and $2p_1$ for p_1 in (10).

5. If now there are two equal balance arms W_3, as shown dotted in Fig. 11, we have:

For the airscrew blades

$$(D^2/c_1 + 1/p_1)\;[c_1\phi_1 \text{ or } c_1\phi_2] = -D^2\theta_1 \quad . \quad . \quad . \quad (1)$$

and by moments

$$c_1(\phi_1 + \phi_2) + c_{12}\theta_{12} = 0 \quad . \quad . \quad . \quad . \quad (2)$$

For the two cranks

$$(D^2/c_2 + 1/p_2)\,[c_2\phi_5 \text{ or } c_2\phi_6] = -D^2\theta_2 \quad . \quad . \quad . \quad (3)$$

For the balance cranks

$$(D^2/c_3 + 1/p_3)\,[c_3\phi_7 \text{ or } c_3\phi_8] = -D^2\theta_2 \quad . \quad . \quad . \quad (4)$$

By moments for the cranks and balance cranks

$$c_2(\phi_5 + \phi_6) + c_3(\phi_7 + \phi_8) - c_{12}\theta_{12} = 0 \quad . \quad . \quad . \quad (5)$$

From (1) we have as one of the frequencies of the blades

$$(1/2\pi)\sqrt{(c_1/p_1)} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

From (3) one of the frequencies of the cranks is

$$(1/2\pi)\sqrt{(c_2/p_2)} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and from (4) one of the frequencies of the balance cranks is

$$(1/2\pi)\sqrt{(c_3/p_3)} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

From (1) and (2) we have

$$-2D^2\theta_1 + c_{12}(D^2/c_1 + 1/p_1)\,\theta_{12} = 0 \quad . \quad . \quad . \quad (9)$$

and from (3), (4) and (5)

$$2D^2[(1/c_2 + 1/c_3)\,D^2 + 1/p_2 + 1/p_3]\,\theta_2$$
$$+\, c_{12}(D^2/c_2 + 1/p_2)(D^2/c_3 + 1/p_3)\,\theta_{12} = 0 \quad . \quad . \quad (10)$$

Thus from (9) and (10) we obtain two frequencies $k_1/2\pi$, $k_2/2\pi$, where k_1^2, k_2^2, are the roots of the equation in k^2.

$$rk^4 - sk^2 + t = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

where

$$r = 2(1/2c_1 + 1/c_{12})(1/c_2 + 1/c_3) + 1/c_2c_3$$
$$s = 2(1/2c_1 + 1/c_{12})(1/p_2 + 1/p_3) + (1/c_2 + 1/c_3)(1/p_1) + 1/p_2c_3 + 1/p_3c_2$$
$$t = (1/p_1)(1/p_2 + 1/p_3) + 1/p_2p_3$$

In addition each blade will have the frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_1/p'_1)} \quad . \quad . \quad . \quad . \quad (12)$$

each crank the frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_2/p'_2)} \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and each balance crank the frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_3/p'_3)} \quad . \quad . \quad . \quad . \quad . \quad (14)$$

for fore and aft oscillations.

In the case of a four-bladed airscrew we write $2c_1$ for c_1 and $2p_1$ for p_1 in (11).

6. Take next a four-throw crankshaft as shown in Fig. 12.

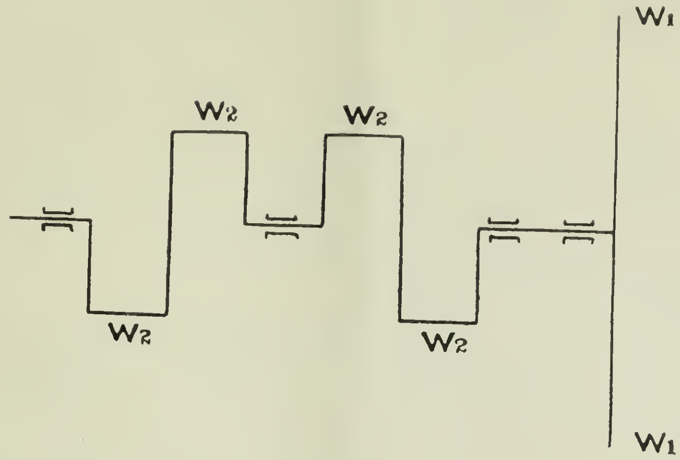


Fig. 12.

We assume that the throws are equal in all respects, that the concentrated loads at the pins are all equal and that there are no balance loads.

Let θ_1 be the angular displacement of the normal line of the airscrew blades, θ_2 the angular displacement of the normal line of the pair of cranks adjacent to the airscrew and θ_3 that for the remaining pair of cranks.

For the airscrew blades we have

$$(D^2/c_1 + 1/p_1)\,[c_1\phi_1 \text{ or } c_1\phi_2] = -D^2\theta_1 \quad . \quad . \quad . \quad (1)$$

and by moments

$$c_1(\phi_1 + \phi_2) + c_{12}\theta_{12} = 0 \quad . \quad . \quad . \quad . \quad (2)$$

For the pair of cranks adjacent to the airscrew we have

$$(D^2/c_2 + 1/p_2)\,[c_2\phi_5 \text{ or } c_2\phi_6] = -D^2\theta_2 \quad . \quad . \quad . \quad (3)$$

and by moments

$$c_2(\phi_5 + \phi_6) + c_{22}\theta_{23} - c_{12}\theta_{12} = 0 \quad . \quad . \quad . \quad . \quad (4)$$

where c_{22} refers to the journal between the two pairs of cranks and

$$\theta_{23} = \theta_3 - \theta_2$$

Similarly for the other pair of cranks

$$(D^2/c_2 + 1/p_2)\,[c_2\phi_7 \text{ or } c_2\phi_8] = -D^2\theta_2 \quad . \quad . \quad . \quad (5)$$

and by moments

$$c_2(\phi_7 + \phi_8) - c_{22}\theta_{23} = 0 \quad . \quad . \quad . \quad . \quad . \quad (6)$$

From (1) we have as one of the frequencies of the blades

$$(1/2\pi)\sqrt{(c_1/p_1)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and from (3) and (5) one of the frequencies of the cranks

$$(1/2\pi)\sqrt{(c_2/p_2)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

From (1) and (2) we obtain

$$-2D^2\theta_1 + (D^2/c_1 + 1/p_1)\,c_{12}\theta_{12} = 0 \quad . \quad . \quad . \quad . \quad (9)$$

From (3) and (4)

$$-2D^2\theta_2 + (D^2/c_2 + 1/p_2)(c_{22}\theta_{23} - c_{12}\theta_{12}) = 0 \quad . \quad . \qquad (10)$$

and from (5) and (6)

$$-2D^2\theta_3 - (D^2/c_2 + 1/p_2)\,c_{22}\theta_{23} = 0 \quad . \quad . \quad . \qquad (11)$$

From (9) and (10) we have

$$[(2/c_{12} + 1/c_1 + 1/c_2)\,D^2 + 1/p_1 + 1/p_2]\,c_{12}\theta_{12} - (D^2/c_2 + 1/p_2)\,c_{22}\theta_{23} = 0 \qquad (12)$$

and from (10) and (11)

$$-(D^2/c_2 + 1/p_2)\,c_{12}\theta_{12} + 2\,[(1/c_{22} + 1/c_2)\,D^2 + 1/p_2]\,c_{22}\theta_{23} = 0 \quad . \qquad (13)$$

Thus we obtain two frequencies $k_1/2\pi$, $k_2/2\pi$, where k_1^2, k_2^2, are the roots of the equation in k^2.

$$rk^4 - sk^2 + t = 0 \quad . \quad . \quad . \quad . \quad . \qquad (14)$$

where

$$r = 4\,(1/2c_1 + 1/c_{12})\,(1/c_{22} + 1/c_2) + 2/c_{22}c_2 + 1/c_2^2$$

$$s = 4\,(1/2c_1 + 1/c_{12})\,(1/p_2) + 2\,(1/c_{22} + 1/c_2)\,(1/p_1 + 1/p_2)$$

$$t = (2/p_2)\,(1/p_1 + 1/2p_2)$$

In addition there are the frequencies

$$(1/2\pi)\sqrt{(\Omega^2 + c'_1/p'_1)},\quad (1/2\pi)\sqrt{(\Omega^2 + c'_2/p'_2)} \quad . \quad . \qquad (15)$$

for the blades and cranks respectively for fore and aft oscillations.

For four-bladed airscrews we write $2c_1$ for c_1 and $2p_1$ for p_1 in (14).

7. Consider next a four-throw crankshaft, there being three equal intermediate journals. All the cranks are taken as equal in all respects. The engine is geared and the gear wheel driving the airscrew gear is supposed of small mass and inertia. Fig. 13 is a diagrammatic representation of such a system.

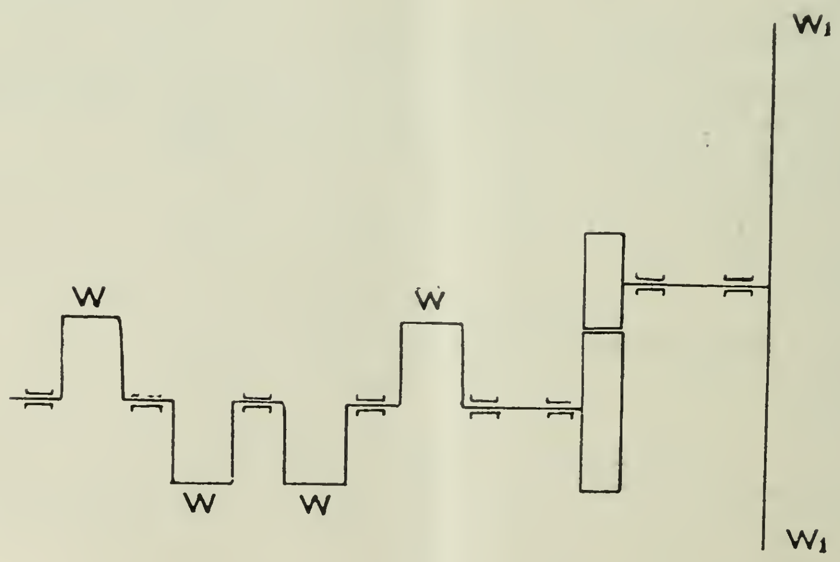


Fig. 13.

In this case the system is split up into two parts. There is the system comprising the airscrew, the airscrew shaft and the gear wheel driving the latter. The other system is the crankshaft and its gear wheel.

We will first ignore the airscrew system.

Let θ_1, θ_2, θ_3, θ_4, be the angular displacements of the normal lines of the cranks (numbering from the airscrew end).

Let c_2 and p_2 refer to each crank and c_{22} to each journal.

We have for the cranks

$$(D^2/c_2 + 1/p_2)\, c_2\phi_r = -D^2\theta_r \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where $r = 1, 2, 3$ or 4, or

$$\delta_2 c_2 \phi_r = -D^2\theta_r \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where

$$\delta_2 = D^2/c_2 + 1/p_2$$

Also by moments

$$c_2\phi_1 + c_{22}\theta_{12} = 0 \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$c_2\phi_2 + c_{22}(\theta_{23} - \theta_{12}) = 0 \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$c_2\phi_3 + c_{22}(\theta_{31} - \theta_{23}) = 0 \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and

$$c_2\phi_4 - c_{22}\theta_{34} = 0 \quad . \quad . \quad . \quad . \quad . \quad (6)$$

From (1) we find

$$\delta_2 c_2 (\phi_2 - \phi_1) = -D^2\theta_{12} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

or

$$\delta_2 c_2 \phi_{12} = -D^2\theta_{12} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where

$$\phi_{12} = \phi_2 - \phi_1$$

Similarly

$$\delta_2 c_2 \phi_{23} = -D^2\theta_{23} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and

$$\delta_2 c_2 \phi_{34} = -D^2\theta_{34} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

From (3), (4), (5) and (6) we find

$$\delta_2 c_2 \phi_{12} + \delta_2 c_{22}(\theta_{23} - 2\theta_{12}) = 0 \quad . \quad . \quad . \quad . \quad . \quad (11)$$

$$\delta_2 c_2 \phi_{23} + \delta_2 c_{22}(\theta_{12} + \theta_{34} - 2\theta_{23}) = 0 \quad . \quad . \quad . \quad (12)$$

and

$$\delta_2 c_2 \phi_{34} + \delta_2 c_{22}(\theta_{23} - 2\theta_{34}) = 0 \quad . \quad . \quad . \quad . \quad . \quad (13)$$

By pairing (8), (9) and (10) with (11), (12) and (13) respectively we obtain

$$\delta_3\theta_{12} - \delta_2\theta_{23} = 0 \quad . \quad . \quad . \quad . \quad . \quad (14)$$

$$-\delta_2\theta_{12} + \delta_3\theta_{23} - \delta_2\theta_{34} = 0 \quad . \quad . \quad . \quad . \quad . \quad (15)$$

and

$$-\delta_2\theta_{23} + \delta_3\theta_{34} = 0 \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where

$$\delta_3 = D^2/c_{22} + 2\delta_2$$

From (14), (15) and (16) we find there are three frequencies, $k_1/2\pi$, $k_2/2\pi$, $k_3/2\pi$, where k_1^2, k_2^2, k_3^2, are the roots of the equation in k^2.

$$\Delta_4 = K^6 - 2K^2 = 0 \quad . \quad . \quad . \quad . \quad . \quad (17)$$

where

$$K^2 = [(1/c_{22} + 2/c_2)\,k^2 - 2/p_2]/(k^2/c_2 - 1/p_2)$$

Thus we obtain the frequencies

$$(1/2\pi)\sqrt{[(2-\sqrt{2})/p_2\,\{1/c_{22} + (2-\sqrt{2})/c_2\}]}$$

or

$$(1/2\pi)\sqrt{[(.586)/p_2\,(1/c_{22} + .586/c_2)]} \quad . \quad . \quad . \quad (18)$$

$$(1/2\pi)\sqrt{[2/p_2\,(1/c_{22} + 2/c_2)]} \quad . \quad . \quad . \quad . \quad (19)$$

and

$$(1/2\pi)\sqrt{[(2+\sqrt{2})/p_2\,\{1/c_{22} + (2+\sqrt{2})/c_2\}]}$$

or

$$(1/2\pi)\sqrt{[(3.414)/p_2\,(1/c_{22} + 3.414/c_2)]} \quad . \quad . \quad . \quad (20)$$

In addition the cranks have the frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_2/p'_2)}$$

for fore and aft oscillations.

8. Take next the case of a six-throw crankshaft under the same conditions as in the preceding example.

With the same notation we have for the six cranks

$$\delta_2 c_2 \phi_r = -D^2\theta_r \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where $r = 1, 2, 3, 4, 5$ or 6, and by moments

$$c_2\phi_1 + c_{22}\theta_{12} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$
$$c_2\phi_2 + c_{22}(\theta_{23} - \theta_{12}) = 0 \quad . \quad . \quad . \quad . \quad . \quad (3)$$
$$c_2\phi_3 + c_{22}(\theta_{34} - \theta_{23}) = 0 \quad . \quad . \quad . \quad . \quad . \quad (4)$$
$$c_2\phi_4 + c_{22}(\theta_{45} - \theta_{34}) = 0 \quad . \quad . \quad . \quad . \quad . \quad (5)$$
$$c_2\phi_5 + c_{22}(\theta_{56} - \theta_{45}) = 0 \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and

$$c_2\phi_6 - c_{22}\theta_{56} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

The corresponding equation in K^2 is found to be

$$\Delta_6 = K^{10} - 4K^6 + 3K^2 = 0 \quad . \quad . \quad . \quad . \quad . \quad (8)$$

From which we obtain the five frequencies

$$(1/2\pi)\sqrt{[(2-\sqrt{3})/p_2\{1/c_{22} + (2-\sqrt{3})/c_2\}]}$$

or

$$(1/2\pi)\sqrt{[.268/p_2(1/c_{22} + .268/c_2)]} \quad . \quad . \quad . \quad (9)$$
$$(1/2\pi)\sqrt{[1/p_2(1/c_{22} + 1/c_2)]} \quad . \quad . \quad . \quad . \quad (10)$$
$$(1/2\pi)\sqrt{[2/p_2(1/c_{22} + 2/c_2)]} \quad . \quad . \quad . \quad . \quad (11)$$
$$(1/2\pi)\sqrt{[3/p_2(1/c_{22} + 3/c_2)]} \quad . \quad . \quad . \quad . \quad (12)$$

and

$$(1/2\pi)\sqrt{[(2+\sqrt{3})/p_2\{1/c_{22} + (2+\sqrt{3})/c_2\}]}$$

or

$$(1/2\pi)\sqrt{[3.732/p_2(1/c_{22} + 3.732/c_2)]} \quad . \quad . \quad . \quad (13)$$

In addition the cranks have the frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_2/p'_2)}$$

for fore and aft oscillations.

9. For an eight-throw crankshaft under the same conditions we find the corresponding equation in K^2 is

$$\Delta_8 = K^{14} - 6K^{10} + 10K^6 - 4K^2 = 0 \quad . \quad . \quad . \quad (1)$$

From which we obtain the seven frequencies

$$(1/2\pi)\sqrt{[\{2-\sqrt{(2+\sqrt{2})}\}/p_2\{1/c_{22} + [2-\sqrt{(2+\sqrt{2})}]/c_2\}]}$$

or

$$(1/2\pi)\sqrt{[.152/p_2(1/c_{22} + .152/c_2)]} \quad . \quad . \quad . \quad (2)$$
$$(1/2\pi)\sqrt{[(2-\sqrt{2})/p_2\{1/c_{22} + (2-\sqrt{2})/c_2\}]}$$

or

$$(1/2\pi)\sqrt{[.586/p_2(1/c_{22} + .586/c_2)]} \quad . \quad . \quad . \quad (3)$$
$$(1/2\pi)\sqrt{[\{2-\sqrt{(2-\sqrt{2})}\}/p_2\{1/c_{22} + [2-\sqrt{(2-\sqrt{2})}]/c_2\}]}$$

or

$$(1/2\pi)\sqrt{[1.234/p_2(1/c_{22} + 1.235/c_2)]} \quad . \quad . \quad . \quad (4)$$
$$(1/2\pi)\sqrt{\{2/p_2(1/c_{22} + 2/c_2)\}} \quad . \quad . \quad . \quad . \quad (5)$$
$$(1/2\pi)\sqrt{[\{2+\sqrt{(2-\sqrt{2})}\}/p_2\{1/c_{22} + [2+\sqrt{(2-\sqrt{2})}]/c_2\}]}$$

or

$$(1/2\pi)\sqrt{[2.766/p_2(1/c_{22} + 2.765/c_2)]} \quad . \quad . \quad . \quad (6)$$
$$(1/2\pi)\sqrt{[(2+\sqrt{2})/p_2\{1/c_{22} + (2+\sqrt{2})/c_2\}]}$$

or

$$(1/2\pi)\sqrt{[3.414/p_2(1/c_{22} + 3.414/c_2)]} \quad . \quad . \quad . \quad (7)$$
$$(1/2\pi)\sqrt{[\{2+\sqrt{(2+\sqrt{2})}\}/p_2\{1/c_{22} + [2+\sqrt{(2+\sqrt{2})}]/c_2\}]}$$

or

$$(1/2\pi)\sqrt{[3.848/p_2\,(1/c_{22} + 3.848/c_2)]} \quad . \quad . \quad . \quad (8)$$

In addition the cranks have the frequency

$$(1/2\pi)\sqrt{(\Omega^2 + c'_2/p'_2)}$$

for fore and aft oscillations.

10. We find generally

$$\Delta_n = K^2\Delta_{n-1} - \Delta_{n-2} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Thus, Δ_n is the coefficient of x^{n-1} in the expansion of

$$1/(1 - K^2x + x^2) \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in ascending power of x.

Actually

$$\Delta_n = K^{2n-2} - (n-2)\,K^{2n-6} + \tfrac{1}{2}(n-3)(n-4)\,K^{2n-10}$$
$$- \{(n-4)(n-5)(n-6)/(1\,.\,2\,.\,3)\}\,K^{2n-14} + \ldots \quad . \quad . \quad (3)$$

The last term in the expression on the right hand side of (3) will be a multiple of K^2 when n is even and ± 1 when n is odd.

Let the roots in K^2 of

$$\Delta_n = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

be $\lambda_1, \lambda_2, \lambda_3, \lambda_4, \ldots \lambda_{n-1}$.

Then the corresponding k's are obtained from the relations

$$(1/c_{22} + 2/c_2)\,k^2 - 2/p_2 = \lambda_r\,(k^2/c_2 - 1/p_2) \quad . \quad . \quad (5)$$

or

$$k^2 = (2 - \lambda_r)/p_2\,[1/c_{22} + (2 - \lambda_r)/c_2] \quad . \quad . \quad . \quad (6)$$

From which we obtain the frequency

$$(1/2\pi)\sqrt{[(2-\lambda_r)/p_2\,\{1/c_{22} + (2-\lambda_r)/c_2\}]} \quad . \quad . \quad (7)$$

The values for $\sqrt{(2-\lambda_r)}$ have been calculated in the cases

$$\Delta_n = 0$$

when $n = 1, 2, 3, \ldots$ or 10, and are given in Table I.

It can be shown that

$$\Delta_{2n} = \Delta_n\,(\Delta_{n+1} - \Delta_{n-1}) \quad . \quad . \quad . \quad . \quad (8)$$

so that a system of $2n$ equal cranks will have all the frequencies of a system of n of the same cranks.

Further, if $\lambda_1, \lambda_2, \lambda_3, \ldots \lambda_{2n-1}$ are the roots in K^2 of the equation

$$\Delta_{2n} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

the values

$$\sqrt{(2-\lambda_1)},\ \sqrt{(2-\lambda_3)},\ \sqrt{(2-\lambda_5)},\ \ldots\ \sqrt{(2-\lambda_{2n-1})}$$

arise from

$$\Delta_{n+1} - \Delta_{n-1} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and

$$\sqrt{(2-\lambda_2)},\ \sqrt{(2-\lambda_4)},\ \sqrt{(2-\lambda_6)},\ \ldots\ \sqrt{(2-\lambda_{2n-2})}$$

arise from

$$\Delta_n = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Similarly it can be shown that

$$\Delta_{2n+1} = \Delta^2_{n+1} - \Delta^2_n \quad . \quad . \quad . \quad . \quad . \quad (12)$$

and that if $\lambda_1, \lambda_2, \lambda_3, \ldots \lambda_{2n}$ are the roots in K^2 of the equation

$$\Delta_{2n+1} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

the values

$$\sqrt{(2-\lambda_1)},\ \sqrt{(2-\lambda_3)},\ \sqrt{(2-\lambda_5)},\ \ldots\ \sqrt{(2-\lambda_{2n-1})}$$

arise from

$$\Delta_{n+1} - \Delta_n = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

TABLE I.

Values of $\sqrt{(2 - \lambda_r)}$ where $\lambda_1, \lambda_2, \lambda_3, \ldots \lambda_{n-1}$ are the roots in K^2 of the equation $\Delta_n = 0$.

n	2	3	4	5	6	7	8	9	10	APPROXN.
$\Delta_2 = K^2$	1.414	1.000	0.766	0.618	0.518	0.445	0.390	0.347	0.313	$3/n$
$\Delta_3 = K^4 - 1$	...	1.732	1.414	1.176	1.000	0.868	0.766	0.684	0.618	$7/(n+1)$
$\Delta_4 = K^6 - 2K^2$	...	...	1.848	1.618	1.414	1.247	1.111	1.000	0.908	$11/(n+2)$
$\Delta_5 = K^8 - 3K^4 + 1$	...	...	...	1.902	1.732	1.564	1.414	1.286	1.176	$15/(n+3)$
$\Delta_6 = K^{10} - 4K^6 + 3K^2$	...	...	...	...	1.932	1.802	1.663	1.532	1.414	$19/(n+4)$
$\Delta_7 = K^{12} - 5K^8 + 6K^4 - 1$	...	...	...	...	...	1.950	1.848	1.732	1.618	$23/(n+5)$
$\Delta_8 = K^{14} - 6K^{10} + 10K^6 - 4K^2$	...	...	...	...	...	...	1.962	1.880	1.782	$27/(n+6)$
$\Delta_9 = K^{16} - 7K^{12} + 15K^8 - 10K^4 + 1$	...	...	...	...	...	...	...	1.970	1.902	$31/(n+7)$
$\Delta_{10} = K^{18} - 8K^{14} + 21K^{10} - 20K^6 + 5K^2$	...	...	...	...	...	...	...	...	1.975	$35/(n+8)$

and

$$\sqrt{(2-\lambda_2)},\ \sqrt{(2-\lambda_4)},\ \ldots\ \sqrt{(2-\lambda_{2n})}$$

arise from

$$\Delta_{n+1} + \Delta_n = 0 \qquad \ldots \qquad (15)$$

11. From the values of $\sqrt{(2-\lambda_r)}$ in Table I. we observe that as the number of cranks increases

$$\sqrt{(2-\lambda_r)} \longrightarrow 0$$

$$\text{as } n \longrightarrow \infty$$

where λ_r is the highest positive root of the equation in K^2

$$\Delta_n = 0$$

Also

$$\sqrt{(2-\lambda_r)} \longrightarrow 2$$

$$\text{as } n \longrightarrow \infty$$

where λ_r is numerically the greatest negative root.

Thus the greatest frequency possible is

$$(1/2\pi)\sqrt{[4/p_2\,(1/c_{22} + 4/c_2)]}$$

the lowest being zero.

The values in Table I. have remarkable approximations.

For example, if n be the number of cranks, the lowest value of $\sqrt{(2-\lambda_r)}$ is very approximately

$$3/n$$

This approximation is within 4 per cent., except in the case of two cranks when it is within 6.5 per cent. of the true value. The rule will be near enough for practice allowing a margin of 5 per cent.

Thus very approximately the fundamental frequency of torsional oscillations in the case of n cranks is

$$1/2\pi\sqrt{[p_2\,(n^2/9c_{22} + 1/c_2)]}$$

Generally $1/c_2$ will be small compared with $1/c_{22}$, so that with throws exceeding four the flexibility of the cranks will not usually be important.

For example, in a geared engine with a six-throw crankshaft let the dimensions of the journals be

$$2'' \times 1\tfrac{3}{4}''\ (\text{dia.})$$

and of the crankwebs

$$3'' \times 2\tfrac{1}{2}'' \times 1''$$

Then

$$c_{22} = 5.52 \times 10^6$$

and remembering there are two webs to one crank

$$c_2 = 78.13 \times 10^6$$

Thus

$$n^2/9c_{22} + 1/c_2 = (4/5.52 + 1/78.13)\,10^{-6}$$

The flexibility of the cranks will decrease the fundamental frequency in the ratio

$$\sqrt{(4/5.52)}/\sqrt{(4/5.52 + 1/78.13)}$$

i.e., approximately 155/156, or a decrease of less than 1 per cent.

If the estimated crankpin load is 24/3 lbs.

$$p_2 = (24/3) \times 3^2 \times (1/384) = 1/4$$

and the fundamental frequency of torsional oscillations

$$1175/\pi$$

vibrations per second.

The other values of $\sqrt{(2-\lambda_r)}$ in Table I. also have close approximations, as shown in the last column of the table.

12. Consider next the case of a four-throw crankshaft with the airscrew driven direct. Fig. 14 is a diagrammatic representation of such a system.

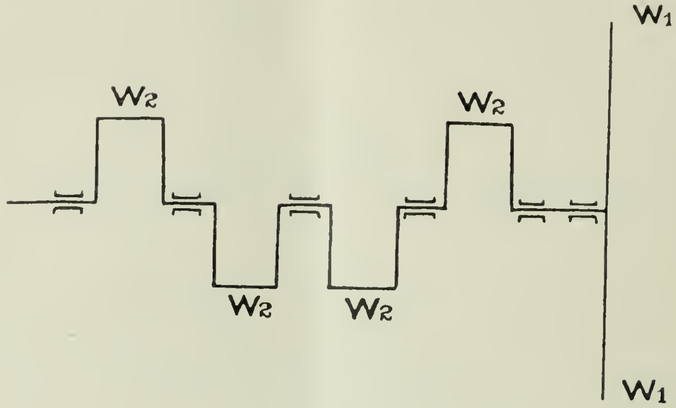


Fig. 14.

Let θ_1, θ_2, θ_3, θ_4, θ_5, be the angular displacements of the normal lines of the airscrew blades and cranks respectively (numbering from the airscrew end).

Let c_1, p_1, refer to the airscrew blades and c_2, p_2, to the cranks.

All the journals are supposed equal; c_{12} refers to the portion of the shaft between the airscrew and the nearest throw and c_{22} refers to any of the journals.

We have for the airscrew (supposed two-bladed)

$$(D^2/c_1 + 1/p_1)\,[c_1\phi_1 \text{ or } c_1\phi_2] = -D^2\theta_1 \quad (1)$$

From which we obtain

$$\delta_1 c_1 (\phi_1 + \phi_2) = -D^2\theta_1 \quad (2)$$

where

$$\delta_1 \equiv D^2/2c_1 + 1/2p_1$$

For the cranks

$$\delta_2 c_2 \phi_5 = -D^2\theta_2 \quad (3)$$

$$\delta_2 c_2 \phi_6 = -D^2\theta_3 \quad (4)$$

$$\delta_2 c_2 \phi_7 = -D^2\theta_4 \quad (5)$$

and

$$\delta_2 c_2 \phi_8 = -D^2\theta_5 \quad (6)$$

where

$$\delta_2 \equiv D^2/c_2 + 1/p_2$$

Also by moments

$$c_1 (\phi_1 + \phi_2) + c_{12}\theta_{12} = 0 \quad (7)$$

$$c_2\phi_5 + c_{22}\theta_{23} - c_{12}\theta_{12} = 0 \quad (8)$$

$$c_2\phi_6 + c_{22} (\theta_{34} - \theta_{23}) = 0 \quad (9)$$

$$c_2\phi_7 + c_{22} (\theta_{45} - \theta_{34}) = 0 \quad (10)$$

and

$$c_2\phi_8 - c_{22}\theta_{45} = 0 \quad (11)$$

By the elimination of the ϕ's we find

$$(D^2/c_{12} + \delta_1 + \delta_2)\, c_{12}\theta_{12} - \delta_2 c_{22}\theta_{23} = 0 \quad (12)$$

$$-\delta_2 c_{12}\theta_{12} + \delta_3 c_{22}\theta_{23} - \delta_2 c_{22}\theta_{34} = 0 \quad (13)$$

$$-\delta_2 c_{22}\theta_{23} + \delta_3 c_{22}\theta_{34} - \delta_2 c_{22}\theta_{45} = 0 \quad (14)$$

$$-\delta_2 c_{22}\theta_{34} + \delta_3 c_{22}\theta_{45} = 0 \quad (15)$$

where

$$\delta_3 \equiv D^2/c_{22} + 2\delta_2$$

The equation for k^2 is found to be

$$[(1/c_{12} + 1/c_2 + 1/2c_1)\,k^2 - (1/2p_1 + 1/p_2)]\,\Delta_4 - (k^2/c_2 - 1/p_2)\,\Delta_3 = 0 \qquad (16)$$

where the Δ's are as previously defined, *i.e.*,

$$\Delta_3 = K^4 - 1$$

and

$$\Delta_4 = K^6 - 2K^2$$

where

$$K^2 = [(1/c_{22} + 2/c_2)\,k^2 - 2/p_2]/(k^2/c_2 - 1/p_2)$$

Equation (16) can be written

$$[(1/c_{12} + 1/c_2 + 1/2c_1)\,k^2 - (1/2p_1 + 1/p_2)]\,A_4 - (k^2/c_2 - 1/p_2)^2\,B_4 = 0$$

where

$$A_4 = [(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^3$$
$$- 2\,(k^2/c_2 - 1/p_2)^2\,[(1/c_{22} + 1/c_2)\,k^2 - 2/p_2] \quad . \quad . \qquad (18)$$

and

$$B_4 = [(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^2 - (k^2/c_2 - 1/p_2)^2 \quad . \quad . \qquad (19)$$

There is also the frequency of the blades

$$(1/2\pi)\sqrt{(c_1/p_1)} \quad . \quad . \quad . \quad . \quad . \qquad (20)$$

and the fore and aft frequencies

$$(1/2\pi)\sqrt{(\Omega^2 + c'_1/p'_1)} \text{ and } (1/2\pi)\sqrt{(\Omega^2 + c'_2/p'_2)} \quad . \quad . \qquad (21)$$

of the blades and cranks respectively.

In the case of a four-bladed airscrew we write $2c_1$ for c_1 and $2p_1$ for p_1 in (17).

Example

Let the moment of inertia of the airscrew be very large compared with that of the estimated crank pin loads and let the flexibility of the airscrew and cranks be neglected.

The equation for k^2 will then be

$$(k^2/c_{12} - 1/p_2)\,[(k^2/c_{22} - 2/p_2)^3 - (2/p_2^2)\,(k^2/c_{22} - 2/p_2)]$$
$$- (1/p_2^2)\,[(k^2/c_{22} - 2/p_2)^2 - 1/p_2^2] = 0 \quad . \quad . \quad . \qquad (1)$$

Let the dimensions of the journals be $1\frac{7}{8}'' \times 1''$ (dia.) and those of the shaft between the airscrew and the first throw $6'' \times 2''$ (dia.).

Then

$$c_{22} = (12 \times 10^6 \times \pi \times 1/16)/(2 \times 15/8)$$
$$= (\pi/5) \times 10^6$$

and

$$c_{12} = (12 \times 10^6 \times \pi \times 1)/(2 \times 6)$$
$$i.e., \; c_{12} = 5c_{22}$$

Equation (1) can now be written

$$(1/5)\quad (\lambda + 3)\,\lambda\,(\lambda^2 - 2) - (\lambda^2 - 1) = 0 \quad . \quad . \quad . \qquad (2)$$

where

$$\lambda = -k^2/(c_{22}/p_2) + 2$$

The greatest positive root of (2) in λ is $\lambda = 1.822$ approximately.

Thus the fundamental frequency of the system is given by

$$k/2\pi = \sqrt{(2 - 1.822)} \times (1/2\pi)\sqrt{(c_{22}/p_2)}$$
$$= (.21/\pi)\sqrt{(c_{22}/p_2)}$$

Taking the estimated crank pin load as 8lbs. at 3″

$$p_2 = (8 \times 9)/384 = 3/16$$

Hence

$$k/2\pi = (.21/\pi)\sqrt{\{(\pi/5) \times (16/3) \times 10^6\}}$$

or approximately 122 double vibrations per second.

If the engine has eight cylinders torsional resonance will occur when

$$N = (120 \times 122)/8 \text{ r.p.m.}$$

i.e., at 1,850 r.p.m. approximately.

With a four-cylinder engine with the same crankshaft dimensions and crank pin loads torsional resonance occurs at 3,700 r.p.m. approximately.

13. For a six-throw crankshaft in an ungeared engine we find the equation for k^2 is

$$[(1/c_{12} + 1/c_2 + 1/2c_1)\,k^2 - (1/2p_1 + 1/p_2)]\,\Delta_6 - (k^2/c_2 - 1/p_2)\,\Delta_5 = 0 \qquad (1)$$

or

$$[(1/c_{12} + 1/c_2 + 1/2c_1)\,k^2 - (1/2p_1 + 1/p_2)]\,A_6 - (k^2/c_2 - 1/p_2)^2\,B_6 = 0 \qquad (2)$$

where

$$A_6 = [(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^5 - 4\,(k^2/c_2 - 1/p_2)^2\,[(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^3 + 3\,(k^2/c_2 - 1/p_2)^4\,[(1/c_{22} + 1/c_2)\,k^2 - 2/p_2] \quad . \quad . \qquad (3)$$

and

$$B_6 = [(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^4 - 3\,(k^2/c_2 - 1/p_2)^2\,[(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^2 + (k^2/c_2 - 1/p_2)^4 \quad . \qquad (4)$$

For a four-bladed airscrew we write $2c_1$ for c_1 and $2p_1$ for p_1 in (1).

In addition there will be the frequencies for the blades and the fore and aft frequencies of the cranks.

14. For an eight-throw crankshaft in an ungeared engine the equation for k^2 is

$$[(1/c_{12} + 1/c_2 + 1/2c_1)\,k^2 - (1/2p_1 + 1/p_2)]\,\Delta_8 - (k^2/c_2 - 1/p_2)\,\Delta_7 = 0 \qquad (1)$$

or

$$[(1/c_{12} + 1/c_2 + 1/2c_1)\,k^2 - (1/2p_1 + 1/p_2)]\,A_8 - (k^2/c_2 - 1/p_2)^2\,B_8 = 0 \qquad (2)$$

where

$$A_8 = [(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^7 - 6\,(k^2/c_2 - 1/p_2)^2\,[(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^5 + 10\,(k^2/c_2 - 1/p_2)^4\,[(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^3 - 4\,(k^2/c_2 - 1/p_2)^6\,[(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]$$

and

$$B_8 = [(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^6 - 5\,(k^2/c_2 - 1/p_2)^2\,[(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^4 + 6\,(k^2/c_2 - 1/p_2)^4\,[(1/c_{22} + 1/c_2)\,k^2 - 2/p_2]^2 - (k^2/c_2 - 1/p_2)^6$$

For a four-bladed airscrew we write $2c_1$ for c_1 and $2p_1$ for p_1 in (1).

In addition there will be the frequencies for the blades and the fore and aft frequencies of the cranks.

15. Generally for an ungeared engine with an n throw crankshaft the equation for k^2 will be

$$[(1/c_{12} + 1/c_2 + 1/2c_1)\,k^2 - (1/2p_1 + 1/p_2)]\,\Delta_n - (k^2/c_2 - 1/p_2)\,\Delta_{n-1} = 0 \qquad (1)$$

where

$$\Delta_n = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

is the equation for the crankshaft system alone.

16. In the case of the airscrew blades being rigid (if this is possible) or the airscrew being replaced by a flywheel, we put

$$1/2c_1 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

in the general equation in §15 and for $2p_1$ we write p_1 where gp_1 is the moment of inertia of the flywheel about the axis of the crankshaft.

If it is desired to neglect the flexibility of the cranks we put

$$1/c_2 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

in both (1) and (2) of §15.

Under these conditions we find

$$K^2 = -k^2/(c_{22}/p_2) + 2 \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

If in addition p_1 is extremely large two interesting cases arise.

CASE I.—All the journals equal. In this case the general equation, for an n throw crankshaft ungeared becomes

$$[-k^2/(c_{22}/p_2) + 1]\,\Delta_n - \Delta_{n-1} = 0 \quad . \quad . \quad . \quad . \qquad (4)$$

or

$$K^2\Delta_n - \Delta_{n-1} - \Delta_n = 0 \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

or

$$\Delta_{n+1} - \Delta_n = 0 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

The roots of this equation have already been discussed in §10.

CASE II.—The journal at the flywheel end, twice as stiff as the other journals. The general equation now becomes

$$[-k^2/(c_{22}/p_2) + 2]\,\Delta_n - 2\Delta_{n-1} = 0 \quad . \quad . \quad . \quad . \qquad (7)$$

or

$$\Delta_{n+1} - \Delta_{n-1} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

The roots of this equation have also been discussed in §10.

As an example consider the 300 h.p. Maybach aero engine. This is a six-cylinder vertical engine with a direct drive.

All the journals are equal.

The approximate dimensions of the journals are:—Outer dia. 2.6ins., inner dia. 1.4ins., and length 2.64ins.

Thus

$$c_{12} = c_{22} = \{\,12 \times 10^6 \times \pi\,(2.6^4 - 1.4^4)\,\}/(32 \times 2.64)$$
$$= 18.84 \times 10^6 \text{ approx.}$$

The moment of inertia of a throw complete with rotating part of connecting rod is approximately 96lb. ins.^{2}.

Thus

$$p_2 = 96/384 \text{ or } \tfrac{1}{4}$$

Hence

$$\sqrt{(c_{22}/p_2)} = 8{,}700 \text{ approx.}$$

For this case we have

$$\Delta_7 - \Delta_6 = 0$$

and by §10 the lowest value for k will be the lowest value for k in

$$\Delta_{13} = 0$$

Thus approximately the fundamental frequency of the system is

$$(3/13) \times (1/2\pi)\sqrt{(c_{22}/p_2)}$$
$$\text{or } 320.$$

Torsional resonance will occur when

$$6N/120 = 320$$
$$\text{or } N = 6{,}400 \text{ r.p.m.}$$

As the engine was designed to run up to 1,500 r.p.m. there was no danger of the crankshaft failing through torsional resonance.

17. In dealing with geared engines, the treatment will depend to a great extent on the type of gearing employed. In the case of the airscrew being

geared to the crankshaft, as in Fig. 14, the treatment is comparatively simple. We will return to that case, taking into consideration the airscrew system.

Let θ_1 be the angle turned through by the crankshaft gear wheel and θ_2 that by the first crank; and let ψ_2 be the angle turned through by the normal line of the airscrew blades and ψ_1 that turned through by the airscrew shaft gear wheel.

If c_2 and p_2 refer to the cranks, we have for the crank adjacent to the gear wheel

$$(D^2/c_2 + 1/p_2)\, c_2\phi_5 = -D^2\theta_2 \quad . \quad . \quad . \quad . \quad (1)$$

and if c_1 and p_1 refer to the airscrew blades

$$(D^2/c_1 + 1/p_1)\,[c_1\phi_1 \text{ or } c_1\phi_2] = -D^2\psi_2 \quad . \quad . \quad . \quad (2)$$

Let c_{12} refer to the shaft between the first crank and the crankshaft gear wheel and c'_{12} to the airscrew shaft between the airscrew and the gear wheel.

If F is the frictional force between the gear wheels and R_1 is the radius of the crankshaft gear wheel and r_1 that of the airscrew shaft gear wheel, then

$$Fr_1 + c'_{12}(\psi_2 - \psi_1) = 0 \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and

$$FR_1 - c_{12}(\theta_2 - \theta_1) = 0 \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$\therefore\ (c'_{12}/r_1)(\psi_2 - \psi_1) + (c_{12}/R_1)(\theta_2 - \theta_1) = 0 \quad . \quad . \quad (5)$$

By moments for the airscrew

$$c_1(\phi_1 + \phi_2) - c'_{12}(\psi_2 - \psi_1) = 0 \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Thus by (2) and (5)

$$-2D^2\psi_2 + (r_1/R_1)(D^2/c_1 + 1/p_1)\, c_{12}\theta_{12} = 0 \quad . \quad . \quad (7)$$

But

$$\psi_2 = \psi_1 - (c_{12}/c'_{12})(r_1/R_1)\,\theta_{12} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and

$$R_1\theta_1 = r_1\psi_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Hence (7) becomes

$$-D^2\theta_1 + [(r_1^2/R_1^2)(1/2c_1 + 1/c'_{12})\, D^2 + r_1^2/2R_1^2p_1]\, c_{12}\theta_{12} = 0 \quad . \quad . \quad (10)$$

By moments for the first crank

$$c_2\phi_5 - c_{12}\theta_{12} + c_{22}\theta_{23} = 0 \quad . \quad . \quad . \quad . \quad . \quad (11)$$

so that by (1)

$$D^2\theta_2 + (D^2/c_2 + 1/p_2)\, c_{12}\theta_{12} - (D^2/c_2 + 1/p_2)\, c_{22}\theta_{23} = 0 \quad . \quad . \quad . \quad . \quad (12)$$

Hence by (11) and (12)

$$[\{1/c_{12} + 1/c_2 + (r_1^2/R_1^2)(1/2c_1 + 1/c'_{12})\}\, D^2 + r_1^2/2R_1^2p_1 + 1/p_2]\, c_{12}\theta_{12}$$
$$- (D^2/c_2 + 1/p_2)\, c_{22}\theta_{23} = 0 \quad . \quad . \quad . \quad . \quad . \quad (13)$$

or

$$(D^2/c_{12} + \delta'_1 + \delta_2)\, c_{12}\theta_{12} - \delta_2 c_{22}\theta_{23} = 0 \quad . \quad . \quad . \quad (14)$$

where

$$\delta' \equiv (r_1^2/R_1^2)(1/2c_1 + 1/c'_{12})\, D^2 + r_1^2/2R_1^2p_1$$

and

$$\delta_2 \equiv D^2/c_2 + 1/p_2$$

as previously defined.

The equations for the remaining cranks will be as for the ungeared case in §12. Hence the equation for k^2 in the case of the four-throw crankshaft with the engine geared as in Fig. 14 (the airscrew being two-bladed) is

$$[\{1/c_{12} + 1/c_2 + (r_1^2/R_1^2)(1/2c_1 + 1/c'_{12})\}\ k^2 - (r_1^2/2R_1^2p_1 + 1/p_2)]\,\Delta_4$$
$$- (k^2/c_2 - 1/p_2)\,\Delta_3 = 0 \quad . \quad . \quad . \quad . \quad . \quad (15)$$

where the Δ's are as previously defined. The equation (15) can be written

$$[\{1/c_{12} + 1/c_2 + (r_1^2/R_1^2)(1/2c_1 + 1/c'_{12})\} k^2 - (r_1^2/2R_1^2p_1 + 1/p_2)] A_4 - (k^2/c_2 - 1/p_2)^2 B_4 = 0 \quad . \quad . \quad . \quad . \quad (16)$$

where A_4 and B_4 are as in the ungeared case in §12.

Similarly for the case of a six-throw crankshaft, geared as in Fig. 14, the equation for k^2 is

$$[\{1/c_{12} + 1/c_2 + (r_1^2/R_1^2)(1/2c_1 + 1/c'_{12})\} k^2 - (r_1^2/2R_1^2p_1 + 1/p_2)] A_6 - (k^2/c_2 - 1/p_2)^2 B_6 = 0 \quad . \quad . \quad . \quad . \quad (17)$$

where A_6 B_6 are as defined in §13; and for an eight-throw crankshaft we have A_8 and B_8 as defined in §14.

CORRESPONDENCE

To the Editor of the Journal of the Royal Aëronautical Society.

Sir,—The notable addition to the Society's library of a selection of rare and interesting books relating to early phases of aeronautics (as described in the article reprinted in last month's Journal from the "Times" Literary Supplement) is rightly regarded as a matter of congratulation. May I be allowed, as one greatly interested in aeronautical history, to offer a few observations on the two-fold interest of this acquisition?

Firstly, it may be claimed that the purchase of these books for the Society's library is of consequence, inasmuch as it tends to make the library—what it surely ought to become—the most important collection of historical and technical books, English and foreign, in all branches of that science which it is the aim of the Society to further by every means in its power. If it be said—as one has heard it said of collections of early works on other sciences—that the older books are seldom referred to, it may be answered that the prestige of any scientific institution receives permanent support from the possession of an important and well-arranged library of reference. The acquisition for the library of the Institution of Electrical Engineers of the remarkable collection of rare early works on electricity and magnetism, formed by the late Professor Sylvanus Thompson, may be cited as a signal example.

Secondly, the possession of a library of great and increasing historical value may well prove to be a matter of more direct practical importance, should it lead the Society to encourage a study of the historical aspects of aeronautical history. Hitherto, very little serious work of that character has been done in England—the publication by the Society of the series of "Aeronautical Classics" is hardly more than an admirable exception which goes to prove the rule. Encouragement is at the present time the more desirable in view of the marked attention which has been paid in recent years to the study of historical science and technology in all directions. Important work of this kind has been done, and is now being done, in England—examples will readily occur to all students of science—and it is therefore unnecessary to particularise. The remark holds good of the Continent, and in the United States there has been remarkable activity on similar lines of historical research, covering the more important fields of applied science and technical industries. Aeronautical science, though the youngest regarded from the point of view of practical achievement, is destined for years to come to be in the van of progress. Can it afford to stand apart from the endeavours now being made to satisfy the need for historical knowledge of a technical character by neglecting the study of its own history—a history which, though scant in important material, and interminably slow in the tale of its unfolding, is yet of undeniable interest, and should be allowed its rightful place in the annals of science and engineering?

Up to the present, however, it must be admitted that in England scholarship has almost wholly ignored, or at the least shown a marked indifference to, the historical aspects of aeronautics. It is significant that to this day Hatton Turnor's cumbrous and ill-arranged compilation, "Astra Castra" (published in 1865), is perforce still frequently referred to as the "best" and "fullest" authority. That it has been quoted by countless writers may be fully admitted, but it is difficult to believe that those who cite it so freely (though not always by name) ever studied the subject at first hand. As a matter of fact, there is very little original work in Turnor's volume—of the 518 pages, about 350 consist of tedious padding in the form of poetical and Scriptural quotations, or wholesale

reprints of earlier books and essays, made for the most part without any comment or connected narrative. Further, it contains a full measure of inaccuracies and notable omissions, and a bibliography which is hopelessly inadequate.

Moreover—not to attempt a survey of aeronautical literature—the D.N.B., though it includes such well-known pioneers of the free balloon as Vincenzo Lunardi (who rightly described himself as a child of Britain only by a species of adoption), Charles Green, George Gale (whose inclusion it would be hard to justify), and Henry Coxwell, omits such important names in the evolution of British aeronautical science as Sir George Cayley (the full extent of whose work in both branches of aeronautics—dirigible balloons and mechanical flight—has not yet been adequately noticed), John Stringfellow, F. H. Wenham and Percy S. Pilcher. Indeed, it must be said that English aeronautical books dealing with the historical aspects are, broadly speaking, unscholarly, compiled with a minimum of research, unreliable as to facts, and usually lacking in authorities, while they fail to convey any large view of the lines on which aeronautical science evolved.

On the other hand, France has been far better served by her scholars and scientists. Faujas de Saint-Fond set an example as early as 1783 in his scientific account of the first aerostatic achievements, an example followed in later times by Dupuis-Delcourt, Landelle, Tissandier (whose fine "Histoire des Ballons" is, however, marred by too frequent errors), Lecornu, and others, all of whose writings are distinctly in advance of any similar works published in England. Nor can we boast anything to compare with the sumptuously illustrated volumes —designed as pictorial histories of flight—published in Paris under the editorship of M. F.-L. Bruel (1909) and the Comte de la Vaux (1922), or with the comprehensive and accurate I.L.A. Catalogue compiled by Dr. Liebmann and Dr. Wahl in connection with the Aeronautical Exhibition at Frankfort in 1909. Italy, also, has to her credit the scholarly historical monograph "Il Volo in Italia" by Guiseppe Boffito, published in Florence two years ago, while a more modest but useful little work by E. Fuld was privately printed at Amsterdam in 1918 under the title of "Uit de eerste jaren der Luchtvaart in Nederland."

In face of these facts—capable of amplification, but of themselves surely not creditable to a country which has done great things in the air, or to an Empire which must ever be as deeply concerned in aerial as it is in maritime navigation—may I respectfully urge the Council of the Society to encourage, if they cannot actively promote, researches into the history of aeronautical science, to which the admirable acquisition herein discussed may be deemed to serve as a fitting prelude. Would it, for instance be possible to afford a brief space in the pages of the Journal for limited excursions into the technology of aeronautics? Or if the regrettably high cost of printing precludes even occasional essays in that direction—and it will be generally allowed that the invaluable papers recording the most recent results of aeronautical research, as printed in the Journal from month to month, constitute a very important part of the Society's work—might it not exert some influence by inviting the co-operation of the Newcomen Society (whose excellent work on the history of engineering and technology is deserving of all possible support) in discussions on subjects of aeronautical interest? As a lay member of both Societies I throw out these ideas unofficially and with all diffidence, but not without hope of some result.

And may it not be added there is reason to believe that endeavours on the lines above mentioned would tend to have the incidental but most desirable result of stimulating public interest in aeronautical matters at large? One's limited experience in lecturing on historical aeronautics confirms the impression that in this, as in other matters, the regrettable lack of interest is due, partly at least, to lack of knowledge.—Yours faithfully,

J. E. Hodgson.

Bromley.

March 12th, 1923.

REVIEWS

Aerodynamik

R. Fuchs and L. Hopf (R. C. Schmidt & Co., Berlin).

In this volume the authors aim at a complete account of the aerodynamics of the aeroplane which may serve as a basis for any further work either on practical or on theoretical lines, and they have undoubtedly produced a most interesting and valuable book on this subject. The book is divided into two main parts, the first dealing with the physical theory of air forces and the second with the motion and stability of an aeroplane.

After a short introductory chapter on the methods of classical hydrodynamics, a very full account is given of the problem of the two dimensional aerofoil, including the well-known series of Joukowski aerofoils and the extended types which have been discussed more recently by Karman and Trefftz. The next two chapters deal with Prandtl's theory of finite aerofoils on lines characteristic of Dr. Fuchs himself. The fifth chapter is entitled the "Theory of Drag," and contains an excellent and full account of the theoretical development of this subject. The theory of drag forces is the weakest part of aerodynamic theory, but the various attempts to obtain an analytical solution of the problem are reproduced. Helmholtz' surfaces of discontinuity, Karman's vortex rows and Prandtl's viscous boundary layer all throw some light on this obscure problem and indicate the lines of further progress even if they do not as yet provide a method of calculating the drag of a body.

The theoretical work is followed by a long chapter containing typical experimental results relating to scale effect, the forces on aerofoil structures, tail planes and other aeroplane parts, and concluding with a detailed analysis of the drag of two aeroplanes. The first part of the book concludes with a short and rather unsatisfactory outline of airscrew theory.

The second part of the book, due mainly to Dr. Hopf, discusses the motion and stability of an aeroplane. The opening chapter deals with performance, atmospheric conditions, methods of test and of analysis, and concludes with an analysis of the component weights of a large number of aeroplanes. The second chapter is concerned with the balance of the pitching moment of an aeroplane, and is a very full treatment of the problem, including a criticism of the analogy of the metacentric height of a ship which is so often used in a fallacious manner. The last two chapters discuss very fully the general longitudinal and lateral motion of an aeroplane, including the stability of straight flight, the equilibrium of circling flight, and the problems of spinning and autorotation. The treatment of these problems is quite satisfactory, and, although differing in formal presentation, is essentially identical with current English practice, to which indeed the author expresses his indebtedness in several particulars.

The whole book is well composed, and is a continual inspiration to further investigation. There are unfortunately a number of misprints which increase the difficulty of understanding the mathematical analysis, and it is regrettable that so good a book should contain the statement that the centre of pressure of a wing goes to infinity when the lift vanishes. These are, however, only minor points in an excellent book which should become one of the classical works on aerodynamics.

THE JOURNAL

OF THE

ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

No. 149 MAY 1923 VOL. XXVII

NOTICES

Election of Members

The following Members were elected at a Council Meeting held on April 17th :—

Associate Fellows.—P. T. Griffith, Ivor B. Hart.

Council

As a result of the elections announced at the Annual General Meeting (a report of which will be found on another page) the Council for the year 1923-1924 is constituted as follows :—Professor L. Bairstow, C.B.E., F.R.S., Chairman ; Lieut.-Col. M. O'Gorman, C.B., D.Sc., Vice-Chairman ; Brig.-General R. K. Bagnall-Wild, C.M.G., C.B.E. ; Griffith Brewer, Esq. ; Wing Commander T. R. Cave-Browne-Cave, C.B.E. ; Sir Mackenzie D. Chalmers, K.C.B., C.S.I. ; C. R. Fairey, Esq., C.B.E. ; H. P. Folland, Esq. ; Squadron Leader R. M. Hill, M.C., A.F.C. ; Professor C. Frewen Jenkin, C.B.E. ; Professor B. Melville Jones, A.F.C. ; Lieut.-Col. J. T. C. Moore-Brabazon, M.C., M.P. ; J. D. North, Esq. ; Lieut.-Col. A. Ogilvie, C.B.E. ; Professor A. J. Sutton Pippard, D.Sc. ; J. L. Pritchard, Esq. ; Colonel the Master of Sempill, A.F.C. ; Major R. V. Southwell ; Lieut.-Col. H. T. Tizard, A.F.C. ; Major H. E. Wimperis, O.B.E. ; *Honorary Treasurer*—A. E. Turner, Esq.

Students' Section

Students attending the visit to Messrs. Fairey's works, at Hayes, on Wednesday, May 30th, will meet in the Booking Hall at Paddington Station at 2.5 p.m. The train leaves at 2.25 p.m.

The numbers attending these visits have up to the present been small, and it should be mentioned that if the number of names received by the Honorary Secretary of those wishing to attend any visit proves inadequate the visit is liable to be cancelled at short notice. Names should be forwarded in each case not less than four days before the date of the visit concerned. It is usually possible to arrange for members of other grades to accompany these parties if they wish to do so.

Overseas League Lecture

Mr. F. Handley-Page is lecturing to members of the Overseas League on "British Commercial Flying," at Vernon House, Park Place, St. James's Street, S.W.1., on Monday, May 7th, at 8.0 p.m. Any member wishing to attend should apply for tickets to the Reception Secretary, Overseas League, at Vernon House.

International Air Congress, London, 1923

Members are reminded that the organisation of the International Air Congress, which is to take place at the Institution of Civil Engineers, Great George Street, Westminster, during the week June 25th-30th, is being undertaken by the Society's staff, from whom all particulars can be obtained. A strong programme of entertainments and special visits to places of technical interest is being arranged.

Forthcoming Arrangements

May 7, 7.0 p.m. *Overseas League.* Mr. Handley-Page, "British Commercial Flying."
,, 15, 5.0 p.m. Council Meeting.
,, 30, — *Students' Visit.* The Fairey Aviation Co., Hayes.
,, 31, 5.30 p.m. *Wilbur Wright Lecture.* Dr. J. S. Ames, "The Relation between Aeronautical Research and Aircraft Design," at Royal Society of Arts, John Street, Adelphi, W.C.2.
June 25-30. *International Air Congress.*

W. Lockwood Marsh, *Secretary.*

THE ROYAL AERONAUTICAL SOCIETY

Annual General Meeting

The Fifty-eighth Annual General Meeting of the Society was held in the Society's offices, 7, Albemarle Street, London, at 5 p.m., on Tuesday, March 27th, 1923. The Chairman, Professor L. Bairstow, C.B.E., F.R.S., presided, and the following were present :—

Council.—Sir Mackenzie Chalmers, Colonel The Master of Sempill, Major R. V. Southwell.

Members.—R. M. Balston, Commander F. L. M. Boothby, C. F. Dendy Marshall, Lieut.-Colonel S. Heckstall Smith, Major D. H. Kennedy, J. L. Lake, Major A. R. Low, Lieut.-Colonel V. C. Richmond and Dr. H. C. Watts.

Commander Boothby, Mr. Dendy Marshall and Major Kennedy were appointed scrutineers of the ballot for the election of Council.

The Chairman, in presenting the Council's report and balance sheet, said that it was of great interest that the organisation of the International Air Congress, which is to be held in London this year, is to be carried on here, a special grant which will cover the estimated expenditure having been made for the purpose.

He called attention to the grant which had been made to the Society by the Trustees of the Carnegie United Kingdom Trust Fund, which had made it possible to purchase a very valuable collection of early aeronautical books, and also more modern ones, badly needed to complete the Society's collection. The Secretary had found that these rare books were in the hands of Messrs. Maggs, and the Council mentioned the matter to Lord Weir, who approached the Trustees of the Carnegie Fund, with the result that a sum of £500 was granted to the Society, most of which had been expended in the manner described, while the rest would be spent during the coming year. The Trustees had made very few conditions with regard to the grant, the most important being that the Society should agree to lend the books if applied for through the Central Library for Students.

On the whole, the position as regards Membership of the Society was rather more satisfactory than it had been during the last two or three years. Instead of the membership falling, the total for the year showed a slight increase. Subscriptions are difficult to get in, but on the whole he considered the position of the Society far better than it was last year.

The Society's custom of awarding medals for papers had been revived during the year under review, and Mr. H. R. Ricardo was awarded the Silver Medal for 1922 for his paper on "Some Possible Lines of Development in Aircraft Engines." That medal is given to the author of the best paper published in the Journal during each year, but it should be explained that the Council, who decide on the merits of the various papers, do not consider it desirable to award this prize to Members of Council, who are therefore debarred from an award under that heading.

The R.38 Memorial Research Fund had now reached a total of £1,264 19s. 11d., and the Council had decided to devote the Fund in part to the erection of a memorial tablet in the Library over the mantelshelf. This work

was in the hands of Mr. Paul Cooper, who hoped shortly to show the Council a small model of his design. Part of the income of the Fund is to be devoted to a prize of twenty-five guineas annually for the best paper dealing with some technical subject in aeronautics, preferably in connection with airships, to which there had been a good response. Although the entries had to be made before December 31st last year, twelve entries had been received (British, German, Italian and American), and it was interesting to find that they were all connected with airship problems.

The last year had seen the institution of the Society's examination for Associate Fellowship. Not many entries were made, but the important point was that the whole machinery for the examination is now ready so that the Society can deal with any Students who come forward. The fees decided upon barely covered expenses, but University practice had been followed, and it was not proposed to alter them.

Another new feature was the holding of a Technical Discussion. So far there had been two very good meetings on the "Prandtl Theory of the Flow Round an Aerofoil." Mr. Glauert wrote a paper to start the discussion, which had been put into print. At both meetings several people spoke, and now three or four papers were coming along ready for printing. When the whole discussion was published in the Journal it would give an interesting account of the various opinions held on this subject. By no means a unanimous view would appear, but the effect of the discussion had been to show just where the Prandtl theory is useful, and just where suspicion about it arises.

Other subjects had been suggested to follow this discussion, the holding of which had been very well attended. On both occasions there was considerable difficulty in finding seating accommodation in the Society's Library for the number of people present.

The title of the Journal had been changed at the end of the year, a certain amount of difficulty having arisen from the fact that the title of "The Aeronautical Journal" did not disclose that it had any connection with the Royal Aeronautical Society. The circulation had increased, owing to efforts made by the Editor and Secretary. A good many of the American Air Service stations now received it officially, and it was also supplied by the Air Ministry to Royal Air Force units. Mention of the Journal reminded him that he should like to bring to the notice of the meeting the services of Mr. J. L. Pritchard, who has acted as Editor of the Journal for some years. He was appointed in 1919 at a salary of £50, and that salary he had never accepted, or had returned it as a donation to the Society, and the Council felt very strongly that they were greatly indebted to him for getting the Journal into a good form and improving it technically. He thought most of the members present must have noticed its improved appearance and form.

The Students' Section had been very active. It had made visits to various works, and had held several meetings. It had also branched out in an entirely new line by holding its first social function—a smoking concert—and this was to be followed by another concert to which other grades of the Society's membership were invited.

With regard to the Balance Sheet, if Members would look at the bottom of this they would find the whole summarised by an excess of expenditure over income of over £278. That was an unsatisfactory state of affairs in some ways, but it did not mean that the Society was not improving its position. The best way to get rid of the deficit would be to improve the number of members. With more members the total cost of Journals went up, but the cost of salaries did not, and the Society was in a position greatly to expand its membership at very slight expense. Of course, if the aeronautical industry were in a more flourishing

condition, the Society's membership would soon feel a response. The Balance Sheet had been approved by the members of Aerial Science Limited.

The adoption of the Report was proposed by Major Low, seconded by Dr. Watts, and carried unanimously.

The scrutineers then presented the result of the ballot, and the following members were declared elected to serve on the Council for the two years ending March, 1925 :—

Professor L. Bairstow, C.B.E., F.R.S., Wing Commander T. R. Cave-Browne-Cave, C.B.E., Sir Mackenzie Chalmers, K.C.B., C.S.I., Mr. C. R. Fairey, C.B.E., Lieut.-Colonel J. T. C. Moore-Brabazon, M.C., M.P., Lieut.-Colonel M. O'Gorman, C.B., D.Sc., Mr. J. L. Pritchard, Colonel The Master of Sempill, A.F.C., Major R. V. Southwell, and Lieut.-Colonel H. T. Tizard, A.F.C.

A vote of thanks to the scrutineers was proposed by Professor Bairstow and seconded by Colonel The Master of Sempill, and the meeting closed with a vote of thanks to the Chairman proposed by Mr. R. M. Balston and seconded by Major Kennedy.

PROCEEDINGS

Eighth Meeting, 58th Session

An Ordinary General Meeting of the Society was held at the Royal Society of Arts, on Thursday, February 1st, 1923, Professor L. Bairstow in the chair.

The Chairman, in opening the proceedings, said that Mr. G. S. Baker, O.B.E., of the National Physical Laboratory, would deal with flying boats and seaplanes. He would deal with the hull and its design, that part of the seaplane which differentiates it from the aeroplane. That subject had been touched on very lightly by Major Rennie at the previous meeting of the Society, in view of the present paper by Mr. Baker. Mr. Baker had begun work in 1912 on the problems of hull design, at a time when nothing of a definite nature was known; a few individual experiments had been carried out, but there was no systematised knowledge at all at that time. From that state of ignorance a great deal of experimental work had now rescued us. He did not know how far Mr. Baker would stress the point, but it was quite clear, from the investigation of certain accidents to seacraft, that there were fundamental differences in the behaviour of seaplane hulls on the water, differences which had a great deal of effect on the risk of flying. For instance, if one type of hull was such that when the plane rose in the air it stalled, then all the aerodynamical consequences of stalling followed, and there was difficulty. On the other hand, it appeared that we had a type of flying boat which did not make the plane stall on getting into the air, and consequently if it came back to the water it was still controlled. For this type of development, which he believed really dated back to the C.E.1, we were mainly indebted to Mr. Baker and his associates at the National Physical Laboratory, and to the generosity of Sir Alfred Yarrow in placing such a magnificent piece of apparatus as the experimental tank at the disposal of the nation.

Mr. Baker then read his paper on "Ten Years' Testing of Model Seaplanes."

"TEN YEARS' TESTING OF MODEL SEAPLANES"

BY G. S. BAKER, ESQ., O.B.E., M.INST.N.A.

Summary

§1. The Paper gives an account of the beginnings of tank tests of seaplane hulls carried out in the first place under the authority of the Advisory Committee for Aeronautics in 1912.

The tests commenced with plate experiments. The broad results of the trials of the floats designed after these first tests are given in §5. A detail account of the apparatus now used for standard tests (§7) is followed by some notes on the aims of the tests, and a very brief account of the work so far accomplished (§9). The differences between the model tests and the machine are dealt with in some detail (§11), and the method of estimating machine data from model tests is given in §12, together with the checks upon such estimates obtained by actual measurement on machines.

A list of published papers giving results of tests carried out in the tank, is contained in the Appendix.

Introduction

§2. For the benefit of the uninitiated, a brief account of the William Froude National Experiment Tank is given here so as to make clear what follows. The tank is a long concrete basin 550ft. in length and 30ft. in breadth, filled with water. A carriage on four wheels spans right across the water way. The apparatus used in testing a model is secured to this carriage, which can be propelled along the tank at any speed required for that particular test of the model. Speeds can be varied from nil to 25ft. per second, and towing forces up to 200lbs. can be measured without disturbing the steady movement of the carriage.

The tank was built mainly from funds provided by Sir Alfred Yarrow, so that research work in hydrodynamics, marine propulsion and naval architecture could be carried out in this country, and was mainly intended for the use of mercantile firms. It has been adapted for the purpose of seaplane testing at comparatively little expense. It should be understood that work for the mercantile marine has been carried out at the same time as the tests here described, and in increasing volume, and for this reason progress with aeronautic work is sometimes slow. Perhaps in the future it may be possible to build a really high-speed tank, devoted to this and high-speed vessel tests alone, when our data can be extended and work proceed continuously.

The majority of the work described is based on work carried out under instructions from the Advisory Committee for Aeronautics, the Air Ministry or other Government authorities. Recently a fair proportion of the test work has come more or less directly from firms. The remarks on full-scale machines are based almost entirely on data taken by the author or his assistant from observations when flying on various machines, and from the opinions and statements made by the various pilots on such machines. The Author is glad to take this opportunity of expressing his appreciation of the trouble taken by the pilots and the officers commanding air stations on his behalf in these tests, and of the work carried out by his assistant, Miss E. M. Keary, under whose care a good deal of the tank test work has been done.

Original Tests

§3. The first experiments on the shape of floats for seaplanes, of which reasonably full data is extant, were those carried out under the direction of Captain Sueter. These were made towards the end of 1911, on a full-size Avro machine in the Cavendish Dock, Barrow-in-Furness, and an account of them is given in Advisory Committee for Aeronautics Report T.157, dated December 21st, 1911. This report reached the tank through Sir R. T. Glazebrook, and after some discussion, I was asked by him to propose means and methods of obtaining data such as Captain Sueter mentioned in the report as being needed. It was due to Sir Richard Glazebrook's support and activity that this work was ultimately put in hand and advanced. In April, 1912, a B.E.2 machine fitted with a single main float was at work on the Fleet Pond, and I was able to see this machine, which did reasonably well on the water, put through certain tests, and a little later obtained some first-hand knowledge of the behaviour of the Avro, Farman and Short types of machines, and heard the opinions of Commander Samson and other expert pilots.

On May 10th proposals for tests in the tank were made to the Advisory Committee, and these were approved in June, 1912. At that time it was difficult to decide what qualities were desirable in a seaplane, and still more difficult to decide on a compromise which gave the best result. There were a few successful skimming motor boats in existence, the general features of which were known, and I had the advantage, in the autumn of 1912, of meeting Sir J. Thornycroft, seeing his models and discussing these with him and his daughter.

It was fairly clear that some of these motor boat features should be embodied in any seaplane float or hull, but that they would need some modification to allow of change of trim at high speed, and this was really the only starting point for experimental work.

§4. As the main features of these boats were flat planing bottoms for a fair proportion of the boat, associated in some cases with a seaworthy bow, it was decided to commence the tests by obtaining data relative to the lift and drift of a flat plane "skimming" on the water surface, and to follow this with tests on an elementary form of float. These tests were made on a surface varying in area up to 2.2 sq. ft. and in angle up to 10deg. with large areas, 15deg. with the smallest areas. The results are given in R. and M. No. 70 of the Advisory Committee for Aeronautics. The apparatus used is shown in Slide 1. It was necessarily strong and heavy, as the forces to be measured were large—ranging up to 70-80lbs.—and was modified for the float tests. The heavy gear for carrying the plate was replaced by a light carrier to which either one or a pair of floats in parallel could be attached. The apparatus did not allow of any change in running angle during any experiment, and this had to be set beforehand as required. The floats, however, could rise to their natural level at the fixed angle, this level varying of course with the speed.

Twenty-five models, beginning with toboggan forms and ending with types having flaring bows on motor boat lines, were tried in this set of experiments. The tests were necessarily incomplete in many respects because of their restrictions, partly because of the assumptions involved, and partly that we were exploring unknown ground, and the importance of some features was only realised slowly. The effect of gap between floats was tested at high speed and found to be negligible. We now know that gap, or rather want of reasonable gap, has its worst effect at 12-18 knots or thereabouts. The general effect of shape of bow upon the wetness of the floats, and their tendency to nose dive under certain conditions was, however, clear and instructive. The same remark applies to both the effect of ventilation holes to the step when this was fitted, and the effect of transverse shape of the bottom at the step. It should be mentioned here that for the sake of simplicity of construction, except in the case of one set of tests of concave and convex bottoms, the models were so shaped that any transverse section consisted of four straight lines, the sides flaring towards the fore end, these surfaces being "developable," *i.e.,* surfaces on which wide planks could be worked without "cockling," the top and bottom being horizontal.

§5. The experiments ended in an attempt to embody what had been learnt, in a design for a machine of some 2,000lbs. weight, the machine being designed at Farnborough, and the floats being arranged in general to suit their requirements. These floats were known as 43B, and are shown in R. and M. 70. Our tests showed a resistance for these only one-half of that of the original form, which was based on forms then built and in use. At that time there was considerable doubt in some minds as to the applicability of our results to the full-size machine. The method of testing differed in several respects from the actual conditions under which the machine worked, and the extension of data obtained with models some 20in. in length to floats 12 or 14ft. in length depended on theories, the applicability of which was considered as doubtful. These points are dealt with in detail in §12, and are mentioned here to explain the position at the time and the desire of the Tank and the Advisory Committee to test in practice what, from tank tests, was considered a good design of float. The lines of 43B were sent to Farnborough in November, 1912, and a little later to the Admiralty. Some trials of a machine fitted with these floats were made at Farnborough in July, 1913, with satisfactory results, but no direct comparison with other types was obtained. Such a comparison was at last made by the Admiralty at Calshot in April and June, 1914—the same floats being used. A machine of 2,200lbs. weight was used, the firm's original floats being removed and replaced by 43B

floats. The original floats were of toboggan type, the fore end having a rather quick contour to its bottom, and reduced beam compared with amidships. The 438 floats had flam bows with shallow mudgards to the step and pointed tails in plan. Lieutenant Longmore's report stated these floats were extremely efficient, the machine could get off the water with three-fourths instead of the full power required with the original floats, and the manœuvring power was good. The flatness of the contour of the mudguard caused solid water to pass over them in waves of 20-25ft. length when taxiing at 14 knots, and this did not look well. I was present on the machine in some of these tests, especially those made in the swell left by the Isle of Wight mail steamers. This broad confirmation of our results was satisfactory as far as it went, and there was no opportunity to obtain further checks for several years.

§6. The next step in the testing of seaplane hulls was taken towards the end of 1914. All the apparatus was lightened, and the floats were then arranged so that they would take up their natural running angle, and the load on the floats was applied at a point at the height of the centre of gravity of the complete machine, which was also the rocking and towing point. The floats as tested were a complete set for machine, *i.e.*, consisted of two main and one tail float in general or one single boat float. These changes led to greater accuracy in the data obtained, and brought the conditions of the experiments nearer to those of the full-size machine, especially at all low and moderate speeds. The first report of experiments with this apparatus was made in December, 1914, but was never printed. Report No. 165, of March, 1915, contains a brief reference to the gear, which continued in use with slight modifications (all tending to one end, viz., lightness of apparatus) until 1917. About this time another attempt was made to get a little nearer working conditions and to still further lighten the gear. A new apparatus was designed and made, and is shown in Fig. 1. This gear has been used for all experiments since 1917, and as it is still in use I should like to describe it in some detail.

§7. In the main it consists of a light parallel motion frame supported on the travelling carriage which spans the tank. On the rear end of the frame the model is hinged, so that it can rise and trim, but is constrained to run a straight course. The fore end of the frame is attached to a dynamometer for measuring pull. The hull or system of floats is usually stripped of all wings and tail air system. In detail the flying boat hull or system of floats is attached to a light framework, called the carrier. This carrier can rock about a horizontal transverse axis, on ball-bearings, the axis being situated relative to the models at the same height and longitudinal position, to scale, as is the C.G. of the complete machine to its floats or hull. Weights are so arranged on the float carrier that the centre of gravity of the whole is on this axis. The bearings on which the float carrier rocks are carried by a light transverse vertical frame, suspended on knife edges 30in. above the ball races. These knife edges are carried on steel rods which work in vertical guides secured to the towing carriage of the tank. A fine wire attached to the upper end of each steel rod, passes over a light pulley, suspended on enclosed double ball-bearings, and is attached to a scale pan. The weight of the vertical frame and float carrier is taken on these scale pans, and the load on or lift obtained from the water can be arranged by varying the weights on the pans. A towing eye can be attached to the float carrier at any height, corresponding to scale to the height of the propeller axis on the machine. The float and frame is towed from this eye by a horizontal gate, the fore end of which is carried on a transverse vertical frame suspended on knife edges fixed to the towing carriage, the distance between the two vertical frames being 9.0ft. The major portion of the weight of the horizontal gate is taken on this forward frame, which is balanced on its knife-edges when carrying this weight. The rear vertical frame is also balanced on its knife-edges with the float carrier and models in position, the models being sometimes in the air and sometimes immersed in the water at their full load line, according to the

experiments in hand. A balanced towing rod connects this forward vertical frame to the lower end of the carriage dynamometer, and the towing force is applied by weights on this dynamometer or by the spring attached to it, at its upper end.

When the towing carriage is moved down the tank the extension of this spring required to balance the float resistance is recorded on the drum. The speed at which an experiment is made is given by records also taken on this drum, one being time in half seconds and the other distance travelled in 20ft. units. The attitude or trim of the float and its vertical movement at any speed is indicated by the pointer which forms one arm of a light bell crank lever pivoted on the rear transverse frame, the other arm of the lever being actuated by a light vertical wooden rod secured at P (Fig. 1) to the model. The point P is so arranged that the line from it to the rocking centre of the float carrier is parallel to the horizontal arm of the bell crank lever. The vertical motion of the pointer is the same as that of the rocking centre of the float carrier, *i.e.*, the centre of gravity of the full-size machine, and its fore and aft motion gives the inclination of the float. These two movements are taken by an observer reading the position of pointer against a fixed scale on the carriage.

As the resistances to be measured vary downwards to about 0.4lb., mechanical resistance must be kept to a minimum, and all swinging frames are on knife-edges, and when making ordinary experiments the floats are kept free from contact with anything in the nature of control. The weights of all moving parts are measured separately in the shop after balancing, and a check on these is obtained when the whole is erected and balanced on the carriage.

§8. An ordinary or normal set of experiments consists in towing the model in this way in smooth water at a series of constant speeds from 10 knots upwards, varying the load with the speed to take account of the air lift (see par. 3, §11), until one of two things happens—either the model porpoises too much for the work to continue or the speed attained is sufficient to show the merits of the form, and is getting too high for the trim to be regarded as accurate without correction for the effect of air structure. In the latter case, if desired, a further set of experiments is made, at high speeds, keeping the speed constant and shifting weights forward and aft on the model carrier frame, and in some cases varying the load on the model to agree with the trim it takes up under the applied moment. In this way a cross curve showing the effect of any control trimming moment on the trim and resistance is obtained.

An observer watching such experiments will ask two questions, viz., What are the aims of the tests? and secondly, Is the data so obtained applicable to the machine itself? The latter question really raises two issues, the discrepancies between model and machine conditions, and the theoretical application and actual confirmation or otherwise on the machine of such data.

Aims of Tests

§9. These may be divided into two broad categories.

(*a*) The exploration of new fields by systematic work, and all research carried out with the object of defining the effect of particular features or supplying data for general design purposes. The testing of new ideas or modifications of old ones, and the reduction of proposed types of hulls on new lines to useful forms as regards water performance.

(*b*) The determination for all machines built, of those qualities of the hull which play a part in their general efficiency on the water, and the checking that small modification necessary to attain some other purpose, have not spoilt the performance on the water or rendered the machine dangerous in its behaviour.

Under the first heading may be gathered such completed work as tests on :—

The comparison of types and the determination of the broad lines for any type on which designs must be based. The loading up of hull, including, in the case of flying boats, the effect of varying beam with given displacement, and the effect of increased load on the water due to the machine running at small angles at high speed. Nose diving at low and taxi speeds, and a study of the wave making and water disturbance in general and of the features which lend themselves to a clean bow. Transverse stability of hulls at low speeds when ploughing, at high speeds when planing, and the effect of shape of transverse section of planing bottom, and of proximity of wing surface to the water on this stability. The effect of curvature and inclination of bottom within certain limits on resistance efficiency. The determination of the resultant forces on and separate purposes served by the planing bottom before the main step, and the portion of the hull abaft the main step. The forces involved in the general running and in the impact of a hull with the water at high speeds, measurements being taken of the total or resultant force when settling under various conditions, and of the actual distribution of the force over the planing bottom in smooth and rough water, when porpoising, getting off and settling. The longitudinal stability, the study of which has advanced sufficiently to enable the tank materially to reduce and sometimes completely eliminate any tendency to porpoise in smooth water. More recently the general effect of retractable and non-retractable wheels as regards resistance and when leaving or settling on the water has been investigated in connection with the development of the amphibian machine.

The data obtained in all these tests have been published at various times as reports and memoranda by the Advisory Committee for Aeronautics, and a list of these is given at the end of the Paper. In the early part of 1918 all of these reports were carefully examined, and a memorandum, R. and M. 437, issued defining what was required in a hull and giving guidance particulars and approximate data for securing them based on the experiments completed. This was brought up-to-date in a paper on flying boat hulls issued in 1920 under the auspices of the Institution of Engineers and Shipbuilders in Scotland.

It should be said here that all such data in existence (not having an obvious military bearing) is available for English designers and builders, and the staff has always been ready and willing to assist in its application to particular designs, and to give such help as may be possible to render such designs successful, or to overcome difficulties experienced when testing an actual machine.

Under the second heading comes the standard work with new machines, carried out as "normal" tests for firms or the Air Ministry, and much miscellaneous work. A normal report on such machines gives the following data :—

(*a*) The water resistance and running attitude of the machine for all speeds up to those when the hull is planing cleanly, and higher than this in the case of those hulls which are longitudinally stable.

(*b*) Ratio of load on water to hull resistance.

(*c*) The value of the air moments required at high speeds to overcome the water moments introduced if the machine is trimmed to angles out of the normal, the location of the trim for the minimum resistance, any important resistance change with angle of trim, and the determination of the maximum angle to which the machine can be trimmed without "rocking" it. By these tests the wing chord can afterwards be arranged for the machine to "get off" below the stalling angle without using heavy control moment, and it can be seen whether there is any inducement to run at large angles from a resistance point of view.

(*d*) Notes on its cleanness in running and its longitudinal stability on the water, and any special feature that may arise in the course of the work.

Modifications to overcome any difficulty are of course made in consultation with the designers, and data sufficient to define the effect of such changes are given.

FIG 2 RESULTS FOR FLYING BOAT HULL

MODEL 353B

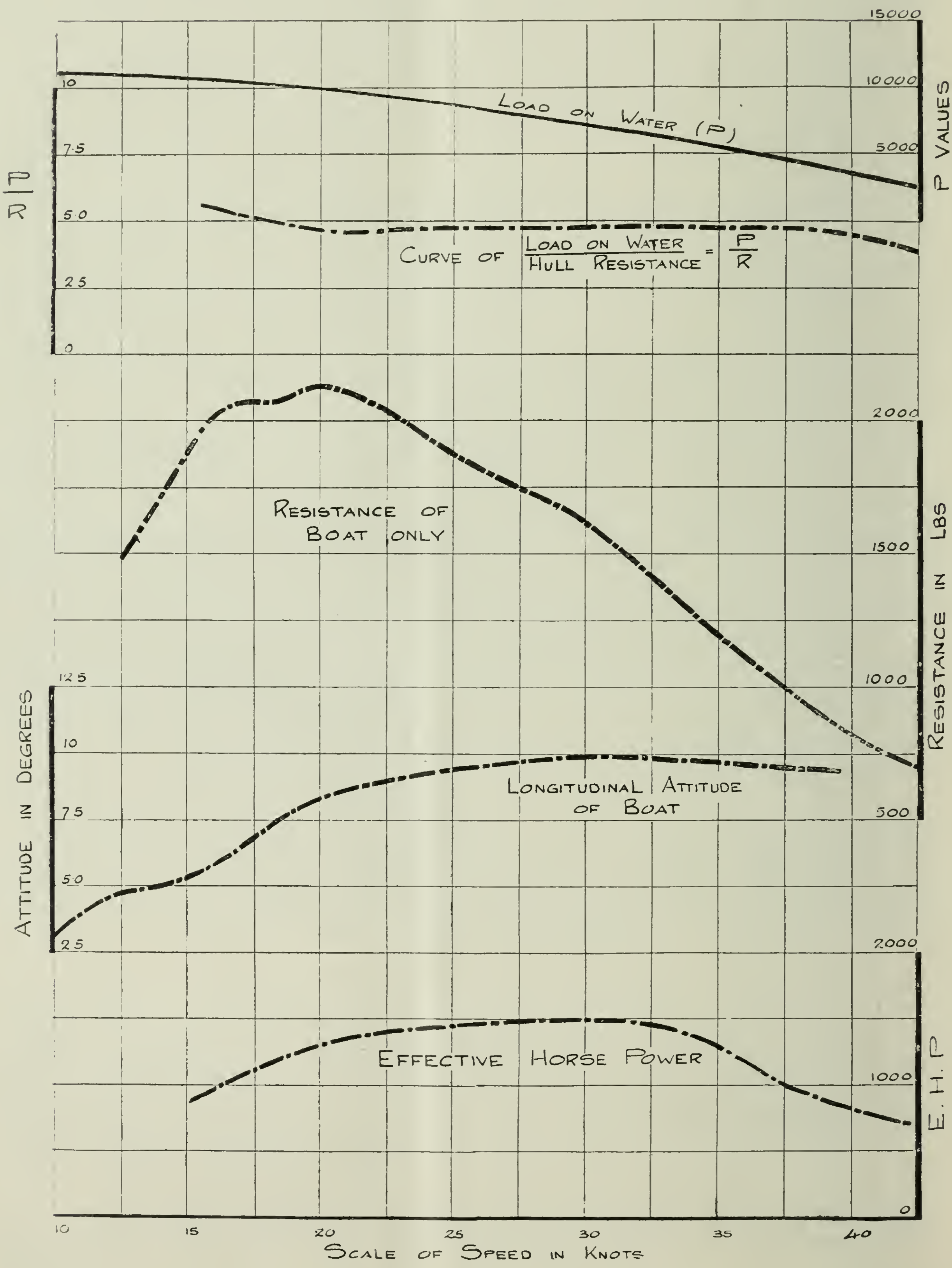
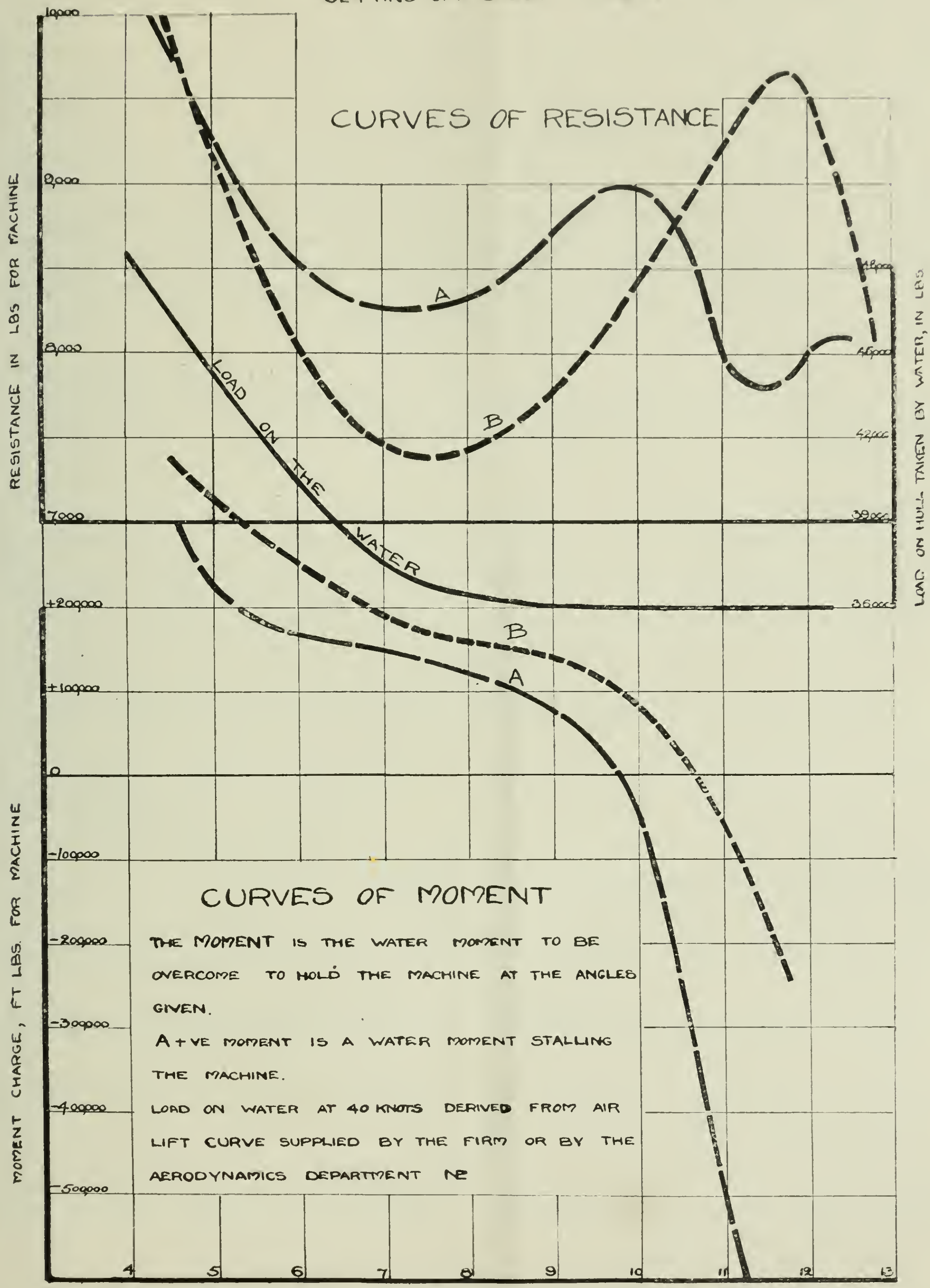


FIG 3 VARYING ANGLE OF HULL AT CONSTANT SPEED 40 KNOTS.

CURVES FOR HULL 84 FT LONG, DISPLACEMENT 100,000 LBS AT REST GETTING OFF SPEED 50 KNOTS.



INCLINATION OF HULL DATUM IN DEGREES (FOR WING CHORD ADD 5 DEGREES)

§10. A typical set of diagrams is given in Figs. 2 and 3. It is hardly possible to quote in full a typical report, but some notes will serve to direct attention to the important features. The chief ones are :—

(*a*) Resistance and cleanness of running mainly at the centre half of the speed range, and in particular at the " hump " in the resistance curve. This hump was the cause of many of the early machines failing to get off the water with any load—the propeller could not exert a thrust sufficient to overcome it. With the high powers that can now be placed on these machines, this hump has lost its pre-eminent importance, and ranks along with cleanness of running (particularly with twin or quadruple propellers) and with

(*b*) Running angle in the upper half of the speed range and ability to trim back to an angle at which the machine will fly, when a flying speed has been attained. As the size of machines increases, there is an increasing need for running at an attitude which will give a reasonably low resistance without the use of any large percentage of the available control moment, and without it being *possible* to trim the hull so far back as to be unstable if it leaves the water. At the same time, in normal flight the axis of the hull should be in the direction of motion to keep its air resistance to a minimum.

(*c*) Longitudinal stability on the water. In the early days porpoising was always a troublesome feature, but now the form is altered if necessary so as to at least reduce it to controllable dimensions, and in many cases eliminate it in smooth water.

Application of Results

§11. With regard to the restrictions involved in the experiments, the assumptions made, and in what respects model conditions differ from those found in the flying of an actual machine. The main differences between model and machine are set out in Table I., and some comments on them are given below.

TABLE 1.

DIFFERENCES BETWEEN MACHINE AND MODEL TESTS (§11).

Full Size.	Model.
Screw thrust equals water resistance + air structure resistance + acceleration force.	Corresponding thrust equals water resistance + air resistance of hull and model structure.
Speed is increasing all the time.	Speed is steady.
Load on water varies with wind force and trim of hull.	Load on water is adjusted for any speed and true for one trim only.
Trim at all high speeds is determined by the air structure.	Trim of model at high speeds depends mainly on water forces.
Pitching is damped to some extent by the wings and tail system of planes.	No material damping against pitching.
Normally the water surface is broken into waves varying up to two or three feet in height.	Experiments usually made in smooth water, occasionally in rough.
Resistance at low speeds is augmented by the wing float being on the water.	Model tested upright without wing floats.

Screw and Model Thrust

The thrust developed by the screw is roughly constant at all low speeds, increasing somewhat as the flying speed is reached. This is balanced by the water resistance of hull, the resistance of the air structure and planes and by the product of mass and acceleration. In moderately still air the air resistance is small compared with the water resistance up to about .4, the flying speed, and in any case its line of action is not materially removed from that of the screw thrust. The mass and acceleration force acts at the centre of gravity of the machine, and as the propeller is always above the C.G. there is a nose diving moment present in the machine not represented in the model. Many experiments on the models have shown that such a moment has very little effect on trim and resistance until the speed has exceeded the planing speed by a fair margin. This difference therefore does not affect the applicability of model results up to this point. Well above this speed, if the model is free from "porpoising," experiments are made to determine the effect of trimming moment on running angle, and the actual running angle can be estimated from this data, or else best inclination determined for setting the wing chord relative to the floats.

Speed Variation

The steady acceleration of the machine involves a continuously changing streamline motion, and there is no corresponding variation in the model. The importance or otherwise of this difference depends upon the sensitivity of the streamline motion to speed change, the extent to which the water resistance is due to sheer impact with and dispersal of water, or to the streamline motion set up. There is no data extant which enables one to come to conclusions on these points. A considerable portion of the resistance is known to be due to mere impact with and dispersal of the water, even at quite moderate speeds, and generally the flow *under* the model changes only slowly with speed. When the machine has reached a speed at which it can plane, this difference probably causes a little change in the speed at which planing occurs in model and machine or in machine when getting off and settling, but otherwise there is some reason for supposing that error due to this cause is not large, but only comparison with machine would determine the point.

Load on Water

The load in the machine is the total weight less the lift on the wings, which varies with the attitude of the machine. On the model this air lift, if data is not supplied by the designer, is obtained by taking it to be the total weight when the speed over the water is the "getting off" speed, and that at other speeds it varies as the speed squared. As the machine nearly always gets off at an angle fairly close to the maximum lift angle, this really comes to assuming the air lift is the maximum at all speeds. The attitude of the machine varies during acceleration from 4deg. to 6deg. at rest to the maximum lift angle at about 17 knots, remains at this angle or above up to about 25 knots, and then is more or less under the control of the pilot and is usually kept a little below the maximum lift angle until he is ready to get off. Fortunately, at speeds below 17 knots the air lift is very small in still air and light breezes, so that error due to the loading is small until the machine is well over the resistance hump. Above this speed the load in some cases is varied with the attitude of the hull to take account of the varying loading of the wings.

Trim

The trim of the machine cannot be altered by any available control until the machine is planing, model and machine running at the same angle. But at high speeds, the forces on the air structure have a greater effect than the water

forces, and the pilot can vary the running angle. It is for this reason that the cross curve at constant high speed already mentioned is made.

Pitching

Compared with the machine, the inertia of the model in pitching is large. If, therefore, any pitching develops in the model, it will come on relatively slower in the model, but will get worse than in the machine owing to the entire absence of any damping of the oscillations from the wing and tail surfaces present in the machine, the general result being the exaggeration of porpoising in an unstable form.

Water Surface

All model tests are made in smooth or nearly smooth water, whereas the machine is run under ordinary sea conditions. Smooth water has always been used, as it is considered the only safe and repeatable criterion of performance of the different forms submitted to tests. Experiments to test seaworthiness are sometimes made in either a regular or irregular sea, but resistance tests are rarely made under these conditions.

Wing Float Effect

If wing floats were fitted to the model it would add to the expense involved and render comparison of the main hulls more difficult. Their resistance can be separately calculated when necessary. They would normally be out of water as soon as the machine reached the planing condition, as its transverse stability on the water is then usually sufficient to keep it upright.

Estimates for Machine

§12. In predicting results for machine it is assumed that moments, forces, dimensions and speeds vary as the fourth, third, first and half power of the relative scales, so that if M.P.L.V. are these quantities for model $1/n$th full size, the corresponding quantities for the machine will be Mn^4, Pn^3, Ln and $Vn^{\frac{1}{2}}$ respectively. Angles will, of course, be the same for machine as in model. The above procedure neglects all corrections for any possible differences in relative value of the skin friction resistance of the model and machine. Experiments with two models of a very efficient type of motor boat, one 13 feet in length and the other 4 feet in length, showed that the skin friction correction required to bring the two sets of data into agreement at high speeds was quite small. For this 40ft. motor boat a little over the hump speed the total skin friction correction was 9 per cent. of the measured resistance. Observations of the area of bottom in contact with the water showed that this diminished with speed and the correction required would also diminish. A certain amount of other experimental data supports this inference. It is believed, therefore, that the present procedure is correct at high speeds, errs a little in giving somewhat high powers at low speeds, and that the moments given for the tests at high speeds are about correct as they are given. Any error involved is probably balanced by adverse sea and weather conditions with the machine.

§13. A more searching check upon the accuracy of estimates based on model results, is to measure the corresponding data upon full size machines, but there are many difficulties, mechanical and otherwise, in the way of such work. In the course of the past ten years the author and his assistant have flown on a number of machines, always with the object of getting such data when feasible. But it is difficult to obtain facilities for this work, and the data obtained, without considerable preparation and some expense, is small in individual cases. The most important tests of this kind were those carried out at Grain in 1918 with a C.E.1 machine. In these experiments the machine was first towed by a destroyer at

various speeds up to 20.2 knots, the speed resistance and running angle being measured. The machine was then run under its own power at steady speeds, ranging up to 25.0 knots, and the running angle and data *re* the clearing of the steps and commencement of planing obtained. A comparison between the measurements so obtained and the estimate from the model is given in Table 2.

TABLE 2.

COMPARISON OF MACHINE AND MODEL.
C.E.I. EXPERIMENTS.

Machine Speed. Knots.	Resistance Estimated. Water only.	Air.	(Lbs.) Total.	Thrust in lbs. Propeller calculated from revs. on taxi trials.	Actual measured pull. (Lbs.)	Wing Chord Angle in Degrees. From Model.	Measured.*
9.3	455	432	487	532	560	11.3	11.4
11.5	520	51	571	624	640	12.3	13.1
12.6	—	—	—	—	—	12.4	12.1
14.5	678	83	761	696	670	12.7	12.7
15.2	705	100	805	710	760	13.0	14.6
17.0	678	133	811	—	850	14.5	14.7
17.5	674	153	827	762	880	14.7	16.1
19.3	680	186	866	—	870	15.0	15.9
20.2	679	205	884	—	870	15.1	15.8
24.8	—	—	—	—	—	15.1	14.2

* In all trials but two the tow point was below the propeller centre, and the forward trimming effect of the tow was a little less than it should be. In the runs at 12.6 and 24.8 knots the machine was self-propelled and the trim a little less.

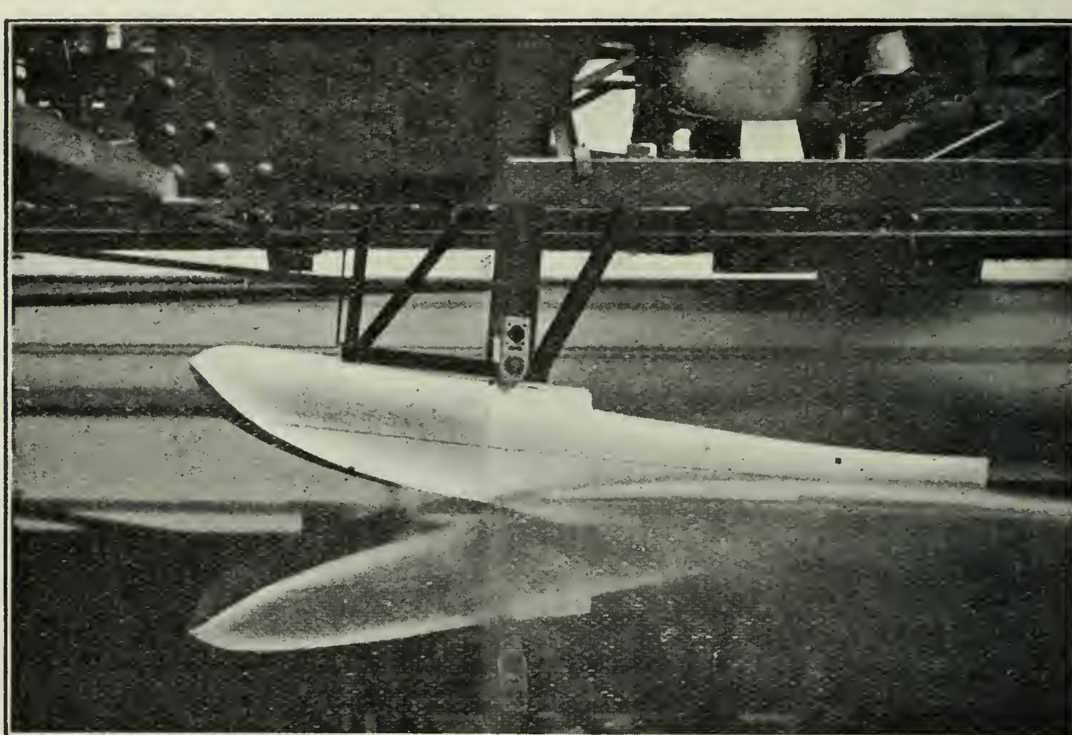
The agreement as regards resistance angle and planing was considered satisfactory over the speed range of the tests. A similarly close agreement between running angle and commencement of planing has been obtained with a P5 flying boat. Thanks to the facilities provided by the Supermarine Aviation Company, comparison of the stability on the water (both transverse and longitudinal) of their four-seater channel flying boat was rendered possible, and as far as could be seen there was general agreement. Speeds had to be taken on the machine's instruments which were stated to be fairly accurate. Predictions from the model tests of failure to fly have been confirmed by two machines built, and the predicted possibility of flying only in favourable conditions in one machine was also confirmed. But such confirmation is necessarily not quantitative, but general in its character.

The only discrepancies so far found are in running angle at high speeds. Both in the C.E.1 and in the P5 this angle was smaller by some 3 degrees than in the model. It is now recognised that this is due to the fact that the air structure has a large effect on the running angle at high speeds, and our cross curves at constant speed with varying angle are made to deal with this feature. It is not pretended that other discrepancies do not exist between model and machine performance. I am always anxious to obtain details of any trouble in a machine and, if possible, to trace it to its source. One's very real difficulty in such matters is to get the real facts separated from some observer's opinion. Most pilots' observations are reliable, but their conclusions as to causes necessarily require revision in the light of better technical knowledge.

Future Tests

§14. In the lecture so far the author has tried to show how the experiments in the past have grown in purpose and usefulness, and slowly taken certain more

or less standard shapes. It is to be assumed that in the future these methods will grow and change with the types of the machines, and one's endeavour in test work is to keep always a little ahead of such changes, so that the test data serve as a guide to the designer. We are beginning to regard data from two points of view, the small machine and the really big one, leaving intermediate ones for individual consideration. Ten years ago our first research paper dealt with the machines then in use of weight less than one ton. Our latest general research paper covered machines of the single hull type weighing 50 tons. Our other work has dealt with machines mostly of 2 to 5 tons, occasionally of 15 tons, generally of amphibian character, and these for the most part indicate the drift of design. Qualities which are easily embodied in a small machine become more difficult of attainment in the large, and in some cases more essential. The bigger the machine the greater is the accuracy required in the design, and in such machines model tests of all features are imperative. In the small ones, small



Flying boat hull (Supermarine Co. design) under test at speed 30 knots for machine.

errors in design are easily eliminated, often at less expense than is involved in making tank tests. But new types, especially of amphibian machines, should be tank tested. Special tests at high speeds are now made with these, for trimming when settling on the water, to determine the trimming effect of the wheels.

Whether the next developments will follow the two general lines of small amphibian and large flying boat cannot be said with any certainty. In the author's opinion the really large flying boat is coming. The direct trade routes from this country to Scandinavia, the ports of the Baltic Sea, Germany and to America are almost entirely by water, and all our Colonies are separated from us by great oceans. To carry a large number of passengers large machines are required, and any machine not capable of settling on the water would hardly come up to accepted standards of safety on these routes. Research on such lines should not be deferred until the machines are here, but should be well in advance of the present requirements and should be unrestricted in its general character.

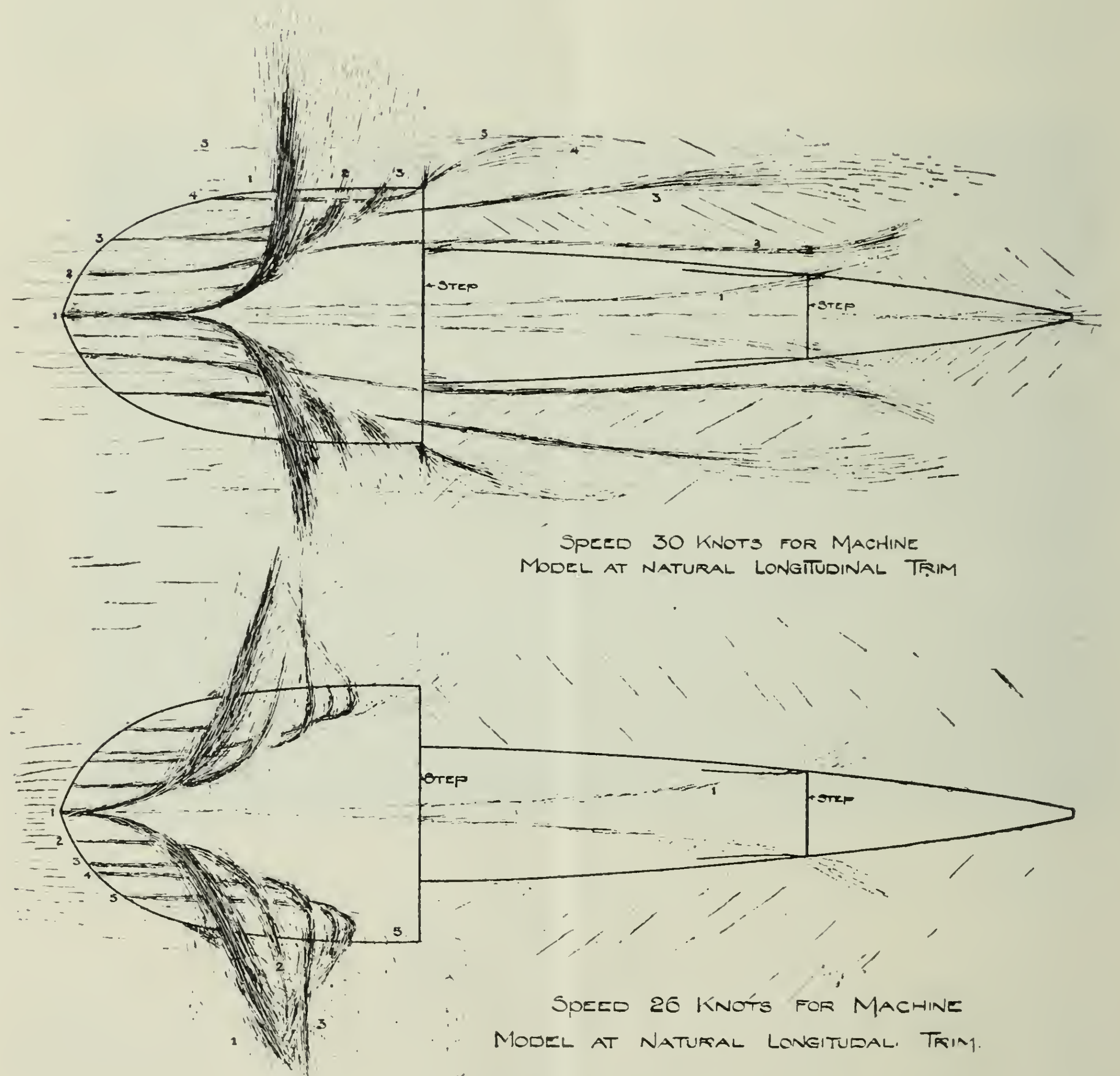
APPENDIX I.

PUBLISHED PAPERS GIVING RESULTS OF TESTS CARRIED OUT IN THE WILLIAM FROUDE NATIONAL TANK.

Date of Publication.	Subject Matter.	Form of Publication.
November, 1912	Plate results, early forms up to model 43. Air tests on 43b.	R. & M. 70
November, 1913	Twin floats, effect of tail angle, ventilation of step, gap at high speeds. Single float of great beam, with and without bastard tunnel.	R. & M. 98
July, 1914	Float type, general requirements, etc.	Trans. I.N.A.
July, 1914	Balloon shape floats, with and without steps.	R. & M. 113
March, 1915	Twin floats—effect of shape, chine, step flare, etc. These were the first tests with a complete set of floats and deals with trimming moment for first time. Longmore's report on 43b floats.	R. & M. 165
June, 1915	America type tests and some two-step flying boats. Effect of beam on resistance.	R. & M. 166
November, 1915	America type, effect of beam.	R. & M. 187
December, 1915	Transverse stability on water. Effect of cross section on same. Notes on wing floats.	R. & M. 188
January, 1916	Twin floats, comparison of types.	R. & M. 189
November, 1916	Flying boats with hooped sections. Effect of size for constant load. Effect of beam for constant load.	R. & M. New Series 300
November, 1917	Wave formation produced by a seaplane.	R. & M. New Series 365
April, 1918	Tests leading to C.E.1 design.	R. & M. New Series 412
March, 1918	Tests for stable form of float for launching air target.	R. & M. New Series 410
May, 1918	General summary of results for flying boats, deals with bending moments on hull for first time.	R. & M. New Series 437
September, 1918	N4 type, some notes on wing floats for same.	R. & M. New Series 472
December, 1918	Comparison of F5, Vickers' F5 and P5 forms for stability and resistance.	R. & M. New Series 483
March, 1919	Impact forces on hull when settling. Total force measured under varying conditions.	R. & M. New Series 583
January, 1920	Loading of flying boat hull, effect of beam and of angling up bow.	R. & M. New Series 655
February, 1920	Flying boats. General data on effect of dimensions, details of form for good results and some strength data.	Trans. N.E.C.I. Eng. & Ship.

EXPERIMENTS WITH FULL SIZE MACHINES.

September, 1918	Summary of early full-scale work. Tests of C.E.1 machine, for longitudinal stability, resistance and inclination.	R. & M. 473
September, 1920	Impact forces on hull when taxi-ing and settling, amount and locality.	R. & M. 683



GENERAL FLOW UNDER HULL OF FLYING BOAT.

The diagram shows the flow of coloured water under the hull, the streamline having the same number entering and leaving. It will be seen that the thick front edge of the water thrown out is produced first by the forefoot of the hull and aggravated if the water cannot easily flow under the lip of the chine at the step (see return of line 4 in lower fig.).

Discussion

Wing Commander T. R. Cave-Browne-Cave, who opened the discussion, said there could be no possible doubt as to the value of tank work. Not only were the actual tests carried out in the tank of very great value, but the knowledge which Mr. Baker and his staff had acquired in the course of those tests made them of extraordinary value when dealing with difficulties occurring in full-scale boats. There were one or two points he would like to raise, which were rather in the nature of requests for further information than criticisms of the work done or of the lecture. When Mr. Baker was comparing results of model and full-scale tests, he had referred to the forces necessary to accelerate machines and it did not appear in the model. He did not know whether there were any calculations that would account for that difference, whether they could use the mass of the machine, or whether there was a certain virtual mass which must also be accounted for. He believed the practical limitation of the flying boat was reached, not so much in its ability to get off the water under calm conditions, but under certain conditions of sea, of which he thought the worst was the long flat swell with very little wind. He had seen the apparatus available in the tank and could not help thinking that a great deal could be done if models were tested in such a long low swell, because the occasions on which a flying boat failed to function were much more frequent at the time of a long difficult swell than when the boat was loaded to the maximum and was trying to get off in a perfectly flat calm or, alternatively, when there was the bigger sea which was accompanied by a fairly strong wind. It would perhaps be well if the design of the boat were made specially suitable for that difficult condition rather than for the rather less difficult condition of a perfectly flat surface. He had been extraordinarily interested in the diagrams shown of the flow of water under the surface of the boat, and it would be valuable if Mr. Baker would add to the text of the paper a sufficient description to allow those diagrams to be published. As the size of flying boats increased, and as the power of the machines increased, one of the biggest difficulties was to accommodate the propeller or propellers. The difficulty was that the water was thrown up by the boat as it passed along and then broke up and passed into the propeller. The density of water was some 900 times the density of air, so that even a comparatively small quantity of water in the air meeting the propeller might very well put up the loading by ten or more times. Therefore the difficulty of the propeller was a very serious one indeed. Perhaps the most marked respect in which the model did not represent the full scale was in the way in which the water was thrown up. He quite believed the flow under the bottom of the model was the same as the flow under the bottom of the full size machine. The law of similarity established that without doubt, but he very much doubted whether the law of similarity held in the motion of this thin film of water through the air. There were present at the meeting two of the greatest authorities on aerodynamics, namely, the Chairman and Mr. Baker, and he would be glad if one or both would assist him in that respect. He believed that what happened was that the water was thrown off from the hull in the same way as it was thrown off by the model, but in the manner in which the water travelled after it was thrown off there seemed to be a large difference as between the model and the full-scale machine. The motion of the water after it was thrown off was a most serious matter, because that was responsible for breaking the propellers, and the avoidance of the tendency to break the propellers was a very serious restriction on design. He did not know whether it was possible to state the way in which the water travelled after it was thrown off. There was another, but quite different, point arising out of that. The Americans had employed motor boats, which he believed they referred to as sea sledges, which were provided with a V-bottom, but with the V upside down. Such a shape would appear to throw much less water to the sides and, consequently, less water on to the propellers, and if that shape worked out satisfactorily from the aero-

dynamic point of view it might have certain very valuable characteristics. The question of the seaworthiness of the flying boat was a most difficult one, and whether seaworthiness would improve as size increased was, of course, an important problem. Whether it was possible to investigate that with models he did not know, but he believed that the principal justification for the large flying boat was the hope that seaworthiness would improve. He did not think the performance went up very greatly with increase of size. There was one point on which he did not agree with the lecturer, although he admitted that he was prejudiced. Mr. Baker had predicted trans-oceanic journeys by flying boat, but he himself was going to be comfortable and travel by airship.

Sir Richard Glazebrook, K.C.B., F.R.S. (late Director, National Physical Laboratory), expressed his appreciation of the paper and called attention to the progress made since 1912, when the possibility of making tests on floats or seaplanes and so on in the tank was first spoken of. Mr. Baker had pointed out very clearly and very distinctly in the paper the method adopted in trying to secure that progress and the results which he had attained. Mr. Baker had also made it very clear that further research on these matters, as, indeed, on all matters connected with aeronautics, was very greatly wanted. In the forthcoming year it might be possible to put rather more funds than had been put hitherto at the disposal of those interested in working the tank, and already a small panel of experts had been appointed to help Mr. Baker and discuss with him the problems arising. He believed we could look forward to further progress and that we should be able to congratulate Mr. Baker again, as on this occasion, at a future period, on successfully carrying forward the work. He would like to direct special attention to that part of the paper in which Mr. Baker had pointed out the importance of getting forward with research work. Mr. Baker had said, "Research on such lines should not be deferred until the machines are here, but should be well in advance of the present requirements and should be unrestricted in its general character." We did want, said Sir Richard, to have research unrestricted in its general character in order to investigate the very difficult problems arising in seaplane work. There were many points on which possibly something might be done. We had learned much in connection with ordinary machines through careful investigation of the laws regulating the flow of air around the machines, stability in the air and so on, and we wanted the same kind of knowledge in regard to seaplanes, and in order to secure that it was necessary to continue the co-operation that there had been in the past between those working on the tank and those carrying out full-scale tests in the air.

Captain D. Nicholson said that the Society could congratulate itself on having a paper from Mr. Baker, who was not only the expert on tank experiments in this country but in every other country. Being himself a naval architect, he fully realised the difficulties Mr. Baker had to face. Referring to his experiences in connection with one of the early tanks on the Clyde, about twenty years ago, he said that at that time very many shipbuilders worked either by past experience or by rule of thumb, but when they had found that other shipbuilders were able to reduce the cost of boats by using the tank, the tank became most prominent. Many shipbuilders were now realising that the tank was of great value, not only from the point of view of the yacht, but from the point of view of the high speed motor boat. Mr. Baker had said that pilots' observations were very often correct and very good. He agreed that they were, but when they asked pilots to give some explanation as to how particular defects might be remedied, as a rule, he did not think it was fair to expect them, with the technical knowledge they had available, to say how it ought to be done. It should be left to technical experts to give the remedy. He agreed with Mr. Baker's suggestion that the larger flying boats were really coming. He did not know exactly how Mr. Baker would answer Commander Cave-Browne-Cave with regard to seaworthiness, but he him-

self believed there could be no doubt about seaworthiness improving as the boats increased in size. As a matter of fact, he had had a great deal of experience with hydroplanes and motor boats, and seaplanes on similar lines to flying boats had greatly increased in seaworthiness as they had increased in size. Mr. Baker had not mentioned the materials of which the models were made. So far as he knew, all the models of flying boats and seaplanes had been made of wood. He could not understand why they should not be made of wax, because if defects were found in the lines of the boat it took a great deal of time and was very difficult in some cases to alter the lines of a wooden model. If they were made of wax it would be much better, because little pieces could be taken off or added on much more easily. Again, he would like to know whether Mr. Baker had carried out tank tests with the model completely submerged. He believed the Americans had been doing that for the last two years. He was glad to see that the Air Ministry had been carrying out many more tests in the tank recently than they had done previously; they were beginning to appreciate that the tank was of some use, and he would like to see a small boat given to the tank so that experience could be given direct from the tank to the D. of R.

Colonel DE VILLAMIL asked if the longitudinal curve on the bottom of the boat was based on any principle; was it a mathematical curve or only a streamline, because they never knew whether a streamline was too much or too little? He was lately at the Model Exhibition, where a young fellow had a boat, at the bottom part of which he had made a parabola, and he had said that the parabola gave very much the best results. The parabola, of course, was a curve of uniform acceleration and also, he believed, of least resistance.

The CHAIRMAN laid emphasis on the need for facilities for full-scale work. Not only in regard to seaplanes, but in regard to every type of aircraft, he believed we could say that we were further ahead in our knowledge of models than on the full-scale machines. It was extremely necessary that a limited number of full-scale experiments should be carried out, although they were relatively expensive. The whole problem, looked at from first principles, was so complicated that we could not hope, in the near future at any rate, to solve it from that point of view. From a purely experimental point of view model experiments could be restricted in number and type, with a considerable saving in expense and time, if we knew roughly what happened on a full-scale machine. In nearly every case, not only in regard to seaplanes on the water, but in regard to all heavier-than-air craft in the air, as well as airships, it was found that the model approximated very closely to what happened on full-scale machines. Another point connected with cost was that he believed less than £500 per annum, on the average, had been spent on model tests, and yet Mr. Baker had said that it was already predicted that two types would be certain failures, and that a third would be a partial and almost complete failure. There was now something less than £30,000 per annum available for model work. The economy to be effected by conducting experiments with models first, particularly if we could get able experimenters such as Mr. Baker, with adequate facilities and, still more important, suitable conditions of working, *i.e.*, a chance to do what he could see was necessary was very great, but we in this country, and possibly also those in America, had reached the stage where the speed at which we could move forward depended to a large extent on getting further facilities for full-scale experiments. He wished our prospects in this country were as bright for the continuation of those experiments as they were for the continuation of model experiments. Commander Cave-Browne-Cave had mentioned the breaking up of the film of water at the side of the boat. He would not attempt a full reply at the moment, but it was extremely likely that the main cause of it was surface tension. If they assumed that it was surface tension the law of comparison for scale could be worked out, but he had not attempted to do it. He then called upon Mr. Baker to reply to the discussion.

Mr. BAKER, replying, first thanked the meeting for their kindly reception of his lecture. He had not set out to do quite what Commander Cave-Browne-Cave had apparently expected, namely, to give a lecture on the various features that ought to be embodied in the hull of a flying boat. His object was to give an account of what had been done in the tank and how they had set about doing it, the experimental differences between model and full-scale work, and to throw what light he could on the effect of those differences. For example, with regard to the acceleration force, Commander Cave-Browne-Cave had asked if he (Mr. Baker) knew what mass to use in working out the force which was equivalent to the acceleration. He could only say that he did not know the virtual mass and he had never seen it worked out by anyone. With regard to the effect of a long flat swell, he believed that problem could be tackled at least mathematically, and that he and his colleagues could tackle it experimentally with the aid of the wave-making apparatus they had at the tank. They could make, in the tank, such a long low swell as that to which Commander Cave-Browne-Cave had referred and could run models in it, but he considered this was a problem which could be treated in the same way that Mr. Kent* had recently tackled the question with regard to the pitching of ships, *i.e.*, from the mathematical point of view first. He would have much pleasure in adding one more diagram to the paper if the Society wished, showing the flow of water underneath the bottom of the boat, together with a few notes on the subject to explain what was shown, and possibly to go a little further than that by giving some general idea of what it meant in regard to the design of the hull. With regard to the spray thrown up in the model and in the machine, there were two factors in this, namely, surface tension, as mentioned by the Chairman, and the wind. In the tank, of course, there was no wind at all, but the machines invariably sailed into the wind, which would blow to pieces the thin film which was thrown up by the model and leave the thick front edge of the wave, and its rear portion where the wave hits the water practically unaltered. That solid piece of water, so far as he knew, would be the same in model and full size; there was no reason why it should not be so. The difference would come about when it got thin enough to be affected by surface tension or wind. Commander Cave-Browne-Cave had said that he held the opinion that the spray thrown up by the model was not representative of the spray thrown up by the machine, but he had not given any actual facts. He (Mr. Baker) had been on a number of machines and had watched this—it had not been possible to measure—and he had never seen any material difference. He had watched when the step was clear, the speed at which it occurred, and had observed the same thing on the model, and it was the same. The angle of the model and the machine was the same, and until someone produced definite data to show that the two things differed, he preferred to hold the opinion he had expressed, which was based, at any rate, on theoretical reasons. Commander Cave-Browne-Cave had also mentioned the American sea sledges, but the Americans were not the first with that type of boat. He had seen something like them 15 years ago, working at Spithead. They were not called sea sledges then, but the "Viper" type of motor boat. They had the hollow underneath in the centre of the hull, as in the case of the American boats. Experiments were being conducted on that type of machine at the present moment, and there was no doubt that they were cleaner with regard to the wing of water which was thrown out by the ordinary type of flying boat hull. Beyond that he did not think it would be very wise to say much at the moment. The experiments would be extended to see whether it was the proper type of hull to put on a flying boat with safety to the people in it. As to the remarks of Sir Richard Glazebrook, nothing had pleased him (Mr. Baker) more than when he had heard that there was to be a Seaplane Panel of the Advisory Committee for Aeronautics. He hoped that through that panel we should be able to make definite progress with the problems that required to be

* T. Inst. N.A., 1922.

tackled, and that that progress would be continuous, so that people could be kept continually at work on research. The last twelve months had been rather painful in that respect; in the first six months everybody had had to be taken off the work and new work found for them, and that sort of thing did not make for efficiency or for the furtherance of our knowledge of seaplanes. It was unnecessary to say that more research was necessary, but research could not be carried on without money; expert researchers were necessary, which meant a continuous flow of money, and that was all that was required so far as he and his colleagues were concerned to carry on with their work. On the other side of it, the comparison of their work with the full-scale machines needed the co-operation of the flying stations. They were fortunate in having had, first, Commander Bustead, and secondly, Commander Cave-Browne-Cave, at Grain, and more recently Commander Cave-Browne-Cave had on his staff Mr. Evans, who, he felt sure, would help a great deal on the technical side, in comparing full-scale work with model work. He hoped, therefore, to see something more like continuous co-operation instead of the necessarily spasmodic co-operation that had existed hitherto. Captain Nicholson had asked why the models were made of wood and not wax, owing to the fact that the latter could be altered more easily. The wood was lighter than the wax if the models were solid, and it also stood the comparatively hard knocking about which the model received during test. They could take its weight off the water on pulleys, which had practically no friction, and except that the rather heavy weight of the wood was a definite defect as regards inertia, and particularly in the porpoising problem, he saw no reason to change. Completely submerged models had been run in the tank on two or three occasions, and they had compared the results with the results of models in air. Dr. Stanton had also carried out experiments in the tank on the same subject with airship forms, and the results were reasonably alike and made one think it was fairly certain that when we got a direct and proper comparison in that way they would find the results alike. He hoped to say more about it twelve months hence, after Mr. Southwell and himself had completed an investigation which had been commenced, using the same apparatus for the comparison of models in the water and in the air channel. Colonel de Villamil had asked whether the longitudinal curve given to the bottom of the boat was based on any mathematical formula. At one time he had produced a formula for getting the curvature of the bottom of the float, but like most other sensible people he had let it go and went for what experiments had shown was necessary rather than for what mathematics had shown might be necessary. He knew of no one at present who drew out the forms of flying boats from mathematical formulæ. A similar formula did exist for ship shape forms and was used in America occasionally, but he did not think it was ever used in England. Coming back to the need for experiments on full-size machines, in order to check and guide us in regard to what was being done with models, nobody knew more than he did how difficult those full-scale experiments were to carry out, but that it was really necessary that they should be carried out was a foregone conclusion. It was necessary to have a knowledge of the general behaviour of a machine when planning out experiments on the model, and when they were working them out in detail they must be able occasionally to compare the results with those obtained on the full-size machine.

A vote of thanks was accorded the author at the conclusion of the discussion.

LEONARDO DA VINCI AS A PIONEER OF AVIATION

BY IVOR B. HART, B.SC., ASSOCIATE FELLOW.

Introduction

Leonardo da Vinci lived in the fifteenth and early sixteenth centuries (1452-1519). He was the natural son of Ser Piero da Vinci, a Florentine notary, and at the age of fourteen was apprenticed to Andrea Verrocchio, a famous artist of those days. Verrocchio's tastes, and as a consequence his circle of acquaintances, were wide, and from all these Leonardo imbibed and developed a passion for scientific inquiry side by side with his development as an artist. At Florence in his early days he came under the influence of such men of science as Benedetto del'Abbaco, Giovanni Agiropulo, L. B. Alberti and Toscanelli.

In 1483 Leonardo migrated to Milan, where he took service under Ludovico Sforza in the capacity of consulting engineer, architect and sculptor, and he was busily employed in all these capacities. His chief scientific friendship during this period was with Fra Luca Pacioli, the famous mathematician.

Leonardo's stay in Milan ended in 1499 with the collapse of the power of Ludovico Sforza, and for some years we find da Vinci back again at Florence. In 1506, however, he accepted an invitation from Louis XII. of France to return to Milan. He remained there till 1512, and later, in 1515, Francis I. of France, Louis XII.'s successor, invited him to take up his residence in the Castle of St. Cloud, near Amboise. Here he spent the remainder of his days. He died on May 2nd, 1519.

His chief writings on the subject of flight are to be found in the following three publications:—

1. "Manoscritti di Leonardo da Vinci—Codice sul Volo Degli Uccelli, e Varie Altre Materie."—Publicato da Teodoro Sabachnikoff—Paris, 1893.
2. "Les Manuscrits de Leonardo da Vinci. Les Manuscrits B.F.K. & L. de la Bibliotheque de l'Institut de France."—Par M. Charles Ravaisson-Mollien—Paris, 1883 to 1893.
3. "Il Codice Atlantico di Leonardo da Vinci nella Bibliotheca Ambrosiano di Milano reprodotto e publicato dalla regia Academia dei Lincei sotto gli auspici e col sussidio del Re e del Governo."—Giovanni Piumati.

I *Flight Before Leonardo*

Aeronautics is a very young science. From the point of view of the achievement of "heavier-than-air" flight, it is but little more than a quarter of a century since the late Professor Langley, secretary to the Smithsonian Institution at Washington, successfully flew his *model* aeroplane (he called it an "aerodrome" and it measured 12 feet from tip to tip, weighed 30lbs., and carried a steam engine and boiler weighing 7lbs.) for half-mile distances over the river Potomac. The brothers Wright achieved the first motor-driven, *man-carrying* flight in an aeroplane for a period lasting but a portion of a minute, as recently as December, 1903. Yet young as is aeronautics as a science, as an article of faith and a philosopher's dream we may claim for it the age of many centuries. Aeronautics has in fact a history, and although it is inevitable that we must accord to this history a much longer period of legend than obtains with most of the sciences, it is very real nevertheless.

Among those who have contributed effectively to the sum of ideas concerning flight no figure is more striking than Leonardo da Vinci, painter, sculptor, architect, engineer, savant, mathematician and philosopher. Leonardo, who died in 1519, has this much at least in common with the science of aeronautics—that he has only recently come into his own. Neglected for nearly three hundred years, we find the first serious discussion of his scientific manuscripts in 1797, with J. B. Venturi's "*Essai sur les ouvrages physico-mathematique de Leonardo da Vinci*," whilst the main flood of publications both on his manuscripts and on their value in the light of modern science is a matter of living memory.

Dreamers of air conquest pass before us throughout the panorama of the early history of civilisation. We see them in the winged statuary of the Egyptians and we read of them in stories of ancient Greek mythology. Ever since man has learnt to think the challenge of the air has tantalised him. He has ever regarded as an anomaly and an incongruity that the ability to fly should be granted to birds and yet denied to him. And so we read of such legendary figures as Icarus and Perseus and Hermes, and of such entertaining but improbable stories as of Simon Magus who, with the aid of some demon colleagues, essayed a short-lived flight and finished with a broken neck; of Abaris (as related by Diodorus of Sicily) and his flight round the world on a golden arrow (somewhat reminiscent of old Mother Shipton and of the nursery pictures of the cow jumping over the moon); and of the story of Aulus Gallius in his "Attic Nights"; of Archytas and his mechanical pigeon of wood. "To wit, it was thus suspended by balancing and was animated by an occult and enclosed aura of spirit."*

Later we have the more circumstantial story of the Saracen of Constantinople who, in the presence of both the Emperor Comnenus and the Sultan of the Turks and a vast concourse of people, attempted to fly round the hippodrome at Constantinople. Wearing a long white robe braced with rods with which to catch the breeze, he took his station at the top of a tower and leaned into the wind. But "the weight of his body having more power to drag him down than the artificial wings had to sustain him, he broke his bones, and his evil plight was such that he did not long survive."†

A like fate befel a similar attempt in the year 1065 by Oliver Malmesbury.‡ Passing on, we come to Roger Bacon's classical thirteenth-century prophecy that "an instrument may be made to fly withall if one sit in the midst of the instrument, and do turn an engine, by which the wings, being artificially composed, may beat the ayre after the manner of a flying bird. . . ." No more references to flight are to be found until we reach the fifteenth century. Here we come to records concerning the famous astronomer Regiomontanus. We are told of him that in his workshop at Nuremburg was an automaton in "perpetual" motion, and that he made an artificial "fly" which, "taking its flight from his hand, would fly round the room," and at last, as if weary, would return to his master's hand, and that he fabricated an eagle which, on the Emperor's approach to the city, he sent out, high in the air, a great way to meet him, and that it kept him company to the gates of the city. Shorn of all the inevitable additions of credulous retailers of the facts, the probability is that Regiomontanus, who was of a mechanical turn of mind, fashioned a clockwork contrivance which, more by luck possibly than by design, acted as a glider when released in his workroom. Finally, there are records of experiments by an obscure contemporary of Leonardo, one Giovanni Baptista Danti, who essayed a flight at Perugia towards the close of the fifteenth century by means of a contrivance of wings which worked "with a horrible hissing sound." It was perhaps an imitation of one of the machines which Leonardo himself designed.

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* Aulus Gallius, "Noctes Atticæ," Lib. X., Cap. XII.

† "Histoire de Constantinople," Cousin, Paris.

‡ "Historiæ Anglicanæ Scriptores," X., Twysden.

2 Da Vinci and His Problem of Flight

Leonardo da Vinci was born in 1452 and died in 1519. It is therefore obvious from what we have said that in attempting to tackle seriously the problem of flight he owed next to nothing either to his contemporaries or to those who lived before his day. Careful study of the manuscripts left by Leonardo make it clear to all fair-minded students that, in the truest sense of the term, da Vinci was not only a real pioneer of the science of flight, but he was also the first pioneer. With all his many interests and activities he yet gave, at intervals during thirty years of his life, a close consideration of the problem. It fascinated and held him. His ambition appears to have been to achieve a flight from the summit of Monte Ceceri (the name of a bird), situated a little to the north and east of Fiesole. No doubt the name appealed to the artist in him as being peculiarly appropriate. "From the mountain which bears the name of the great bird, the famous bird will take its flight, and will fill the world with its great fame,"* he writes; and again, "The great bird"—he frequently refers to his conception of a flying machine as a "bird"—"will take its first flight on the back of the great bird, and filling the world with stupor and all writings with renown and bringing glory to the nest where it was born."†

These are extravagant words which carry their own significance as to the frame of mind of him who penned them, but, coming as they do from one whom we know to have been imbued with the scientific spirit, they have an added importance. It is not surprising, therefore, that we meet with his discussions on flight both as early as the year 1490, during his residence at Milan,‡ and as late as 1514, whilst at Rome.§ His main contribution to the subject, the note-book "Sul Volo degli Uccelli" (on the flight of birds) was written at Florence in 1505.

There were certain fundamental ideas which clearly guided da Vinci through the whole course of his investigations. Of these the most important was the time-old view that the imitation of the bird was the right line to adopt and that its study would reveal the true secrets of flight. "A bird," writes he, "is an instrument working according to mathematical law, an instrument which it is within the capacity of man to reproduce with all its movements, though not with a corresponding degree of strength, for it is deficient in the power of maintaining equilibrium. We may therefore say that such an instrument constructed by man is lacking in nothing except the life of the bird, and this life must needs be supplied from that of man."||

He had no illusions as to the comparison between a bird using its own living members and accessories and a man using wings and accessories which have no life in themselves. "The life which resides in the birds' members will, without doubt, better conform to their needs than will that of man which is separated from them, and especially in the almost imperceptible movements which preserve equilibrium. But since we see that the bird is equipped for many obvious varieties of movements, we are able from this experience to deduce that the most rudimentary of understanding and that he will to a great extent be able to provide against the destruction of that instrument of which he has himself become the living principle and the propeller."¶

Another fundamentally sound principle laid down by Leonardo should also be noted, namely, the need for study of the medium in which flight is carried out. "To attain to the true science of the movement of birds in the air it is necessary

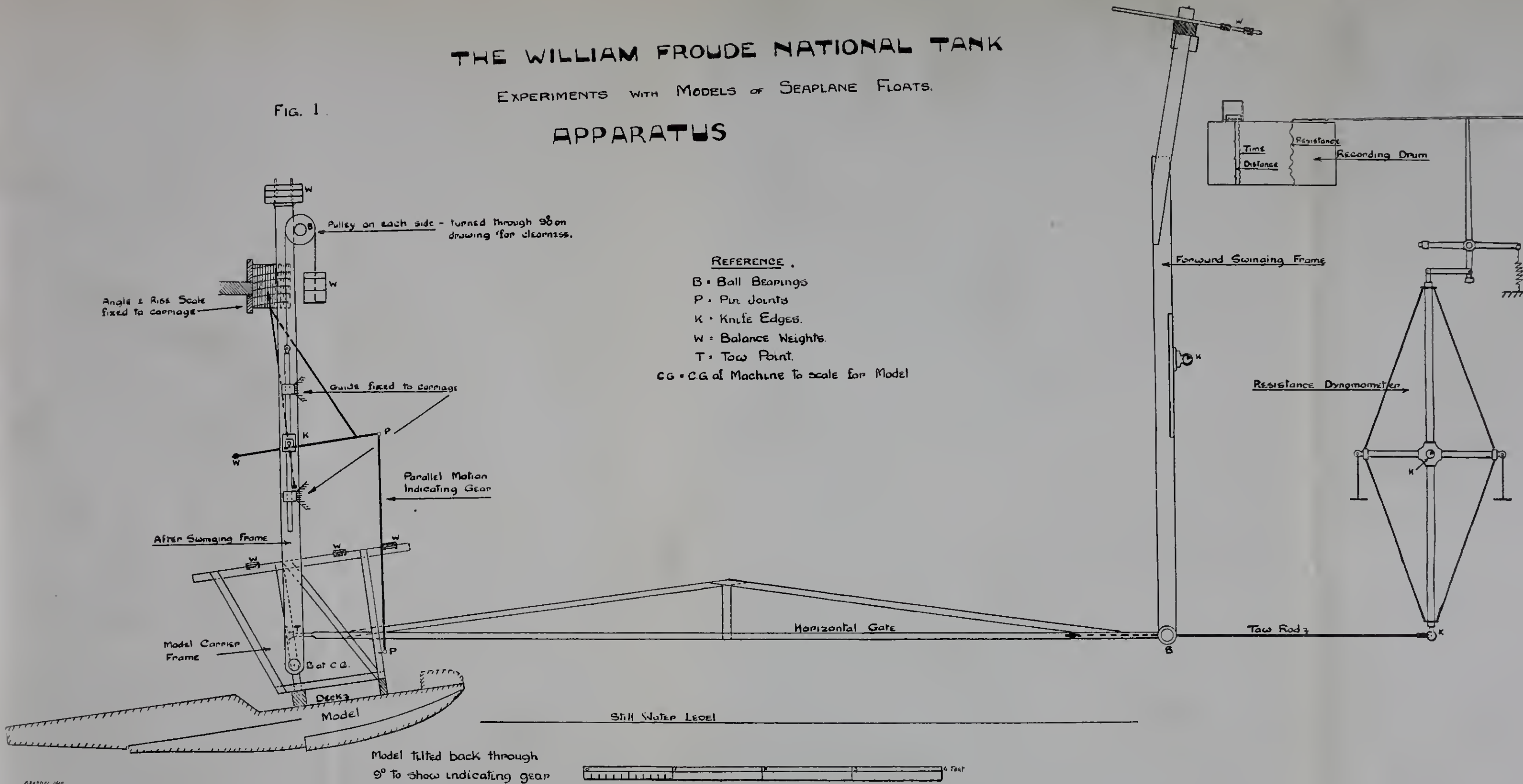
* "Sul Volo degli Uccelli," folio 18, verso.
† "Sul Volo degli Uccelli," inside back cover.
‡ MSS. B of the Collection at the Institute of France.
§ MSS. E of the Collection at the Institute of France.
|| "Codex Atlanticus," 161, r.a.
¶ C.A., folio 161 r.a

THE WILLIAM FROUDE NATIONAL TANK

EXPERIMENTS WITH MODELS OF SEAPLANE FLOATS.

FIG. 1.

APPARATUS



Model tilted back through 9° to show indicating gear

to give first the science of the winds, which we will establish by means of the movements of water. This science will be a degree (means) of arriving at the knowledge of the winged creatures in the air and in the wind."*

Was da Vinci attempting the impossible? He lived centuries before the days of mechanical power. The problem before him was, therefore, the attachment to the human frame of contrivances to be worked by muscular energy only, in conjunction with such natural assistance as could be derived with understanding from air currents. The history of flight before the days of steam, oil and gas engines includes many similar attempts after Leonardo's times, all unsuccessful, and even up to a year or two ago we should perhaps have dismissed the idea as incapable of achievement. But the time has now come to abandon this attitude. That which Leonardo sought to achieve over four hundred years ago, and for which many an intrepid adventurer has since laid down his life, has to some extent been accomplished.

To-day aeroplane flights in machines without mechanical power, on the glider principle, have been successfully carried out. One of the consequences of the Treaty of Versailles was that Germany was forbidden to build aeroplanes fitted with engines. German inventors were thus driven to the old problem of pre-engine days, and with that superior knowledge of the principles of flight which was of necessity denied to Leonardo da Vinci, they have found a solution. This solution is not of the type for which da Vinci worked. There is no attachment to the human frame of a scheme of mechanical wings to be operated by muscular energy. It has been rather a case of a modern type of aeroplane without an engine, deriving its power from a stiffening breeze, and operated by a man, and to some extent capable of both manœuvre and control. To this extent the problem that Leonardo set himself has actually been solved.

It is important to notice that throughout Leonardo faces his problem in a true scientific spirit. He looks to those beings who do fly, he studies them carefully, their physiology, their anatomy, the medium in which they exist; he studies their movements through a vast variety of conditions. To every effect which he observes he looks for a cause, and to that cause he applies modifying conditions. He seeks and makes deductions, always with a view to obtaining control.

There are two very interesting contributions to the general subject of air conquest for which we are indebted to da Vinci that are not only important in themselves, but serve also to show another aspect of the real man of science in the object of our study. The first of these is Leonardo's invention of the parachute. In the "Codex Atlanticus" we read, "An object offers as much resistance to the air as the air does to the object." (Note here a fifteenth century statement of the law of reactions.) "You may see that the beating of its wings against the air supports a heavy eagle in the highest and rarest atmosphere, close to the sphere of elemental fire.† Again you may see the air in motion over the sea fill the swelling sails and drive heavily-laden ships. From these instances and the reasons given, a man with wings large enough and duly connected might learn to overcome the resistance of the air, and by conquering it, succeed in subjugating it and rising above it."‡ Here, clearly, we have the principle of the parachute, and Leonardo includes in his manuscript the figure here shown (Fig. 1) together with the accompanying explanation: "If a man have a tent roof of calked linen 12 braccia broad (roughly a bracci equals a yard) and twelve braccia high, he will be able to let himself fall from any great height without danger to himself."

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* MSS. E, folio 54, r.

† Referring, of course, to the fifteenth century conception of the outer universe.

‡ "Codex Atlanticus," folio 372.

There is evidence that nearly a century (c. 1595) after da Vinci's invention and probably as a direct result of it, Veranzio, a Venetian, modified the original design, using a sort of square sail extended by four rods of equal size and having four cords attached at the corners.* Nevertheless, the first real descent in a parachute was not made till 1783, when Lenormand carried out a successful experiment from an observatory at Montpellier.

The other contribution is da Vinci's virtual discovery of the "lighter-than-air" principle. Leonardo was definitely familiar with the decreasing density of the atmosphere with altitude. "The air," he remarks, "has greater density when it is nearer to water and greater rarity when it is nearer to the cold region, and midway between these it is purer."† Further, he knew of the principle of the fire balloon. Vasari tells us‡ that he made figures of thin wax of strange

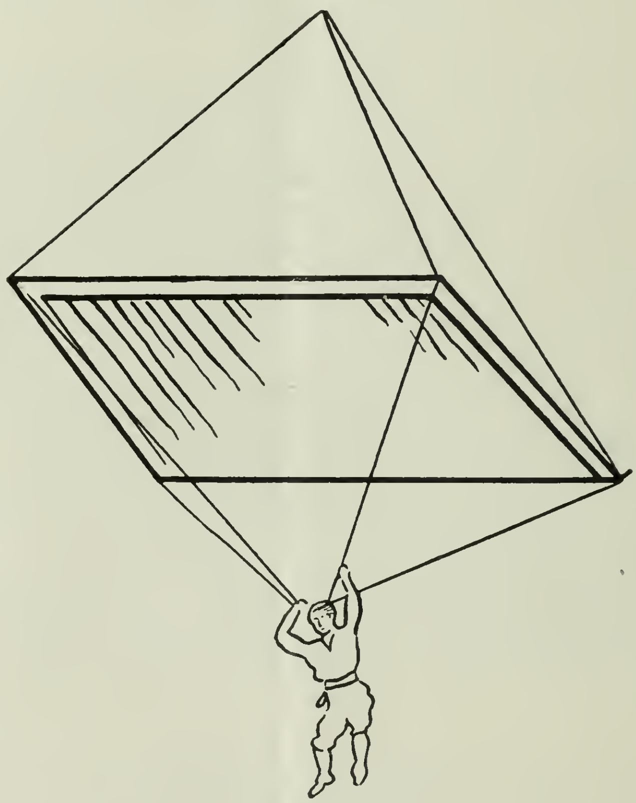

Fig. 1

shapes and filled them with warm air, causing them to "fly" through the air to the great surprise of onlookers. Why then was Leonardo not diverted to the study of balloons and aerostatics? So much simpler is this than the sister study of aerodynamics that as soon as hydrogen was discovered the science of ballooning monopolised the whole attention of students of aeronautics, to the detriment of heavier-than-air design. Having regard to da Vinci's intellect, his practical bent and his opportunities, he could undoubtedly have made the fire balloon and the parachute the starting point of a very successful venture in the field of aerostatics. But this had no appeal for him. In a balloon there is neither life nor control. In the "bird" of da Vinci's imagination there was both, and with this goal clear in his mind there was no room for diversion. We have here a clear instance of the influence of scientific purpose. One less imbued with the true scientific

* "A History of Aeronautics," Vivian, London, 1921.
† "Codex Atlanticus," folio, 161, r.a.
‡ "Lives of the Painters," Vasari, Venice, 1550.

spirit would have lost sight of the original problem in the joy of a new discovery off the beaten track. That this was not so with Leonardo is a fact which must affect our estimate of his scientific worth.

3 *Da Vinci and the Flight of Birds*

Let us now turn to some extracts from da Vinci's note-books dealing with the flight of birds. Leonardo was very clear as to the need for a proper scheme of approach to his problem. In another manuscript we read*: " I have divided the treatise on birds into four books; the first treats of their flight by beating their wings; the second treats of flight without beating their wings and with the help of the wind; the third treats of flight in general, such as that of birds, bats, fishes, animals and insects; the last of the mechanism of this movement." Again there occurs the following passage:—" Whether birds when continually descending without beating their wings will proceed a greater distance in one sustained curve. . . . Whether when they wish to pass in flight from one spot to another they will go more quickly by making impetuous, headlong movements, then rising up with reactive movement and again making a fresh descent, and

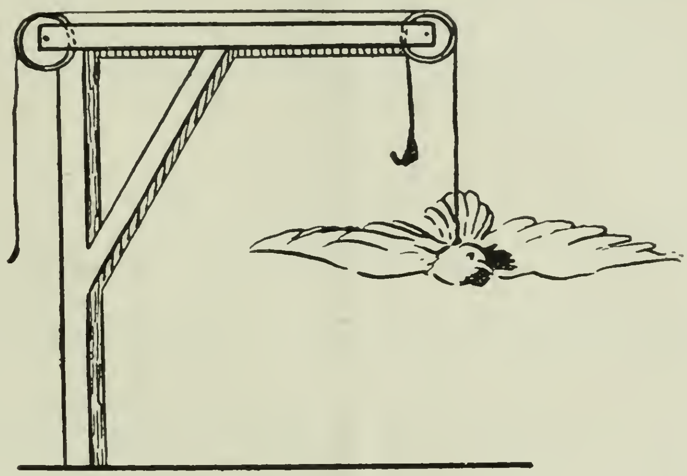

Fig. 2

so continuing. To speak of this subject you must explain in the first book the nature of the resistance of the air, in the second the anatomy of the bird and of its wings, in the third the method of working of the wings in their various movements, in the fourth the power of the wings and of the tail at such time as the wings are not being moved and the wind is favourable, to serve as a guide in different movements."†

Leonardo fully recognised the rôle of centres of gravity as being fundamental in aeronautics, as in technics generally. At the outset we read in his " Flight of Birds " that " mechanical science is very noble and useful beyond all others, for by its means all animated bodies which have movement perform their operations; which movement proceeds from their centre of gravity. This is situated at the centre, except with unequal (distribution of) weight."‡ Later in the same note-book§ we meet with a most interesting little sketch of a " bird " suspended from a bracket by means of pulleys (Fig. 2), together with the note: " This is done to find the centre of gravity of the bird without which this instrument would have but little value."

* MS. K, folio 3, r.
† MS. F, folio 41, v.
‡ " Sul Volo degli Uccelli," folio 3, r.
§ Ibid., folio 16 (15), v.

Da Vinci next interests himself in the problems of wind pressure. For a bird to be sustained in the air during horizontal flight there must be an upward pressure over a moving wing surface. Accordingly he asks, "In which part of the under-surface of the width of the bird does the wing press the air more

Fig. 3

than in any other part of the length of the wings?* (See Fig. 3.) Developing his theme further, he continues: "All bodies which do not bend, whatever their size or weight, exert equal pressures on all the supports that are equi-distant from the centre of gravity, this centre being at the middle of the substance of such a body." He then considers the case of a stiff surface loaded at equal intervals with equal weights and next the case of a flexible body or surface loaded in various ways, always bearing in mind the point that "the heavier portion always guides the movement."

How is the necessary "wind pressure" required to support the bird's weight during flight to be obtained? Da Vinci finds his answer in the beating of the wings, and he studies with care the local variations in pressure in the neighbourhood of the wing surface caused by the "beating" movements. "The properties of the air are such that it may become condensed or rarified," says he. He realises clearly that support must come from air condensation below the wing. "Unless the movement of the wing which presses the air is swifter than the movement of the air so pressed, the air will not become condensed beneath the wing, and in consequence the bird will not support itself above the air. . . .

"That part of the air which is nearest to the wing which presses on it will have the greatest density."† He makes a similar point elsewhere in his notes. "When the bird shall be in the position *a n c* (Fig. 4) and shall wish to rise, it

Fig. 4

will elevate the shoulders *m* and *o*, and will thus be in the position *b m n o d*, and the air will be pressed between the sides and the point of the wings so that it will be condensed and cause an upward movement and give rise to an impetus in the air, which impetus of the air will push the bird upwards by its condensation."‡ He further develops the function of the beating of wings in this connection as follows: "The simple movement possessed by the wings of birds is

* "Sul Volo degli Ucœlli," folio 4, v.
† "Codex Atlanticus," folio 161, r.a.
‡ "Sul Volo degli Uccelli," folio 13 (12), r.

easier, as they are raised and lowered. This ease of movement is born of two causes, of which the first is that the lowering weight raises up the wings a little by themselves; the second is that the wings being convex above and concave below, the air escapes more easily the percussion of the wing with elevation than with lowering since the air in the concavity of the wing engenders more easily its condensation than its escape."* In this connection it is almost startling to notice that even that essentially modern subject of "streamlines" attracted Leonardo's penetrating attention. "That part of the air nearest the wing," he writes, "will most resemble in its movement the movement of the wing which presses on it; and that part will be more stable which is further from the said wing. That part of the air which is the nearest to the wing which presses on it will have the greatest density."†

One of the most important aspects of the "heavier-than-air" theory is that concerned with the relationship between centre of gravity and centre of pressure. In any given machine the centre of gravity is a fixed point, but the centre of pressure being dependent upon a number of varying factors, changes with the conditions of the flight. It shifts with the varying angles formed by the wing surfaces to the direction of the air opposing it, and its position can be calculated for any given position, and for any given amount of wing surface from the ratio of what are termed the lift coefficient and drag coefficient respectively (in effect two components of the resultant air pressure upon the given surface) and the turning moment about some selected point. In the modern machine, longitudinal stability is assured by having the centre of gravity suitably placed and coincident with the centre of pressure and the thrust of the propeller. Obviously, however, a shift in the centre of pressure from this point will produce a "turning moment" about the new position of the point, and will disturb the equilibrium of the machine. Such changes constantly occur during flight and are necessary to effective manœuvre, but so long as they are *controlled* changes, they are all to the good.

This important factor of the centre of pressure, or centre of resistance as we may alternatively call it, was known to da Vinci, though naturally in the absence of exact mathematical knowledge he could deal with it only in a very general way. Nevertheless, his notes indicate remarkable accuracy of observation combined with scientific intuition. "When a bird, which is in equilibrium, throws the centre of resistance of the wings behind the centre of gravity, then it will descend with its head downwards. A bird which finds itself in equilibrium will have the centre of resistance of the wings more forward than the bird's centre of gravity; and such a bird will fall with its tail turned towards the earth."‡ Looking at the matter from the point of view of manœuvre, we have the following passages: "When the bird sinks, then the centre of gravity is outside the centre of its resistance; as if the centre of gravity were on the line *a b* (Fig. 5) and the centre of resistance on the line *c d*. And if the bird wishes to rise, then the centre of its gravity remains behind the centre of its resistance. As if the centre of gravity mentioned might be in *f g*, the centre of resistance would be *e h*"§ (Fig. 6). We quote yet once more from the same note-book. "The descent of a bird will be always by that end nearer to its centre of gravity. The heavier part of a bird descending will always be in front of its centre of pressure. When, without the assistance of the wind and without beating its wings, the bird remains in the air in the position of equilibrium, this shows that the centre of gravity is coincident with the centre of pressure."||

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* MS. E, folio 39, r.

† "Codex Atlanticus," folio 161, r.a.

‡ "Sul Volo degli Uccelli," folio 8 (7), v.

§ Ibid., folio 16 (15), v.

|| Ibid., folio 8 (7), v.

4 *The Function of the Tail*

The function of the tail comes in for Leonardo's consideration and gives rise to very important conclusions. The subject affords particular interest from the point of view of scientific method. Leonardo first carries out close observations on the various uses to which a bird appears to put its tail. He makes deductions as to the functions of the tail, and then proceeds to design models in illustration of these functions. Nothing could be more sound or more essentially modern than the scientific outlook involved, quite apart from the truth or otherwise of the conclusions arrived at. It is unfortunate that comparison with the corresponding functions in the modern aeroplane is difficult, but it must be remembered that whilst in the case of the bird there is merely a tail, in the case of the modern aeroplane we have tailplane, rudder and fin, each with their separate functions. On the other hand, we must also bear in mind the interesting physiological fact that the bird, by virtue of its internal structure, can so control its air sacs as to be able at will to shift its centre of gravity within certain limits. This undoubtedly provides the bird with a mechanism for ensuring stability and control that is denied to the artificial machine.

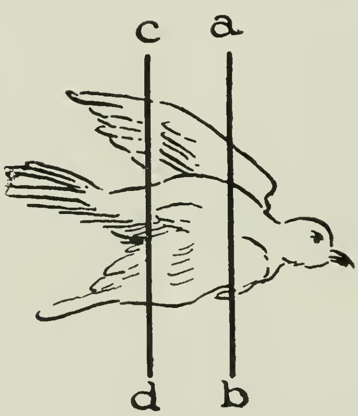

Fig. 5

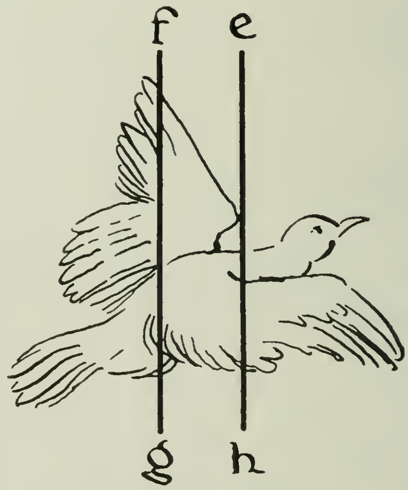

Fig. 6

We will first quote passages illustrative of da Vinci's general observations on the use of the tail in birds, *e.g.*: "The opening and lowering of the tail and the simultaneous spreading out of the wings to their full extent arrests the swift movement of birds. When birds, in descending, approach the ground and the head is below the tail, they lower the tail, spread it wide open, and take short strokes with the wings; as a consequence the head becomes higher than the tail and the speed is checked so that the bird alights on the ground without any shock."—Note here, in passing, the excellent treatment of the subject of landing, which finds so delightful a counterpart in the corresponding manœuvre in modern aviation practice.—"In all changes which birds make in their lines of movement, they spread out their tails."* On the next page of the same note-book we read, "The speed of birds is checked by the opening and spreading out of the tail."† Further observations are recorded in the "Codex Atlanticus." "Beneath the wings the down and feathers are plentiful, and at the ends of the wings and tail the tips of the feathers are flexible or capable of being bent, whilst those on the front of the wings, where it strikes the air, are firm."‡ And again, "Beginnings

* MS. L, folio 58, v.

† Ibid., folio 59, v.

‡ "Codex Atlanticus," folio 308, r.b.

of things are oft the cause of great results. Thus we may see a small almost imperceptible movement of the rudder turn a ship of marvellous size and loaded with a very heavy cargo, and that, too, amid such a weight of water as presses on its every beam, and in the teeth of the impetuous winds enveloping its mighty sails. So in those birds which can support themselves above the course of the winds without beating their wings, a slight movement of wing or tail, serving them to enter either below or above the wind, suffices to prevent their fall."*

We turn next to examples illustrative of da Vinci's combination of observation with deduction. In the note-book "Sul volo degli Uccelli" we read†: "The second shaft in the opposite portion, beyond the centre of gravity of the bird, and that is the tail, which, if it is struck by the wind underneath, since it is beyond the said centre it will cause the forward portion of the bird to lower. And if this tail be struck on top the forward portion of the bird will rise. And if this tail twist a little and turn its under surface obliquely to the right wing the anterior portion of the bird is turned to the right side. And if it turn this side obliquity of the undersurface of the tail to the left wing, it (the bird) will be turned with its forward portion toward the left side, and in each of the two manners the bird will sink.

"But if the tail, placed obliquely, be struck by the wind on its upper surface, the bird will be turned, in turning it (the tail) slowly from this side where the superior surface of the tail shows its obliquity."

Again, in manuscript K‡ occurs the following passage: "When the bird rises up by assistance of the wind without beating its wings, it spreads out and raises its wings so that they form an arch with concave side towards the sky and it receives the wind under its wings continuously, in its movements to and fro, and this would cause it to turn right over if it were not that the point of its tail is turned to the wind as it enters beneath the wind; and this afterwards by its power of resistance acts to prevent the said movement of turning over, because the wings are restrained by the tail in such a way that their various parts are of equal power, and so the tail becomes partly lowered and the bird is raised forward slightly." Perhaps one of the most interesting passages of this type is the following, which shows clearly da Vinci's interpretation of the functions of the tail with reference to that consideration of the shifting of the centre of pressure to which we have already drawn attention: "If the bird fall tail first, by throwing the tail back it will restore itself to the place of equilibrium, and if it throws it forward it will overturn itself."§

These, then, are da Vinci's observations and deductions. What did he do in the way of experiment to support them? In one manuscript‖ occurs the following highly interesting passage, affording another of those references to Leonardo's personal experiments: "Suppose that here then is a body suspended, which resembles that of a bird, and that its tail is twisted to an angle of various different degrees; you will be able by means of this to deduce a general rule as to the various twists and turns in the movements of the birds occasioned by the bending of their tails."

5 *The Movement of the Wings*

It will be convenient at this stage to consider Leonardo da Vinci's observations on the movement of the wings during "beating" flight (as distinct from "soaring" flight) and the criticisms to which these observations have been

* "Codex Atlanticus," folio 308, v.b.
† "Sul Volo degli Uccelli," folio 14 (13), v.
‡ MS. K, folio 10, v.
§ "Sul Volo degli Uccelli," folio 8 (7), v.
‖ MS. L, folio 61, v.

subjected. It will be helpful if we point out at once that the current theory as to wing movements has emerged as a consequence of the careful researches of the late J. B. Pettigrew. The results of these researches were presented in a series of memoirs* in the year 1867. We may summarise his results in his own words as follows :—

"That quadrupeds walk, and fishes swim, and insects, bats and birds fly by figure-of-eight movements.

"That the flipper of the sea bear, the swimming wing of the penguin and the wing of the insect, bat and bird, are screws *structurally* and resemble the blade of an ordinary screw propeller.

"That these organs are screws *functionally*, from their twisting and untwisting, and from their rotating in the direction of their length when they are made to oscillate.

"That they have a reciprocating action, and reverse their planes more or less completely at every stroke.

"That the wing describes a *figure-of-eight* track in space when the flying animal is artificially fixed.

"That the wing, when the flying animal is progressing at a high speed in a horizontal direction, describes a *looped* and then a *waved track*, from the fact that the figure-of-eight is gradually opened out or unravelled as the animal advances.

"That the wing acts after the manner of a kite, both during the down and up strokes."†

These conclusions apply equally to both insect and bird, but naturally the loops of the figure-of-eight will be differently proportioned in the different species. Again, during progressive flight the loops made by the wing of the insect, owing to the more oblique stroke, are more horizontal than those made by the wing of the bat and bird.

The principle is, however, in both cases the same, the loops ultimately terminating in a waved track. The impulse is communicated to the insect wing at the heavy parts of the loops *a b c d e f g h i j k l m n* of Fig. 7a; the waved tracks being indicated at *p q r s t* of the same figure. The recoil obtained from the air is represented at corresponding letters of Fig. 7b, the body of the insect being carried along the curve indicated by the dotted line. The impulse is communicated to the wing of the bat and the bird at the heavy part of the loops *a b c d e f g h i j k l m n o* of Fig. 7c, the waved track being indicated at *p s t u v w* of this figure. When the horizontal speed attained is high, the wing is successively and rapidly brought into contact with innumerable columns of undisturbed air. It consequently is a matter of indifference whether the wing is carried at high speed against undisturbed air, or whether it operates upon air travelling at high speed (as, *e.g.*, the artificial currents produced by the rapidly reciprocating action of the wing); the result is the same in both cases, inasmuch as a certain quantity of air is worked up under the wing and the necessary degree of support and progression extracted from it. It is, therefore, that as the horizontal speed of the body increases the reciprocating action of the wing decreases, and *vice versa*."‡

What then is the actual nature of the stroke of a wing during the progressive movement of a bird in horizontal flight? One further quotation from Pettigrew

* 1. "On the Various Modes of Flight in Relation to Aeronautics," J. Bell Pettigrew. Proc. Royal Inst., March, 1867.
2. "On the Mechanical Appliances by which Flight is attained in the Animal Kingdom," J. Bell Pettigrew. Trans. Linn. Soc., June, 1867.
3. "On the Psychology of Wings." Trans. Roy. Soc., Edin., August, 1867.
† "Animal Locomotion" (p. 16), J. B. Pettigrew. London, 1873.
‡ "Animal Locomotion" (p. 44), J. B. Pettigrew. London, 1873.

will serve to make this clear. "It is a condition of natural wings, and of artificial wings constructed on the principle of living wings, that when forcibly elevated or depressed, even in a strictly vertical direction, they inevitably dart forward. If, for example, the wing is suddenly depressed in a *vertical direction*, as represented at *a b* (Fig. 8), it at once darts downwards and forwards in a curve to *c*, thus converting the down stroke into a *down oblique forward stroke*. If, again, the wing be suddenly elevated in a strictly vertical direction, as at *c d*, the wing

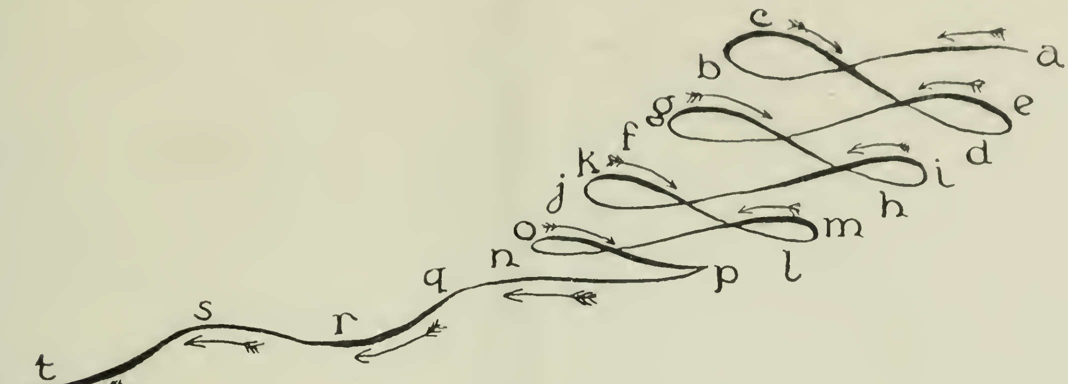

Fig. 7a

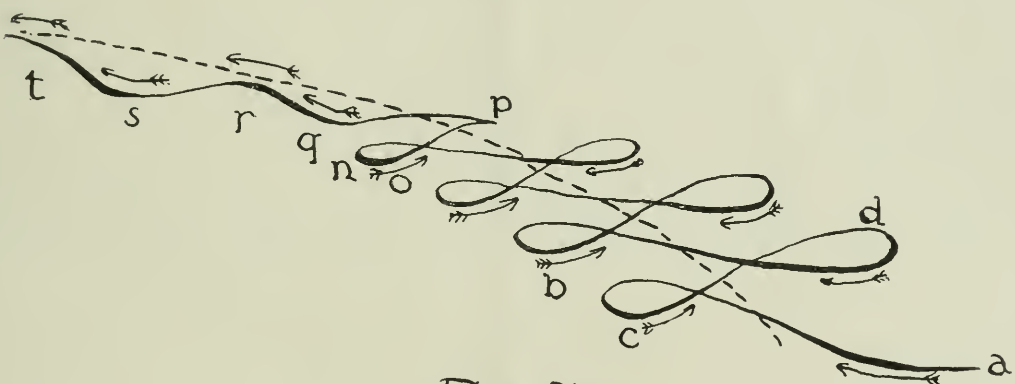

Fig. 7b

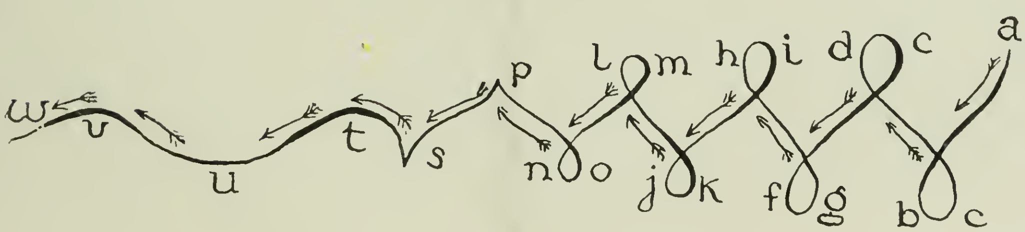

Fig. 7c

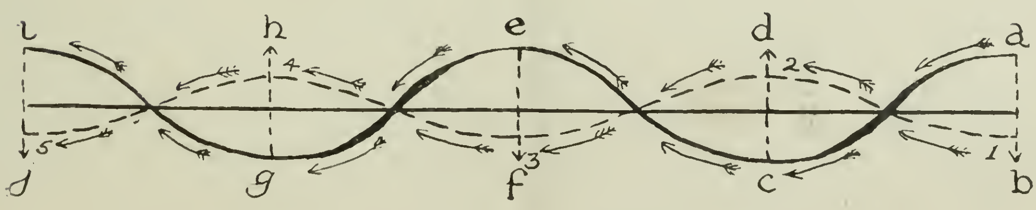

Fig. 8

as certainly darts upwards and forwards in a curve to e, thus converting the vertical up stroke into an *upward oblique forward stroke*."* Clearly, then, *the wing strikes downwards and forwards during the down stroke, and upwards and forwards during the up stroke*, and this was strikingly confirmed two years after Pettigrew's statement of the theory by Marey,† who devised an instrument by means of which he was able to obtain an automatic self-registration of the wing movements for both stationary and progressive flight.

The importance of a true understanding of these movements to the student of both natural and artificial flight is beyond question. This must have been evident to Leonardo da Vinci, and references in his note-books clearly show this. It must be confessed, however, that the observations show our pioneer to be at fault. Three instances occur, one in manuscript L and two in the note-book "Sul volo degli Uccelli." In the former we read as follows: "The birds which fly swiftly, keeping at the same distance above the ground, are in the habit of beating their wings downwards and behind them, downwards to the extent necessary to prevent the bird from descending and behind when they wish to advance with greater speed."‡ In the latter we have the following passage: "If, in the descent of the bird, it rows backwards with its wings, the bird will move rapidly, and this happens because the wings strike in the air which successively flows behind the bird, to fill the void that it has left."§

These two passages clearly indicate the inaccuracy of da Vinci in the light of the facts as they have been presented by Pettigrew. The third passage shows, however, that he was not wholly wrong and that for at least a part of the movement his observations were sound. "But when the wing recovers a new force with its return *upwards and forwards*, then the big finger of the wing puts itself in a straight line with the other fingers, and thus with its cutting extremity divides the air, by some movement, high or low that the bird would rise."‖ On the whole, the balance of argument undoubtedly shows that on this subject Leonardo was at fault.

6 *Soaring Flight*

We turn now to the subject of soaring flight—that is to say, flight without the beating of wings—in so far as it was developed by Leonardo da Vinci. In this, more than in any other branch of the study of natural flight, Leonardo excelled in the care and accuracy of his observations. There are still certain problems of interest connected with the soaring flight of birds awaiting solution. It was known to Leonardo that the power of birds in keeping poised in the air for long periods with apparently very little effort is aided to some extent by the existence of varying air currents at different levels. He also knew that some of the features of the flight and buoyancy of birds are due to their instinctive knowledge and utilisation of air movements which normally appear almost too fine for detection. It is in the attempt at such detection that experiments upon the subject of soaring flight are being carried out to-day, and it is interesting to note that the possibilities have emerged largely as a result of the recent successful experiments on man-propelled machines. The spaces of the sky, in which no apparent motion is visible to the human eye, are full of a movement that the instinct of a bird can apprehend and utilise. It is known that air currents are influenced by ground obstacles, such as hills and woods, while the very colouring of the countryside, which absorbs the solar heat unevenly, gives rise to wind deflections. That da Vinci was to some extent aware of the influence of ground

* "Animal Locomotion" (p. 157), J. B. Pettigrew.
† "Revue descours scientifiques de la France et de l'Etranger," 1869.
‡ MS. L, folio 59, v.
§ "Sul Volo degli Uccelli," folio 12 (11), r.
‖ Ibid., folio 14 (13), v.

contour on air currents is evident from the following passage: "Nature has so provided that all the large birds can stay at so great an elevation that the wind which increases their flight may be of straight course and powerful. If their flight were low, among mountains where the wind goes wandering and is perpetually full of eddies and whirlwinds, they would be unable to find any spot of shelter by reason of the fury of the icy blasts among the narrow defiles of the mountains, nor would they be able to so guide themselves with their great wings as to avoid being dashed upon the cliffs and high rocks and trees, and this would sometimes prove to be the cause of their destruction."*

In 1908 the late Sir Hiram Maxim published his well-known book on "Artificial and Natural Flight," the second chapter of which was devoted to "Air Currents and the Flight of Birds." It contains no reference to Leonardo's note-book, "Sul Volo degli Uccelli." A comparison of the two books, the one written almost exactly four hundred years after the other, shows in many respects a very remarkable similarity of statements and a coincidence of views. For example, we read in Maxim, "I have often observed the flight of hawks and eagles. They seem to glide through the air with hardly any movement of their wings. Sometimes, however, they stop and hold themselves in a stationary position directly over a certain spot, carefully watching something on the earth immediately below. In such cases they often work their wings with great rapidity, evidently expending an enormous amount of energy. When, however, they cease to hover and commence to move again through the air, they appear to keep themselves at the same height with an almost imperceptible expenditure of power."† And again, later, we meet with the following passage: "I have often noticed that gulls are able to follow a ship without any apparent exertion; they simply balance themselves on an ascending column of air, where they seem to be quite as much at ease as they would have been roosting on a solid support. If, however, they are driven out of their position, they generally commence at once to work their passage. If anything is thrown overboard which is too heavy for them to lift, the ship soon leaves them behind, and in order to catch up with it again they move their wings very much as other birds do; but when once established in the ascending column of air, they manage to keep up with the ship by doing little or no work. In a calm or head wind we find them directly aft of the ship; if the wind is from the port side they may always be found on the starboard quarter, and *vice versa*."‡ Again, later in the chapter we read of "soaring birds which practically live upon the wing and, by some very delicate sense of touch, are able to feel the exact condition of the air . . . and I have no doubt that the air cells, which are known to be very numerous and to abound throughout the bodies of birds, are so sensitive as to enable soaring birds to know at once whether they are in an ascending or a descending column of air."§

It is obvious here that the contention is that soaring birds are capable of periods of "rest" by supporting themselves on those columns of ascending air which are so plentifully produced in nature by the ordinary processes of convection. One further quotation will serve to complete the summary of the position and to enable us to make comparisons with the corresponding entries in Leonardo da Vinci's note-books. "Suppose," says Maxim, "that the local influence which causes the up and down motion of the air should be sufficiently great to cause the air to rise at the rate of two miles an hour, and that the wind at the same time should be blowing at the rate of ten miles an hour, the motion of the air would then be the resultant of these two velocities. In other words, it would be blowing up an incline of 1 in 5. Suppose now that a bird should be able to so

* "Codex Atlanticus," folio 308, v.b.

† "Artificial and Natural Flight," p. 13. Maxim, London, 1908.

‡ Ibid., p. 20.

§ Ibid., p. 23.

adjust its wings that it advanced five miles in falling one mile through a perfectly calm atmosphere, it would then be able to sustain itself in an inclined wind, such as I have described, without any movement at all of its wings. If it were possible to adjust its wings in such a manner that it could advance six miles by falling through one mile of air, it would then be able to sustain itself in an inclined wind, such as I have described, without any movement at all of its wings. If it were possible to adjust its wings in such a manner that it could advance six miles by falling through one mile of air, it would then be able to rise as relates to the earth, while in reality falling as relates to the surrounding air."*

We turn now to compare these statements with the following passages from da Vinci's note-books: "The imperceptible flutterings of the wings without any actual stroke keeps the bird poised and motionless amid the moving air."† "The kite and other birds that move their wings very little seek the air currents and when the wind is blowing high up then they will be seen at a great height and if the wind blows low down they remain low down." "When there is little wind the kite beats several times with its wings during flight so that it may rise and obtain impetus, with which impetus descending a little, it goes a long distance without beating its wings; and when it has dropped somewhat it repeats the movements and so successively; and this drop without beating the wings serves as a means of repose in the air after the fatigue of the said beating of the wings."‡ And again, "The helms which are on the shoulders of the wings are necessary when the bird in its flight without beating its wings wishes to maintain itself in part of a tract of air, upon which it is either slipping down or rising, and when it wishes to bend either upwards or downwards or to right or left."§

The similarity of treatment with Maxim is strikingly indicated, too, by the following further passages from the manuscript "On the Flight of Birds": "Here the big fingers of the wings are those which hold the bird still in the air against the movement of the wind; that is to say, the wind moves on which it supports itself without beating its wings and the bird does not change its place. The reason of this is that the bird arranges its wings on such an obliquity that the wind, which strikes it underneath, does not cause a wedge of such a nature as would lift it up, but lifts it up however just as much as its weight would press it down; that is to say, if the bird fall with a force of two the wind would lift it up with another force of two and thus . . . the bird remains in its place without rising or falling."‖ Later in the same note-book we meet with the following passage: "Let us say, that the impetus be as 6, and that the bird weigh 6, and that in the middle of the movement the impetus become 3, and that the weight still remains 6; here the bird would sink by half-movement, that is to say, by the diameter of the square and the wing oblique with the contrary aspect, also by the diameter of this square does not allow such a weight to sink, neither does the weight permit the bird to rise; consequently it moves in a straight line. That is to say, the descent of the bird during the said half-movement would be by the line *a b* "¶ (Fig. 9).

No study of the soaring of birds can be considered in any sense complete without due treatment of the large circular sweeping movements which form so familiar a spectacle of the skies, and these happily come in for a large share of attention in Leonardo's note-books. Thus, in the "Codex Atlanticus" we read: "The ways in which birds rise, without beating their wings but by circles, with the help of the wind, are of two kinds—simple and complex. The simple comprise those in which in their advancing movement they travel above the wind, receiving

* "Artificial and Natural Flight," p. 19. Maxim.
† "Codex Atlanticus," folio 308, v.b.
‡ "Sul Volo degli Uccelli," folio 6 (5), v.
§ MS. K, folio 7, v.
‖ "Sul Volo degli Uccelli," folio 14 (13), r.
¶ Ibid., folio 15 (14), v.

its buffetting from beneath, and so finish the reverse movement against the wind. The complex movement by which birds rise is also circular, and consists of an advancing and reverse movement against the direction of the wind in a course which takes the form of a half circle, and of an advancing and reverse movement which follows the course of the wind.

"The simple circular movement of rising without beating the wings will always occur when there is great agitation of the winds, and this being the case, it follows that the bird in so rising is also carried a considerable distance by the force of the wind. And the complex movement will be found to occur when there are light winds, for experience shows that in these complex movements the

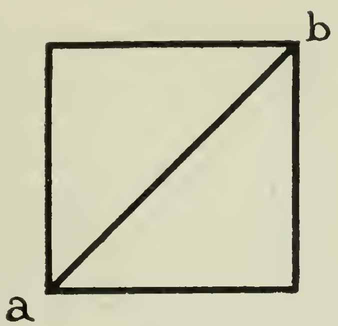

Fig. 9

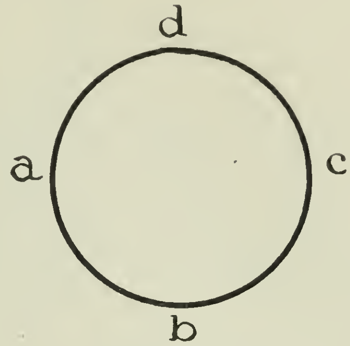

Fig. 10

bird rises through the air without being carried too far by the wind in the direction in which it is travelling."* In the manuscript "Sul Volo degli Uccelli" this theme is further developed with minute completeness,† in the course of which we meet with the following summary: "I conclude that the mounting of the bird without the beating of wings is caused by nothing other than its circular movement which, when it starts from the arrival of the wind, sinks until it reaches the place where the reflex movement begins, after which and so circulating, it has described a semi-circle and its face turned to the wind, and follows the reflex movement on the wind still circulating until, with the help of the wind, it makes its greatest height between its lowest and the arrival of the wind and is left with the left wing to the wind; and from this greatest height again circulating, it descends to the last incident movement, being left with the right wing to the wind. As if to say, the wind goes from *a* to *c* (Fig. 10) and the bird moves from *a* and sinks from *a b c* and in *c* it makes the reflex movement as in *c d a* and by the favour of the wind it is much higher at the end of the reflex movement, which end of the reflex movement is started perpendicularly over the said commencement of the incident movement."‡

7 *Manœuvre*

We may now turn with profit to the question of manœuvre and control, a subject to which da Vinci gave the closest attention. He went far beyond the stage of mere observation. As befits a man with an essentially scientific outlook, he considered practically every type of manœuvre for which one would look in an artificial machine; he closely observed the corresponding procedure in the case of birds and then brought scientific principles to bear upon their physical explanation. In connection with the main topics so far considered in this paper, as for example in the section on the functions of the tail, we have of necessity already

* "Codex Atlanticus," folio 308, r.b.
† "Sul Volo degli Uccelli," folio 7 (6), r; folio 13 (12), r and v; folio 15 (14), v.
‡ Ibid., folio 15 (14), r.

met with a number of references of this kind. The examples are nevertheless by no means exhausted, and we now proceed to indicate some others. As an instance, let us consider the manœuvre of turning. The necessity for "banking" was well known to da Vinci, who tells us that when the bird wishes to turn to the right or the left by beating its wings then it will beat lower with the wing on the side to which it wishes to turn (Fig. 11), and thus the bird will turn the movement

Fig. 11

behind the impetus (elan) of the wing which moves most (Vuscello (si) torcera il moto dirieto all impeto dell'alia che piu si mosse)."*

Other observations on this manœuvre in birds are as follows: "The bird when it wishes to turn does not beat its wings with equal movement, but moves the one which makes the convex of the circle it describes more than that which makes the concave of the circle."† And further on in the same manuscript, "The bird beats its wings repeatedly on one side only when it wishes to turn round while one wing is held stationary; and this it does by taking a stroke with the wing in the direction of the tail, like a man rowing in a boat with two oars, who takes many strokes on that side from which he wishes to escape, and keeps the other fixed,"‡ whilst the case of rapid turning in a high wind is dealt with as follows: "When the bird is carried along by the wind and wishes to turn quickly towards it, it will then enter beneath the wind with the wing turned towards it; and then with the feathers of the tail turned towards the wind, it will enter upon it, and so by the help of the wind striking upon its tail it turns much more rapidly."§

There is yet one further reference to the manœuvre of sudden turning to be found in the note-book "On the Flight of Birds." This calls for special attention on account of the clear indication it contains as to Leonardo's acquaintanceship with the principle of inertia. Two facts in this connection should be carefully borne in mind. Firstly, that in the fifteenth century the science of dynamics had hardly been born; and secondly, that according to accepted histories of mechanics,|| the so-called law of inertia was discovered by Galileo, and was presented by him in his "*Discorsi e demonstrazioni matematiche*" in 1638. In the light of these facts the following entry in Leonardo da Vinci's note-book is most remarkable. "When a bird wishes to turn itself suddenly on one of its sides, then it pushes quickly the point of the wing on this side towards its tail (Fig. 12), and *because all movement tends to maintenance*, or rather, *all moved bodies continue to move as long as the impression of the force of their motors remains in them*, the movement then of such a wing thrust with violence towards the tail reserving still at the end a part of the said impression, not being able by itself to follow the movement commenced at first, will move with itself all the bird, until the impetus of the moved air may be consumed. The tail pushes with its face, and the wind struck by it, makes the bird move suddenly in the contrary direction."¶ Turning to the manœuvre of rising, we have the following interesting paragraph:

* "Sul Volo degli Uccelli," folio 6 (5), r.
† MS. K, folio 4, v.
‡ MS. K, folio 7, r.
§ MS. K, folio 9, v.
|| e.g., "Science of Mechanics," E. Mach. English edition, p. 141.
¶ "Sul Volo degli Uccelli," folio 13 (12), r.

"When the bird by the beating of its wing wishes to rise, it raises the shoulders and it beats the points of the wings towards itself, and so condenses the air which lies between the points of the wings and the breasts. The tension (of the condensed air) lifts up the bird."* This is elsewhere developed further from the point of view of stability. "When the bird rises up by assistance of the wind without beating its wings, it spreads out and raises its wings to form an arch with the concave side towards the sky, and it receives the wind under its wings continually in its movements to and fro. This would cause it to turn

Fig. 12

right over if it were not that the point of its tail is turned to the wind as it enters beneath the wing, and this afterwards by the power of resistance acts to prevent the said movement of turning over, because the wings are restrained by the tail in such a way that their various parts are of equal power, and so the tail becomes partly lowered and the bird is raised forward slightly."†

One of the most interesting features of the note-book "On the Flight of Birds" is the series of passages which deal with problems of stability and control under variety of conditions. Leonardo dicsusses, for example, the behaviour of birds on meeting with a gust of wind. "If the point of the wing be struck by the wind, and this wind enter under such a point (Fig. 13) then the bird would

Fig. 13

be liable to be upset unless it uses one of two remedies, that is to say, either that it force suddenly such a point under the wind or that it lower the opposite wing, from the middle on."‡ This latter alternative, in effect, is designed to create a

* "Sul Volo degli Uccelli," folio 6 (5), v.
† MS. K, folio 10, v.
‡ "Sul Volo degli Uccelli," folio 7 (6), v.

restoring moment. Leonardo frequently makes use of the varying extent of wing surface presented to a wind as a method of maintaining control and equilibrium. The following passage is a striking illustration of this: "When the wind strikes the bird under its course from its centre of gravity towards this wing (Fig. 14) then such a bird will turn itself with its spine to the wind, and

Fig. 14

if the wind were more powerful below than above then the bird would turn upside down, *if it did not immediately take care to draw in the under wing and stretch out the over wings. In this manner it rights itself and returns to the position of equilibrium.* The proof is thus: Let *a c* (Fig. 15) be the wing with-

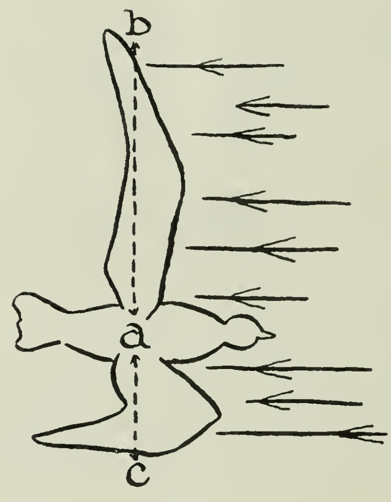

Fig. 15

drawn under the bird, and *a b* be the extended wing. I say that *the forces of the wind that strike the two wings will have the same proportion as that of their extensions,* that is to say, *a b* to *a c*. It is true that *c* is wider than *b*; but it is so near the centre of gravity of the bird that it offers little resistance in comparison with *b*."*

In the same note-book there is to be found an illuminating little passage worthy of notice in this connection, since it shows how sincerely scientific was Leonardo's treatment of problems of wing span in relation to wind pressure. "That the wing does not utilise all the pressure of the air," says he, "and that this is true, note that the interstices of the primary feather are spaces much wider than the width of the feathers themselves; therefore you who study flying should not calculate on the entire surface of the wing, but note the different varieties of wings for all flying creatures."† In thus drawing a clear distinction between *apparent* wing surface and *effective* wing surface, he gives us clear

* "Sul Volo degli Uccelli," folio 10 (9), r.
† Ibid., folio 9 (8), v.

evidence of a true appreciation of values. He was able in fact to probe beneath the surface of his problem.

In manuscript L there occurs an interesting example dealing with "head gusts." "When the wind is about to throw the bird backwards then the bird draws together the shoulders of its wings, so that its weight is massed more to the front than it was at first, and consequently the part that is heaviest is first in its descent while in addition the tail is spread out and bent down."* The following passage is also interesting as being typical of da Vinci's general treatment of problems of equilibrium during flight: "If the wing and the tail are too much on the wind, lower half the opposite wing, and therewith receive the force of the wind and equilibrium will be restored.

"And if the wing and the tail were under the wind raise the opposite wing and it will be corrected to your desire, provided that such wing as is raised be less oblique than that which is opposite it.

"And if the wing and the breast are on the wind, one must depress half the opposite wing, which will be struck by the wind and forced upwards, which will right itself.

"And if the bird is so that its hind quarters are on the wind then the tail ought to be forced under the wind, and thus one will be able to equalise the powers.

"But if the bird has its hind quarters under the wind let it enter with the tail on the wind, and it will right itself."†

8 *Safety Precautions in Natural and Artificial Flight*

We next turn to examples of Leonardo da Vinci's comments on the subject of safety precautions in flight. He had no illusions as to the dangers attendant upon attempts at artificial flight. Consequently it is no matter for surprise that references to safety devices, both in the design of apparatus and in the governing principles of control, are to be found fairly plentifully in his note-books. In particular he clearly realised that which modern aviators universally regard as almost a first principle in safe flying, namely, maintaining as high an altitude as possible. There are very few accidents to apparatus, or troubles due to sudden gusts, resulting in the temporary upsetting of the equilibrium of a machine, which cannot be corrected, given sufficient time and manœuvring space. The restoration of control, in the machine of to-day, is largely a matter of skilful pilotage. But this restoration does take time, and if during this period the machine is falling then disaster must ensue unless a high altitude had at first been attained. "High flying is safe flying" is the accepted principle to-day, and it was clearly so accepted by Leonardo da Vinci four hundred years ago. In his "Sul Volo degli Uccelli" we read: "The movement of the bird ought always to be above the clouds lest the wing should be moistened to disclose a broader view and to avoid the peril of wind revolutions among mountain gorges, which are always full of whirlings and turnings of the winds. Beyond that, if the bird should turn somersault, you would have more time for righting it, with the directions already given, before it reaches the ground."‡

In the next page of the same note-book Leonardo brings this point out again, more specifically with respect to artificial flight, and he deduces the best materials advisable for the construction of the artificial limbs. He condemns the use of iron fittings and argues in favour of joints of strong tanned leather with "nerves" or "sinews" of raw silk cord. The note is illustrated by a "limb" or wing, and reads as follows: "The said bird ought, with the assistance of the wind, to rise to a great height, and that will be its safety; even should it experience all the

* MS. L, folio 62, r.
† "Sul Volo degli Uccelli," folio 9 (8), r.
‡ Ibid., folio 7 (6), v.

above-mentioned revolutions, it has time enough to recover its balance provided that its limbs be very strong so that they may safely resist the fury and vigour of the descent with the above-mentioned defences, its joints of strong tanned leather and its nerves of raw silk cord of great strength; and let not anyone hamper himself with fittings of iron, because they burst easily in twisting, or waste away, for which reason they are not to be used."* On the assumption—and with Leonardo this amounted almost to an article of faith—that man is capable of flight, he wisely advises as much freedom from encumbrances as possible. "A man with wings should," he writes, "be free from the waist upwards in order to balance himself as he does in a boat in order that his centre of gravity and that of the instrument might be able to balance and change when necessity required it, according to the change in the centre of its resistance."† Fig. 16 shows in a modified form his diagram in illustration of this point.

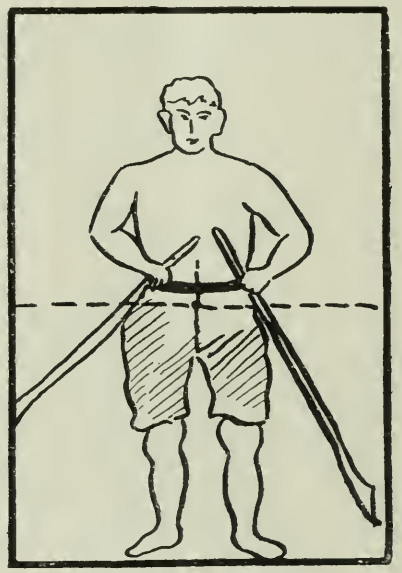

Fig 16

Another interesting passage, later on in the same note-book, is headed "To escape the peril of ruin" and reads as follows: "The ruin of such instruments may happen in two ways, of which the first is that the instrument break; the second is that the instrument turn on its edge or near its edge, because it ought always to descend by a great obliquity or almost by the line of equality. . . . As to the protection of the instrument from rupture, one avoids it by making it of the greatest strength by no matter what line it may be able to turn itself, that is to say, either by the edge falling either with the head or the tail first, or even with the point of the right or left wing, or by the halves or quarters of the said lines as the design shows (referring to a figure accompanying the note). As to turning by any side whatever of the edge, one ought to prevent it even in the beginning by making the instrument in such a fashion that in descending under whatever aspect that may come about the prevention should be ready; and this will be done in giving its centre of gravity on the centre of the weight carried by it, always in a straight line, and one of its centres very distant from the other; that is to say, that in an instrument 30 braccia wide these centres may be 4 braccia apart and that one, as mentioned, be situated under the other and the

* "Sul Volo degli Uccelli," folio 8 (7), r.
† Ibid., folio 6 (5), r.

heavier underneath, so that in the descent the heavier portion might always be the guiding portion of the movement."*

Finally, there is one further note hidden away in the corner of a page in which da Vinci, in anticipation of a fall by the fifteenth century airman, provides the means for absorbing the shock of collision with the ground. Referring to a sketch of some wine skins strung together, he writes: "Bags, where a man, falling from a height of 6 braccia would not injure himself, falling either in water or on land; and that these bags, fastened together in the fashion of beads, are surrounded by others."† On the reverse side of this same folio is the comment, "If you fall, see that you strike the ground with the double wine skin that you hold under you."‡

9 *Leonardo da Vinci's Flying Machines*

We come finally to da Vinci's notes on flying machines. We have seen the general nature of our philosopher's observations and experiments and of the arguments and principles he brought to bear upon them. How did he apply these to the design of suitable mechanism wherewith to achieve for mankind his ambition of mastery over the air? He lay down very specifically a principle for general guidance in the design of wings. "You are to remember," wrote he, "that your bird (*i.e.*, flying machine) ought not to imitate anything but the bat (Fig. 17), because the membranes form an armour or liaison to the armour, that

Fig. 17

is to say, strength to the wings. And if you imitate the wings of the feathered birds, the wings are more powerful in bone and nerve, through being pervious; that is to say, the feathers are disunited and permeable to the air. But the bat is aided by the membrane which binds the whole and is not pervious."§ This was sound advice and finds its final expression in modern aviation design. It is but a small step from the impervious membrane of a bat's wing to the doped fabric of an aeroplane wing. Yet how far did Leonardo follow this advice himself? Here was a line of attack on his practical problem which, in the light of modern developments, would appear to have contained the germs of some possible success. Yet in fact he appears to have neglected it, and it is disappointing that the sketches and notes in his manuscripts show Leonardo occupied chiefly with the idea of the substitution of "jointed" oars for wings. An almost isolated exception occurs in one note-book in which we meet with a sketch of an artificial wing (Fig. 18), obviously modelled after the wing of a bat. Against the shaded portion *X* he writes: "Make the meshes of this fibre of $\frac{1}{8}$in. width." Further details of construction are given with reference to the letters *A*, *B* and *C* in the

* "Sul Volo degli Uccelli," folio 13 (12), v.
† Ibid., folio 17 (16), r.
‡ Ibid., folio 17 (16), v.
§ Ibid., folio 16 (15), r.

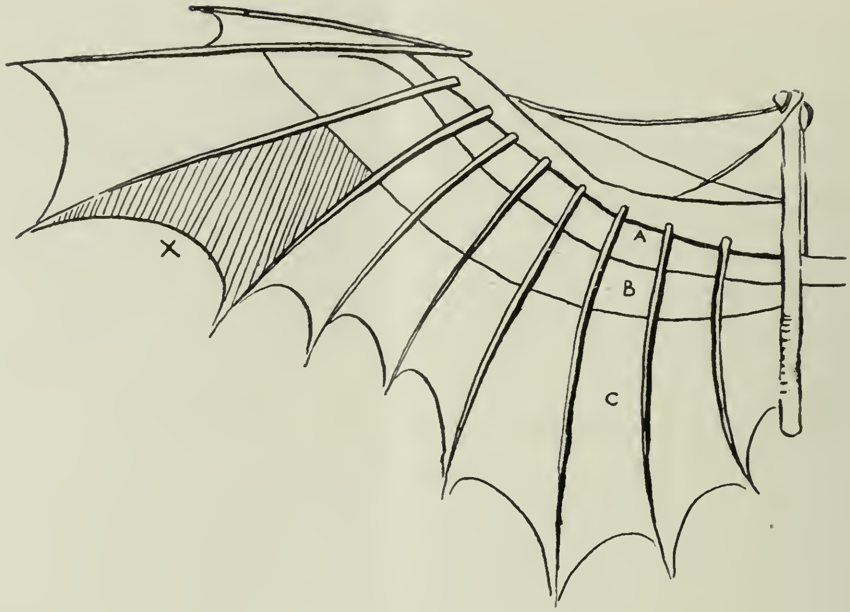

Fig. 18

diagram as follows: "*A* will be of blades of deal, which has threads (?) and is light. *B* will be of fustian, on which will be glued the feathers, so as not to be easily pervious to the air. *C* should be of starched light silk, and in order to test it, you may make it of thin pasteboard."*

Presumably Leonardo abandoned this type of wing as impracticable. Possibly he may have concluded that the wind resistance created by such an expanse of wing surface was far too much for the muscular energy of the human frame to cope with, but if this had been the case, one would have supposed the scientific alternative would have been to have reduced the area of surface accordingly. It seems probable that Leonardo attempted some experiments with a mechanical wing, and if so it is difficult to believe that the relationship between wing surface and wind resistance did not claim his attention. Thus we meet with a sketch of a man operating a mechanical wing (Fig. 19) accompanied by

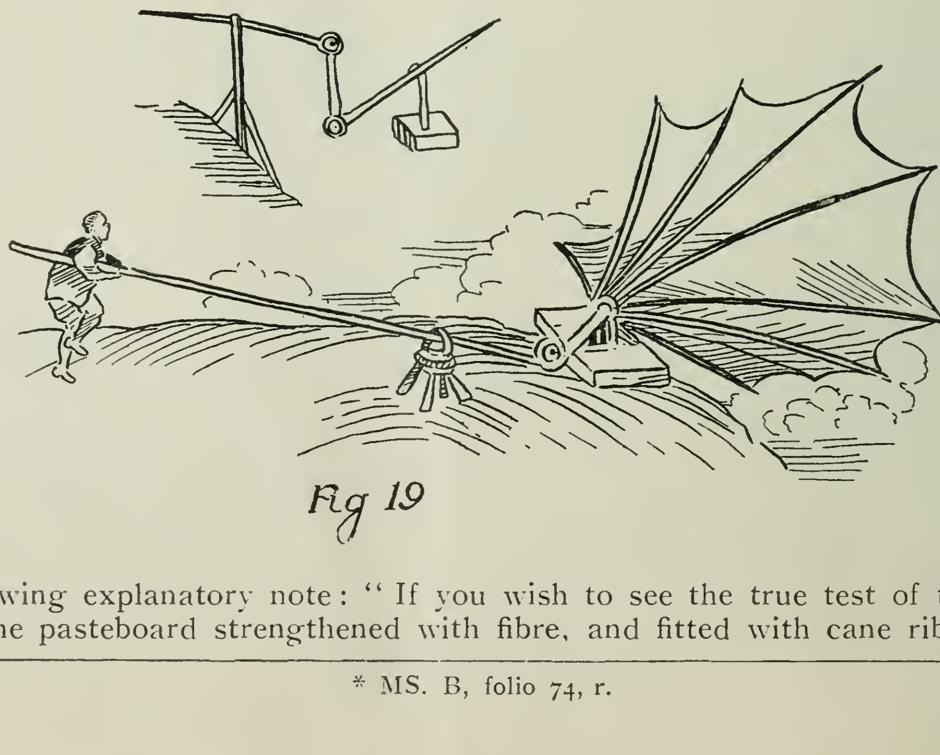

Fig 19

the following explanatory note: "If you wish to see the true test of the wings, take some pasteboard strengthened with fibre, and fitted with cane ribs, a wing

* MS. B, folio 74, r.

of the width and length of 20 braccia at least, and fix it on a board (sheet-pile) of 200 pounds weight;* it will produce, as shown in the figure, an effective force. And if the board of 200lbs. is lifted before the wing falls, the test is good, but see to it also that the force be prompt (to act) and if the above-mentioned effect is not obtained, lose no more time on it."†

We conclude from Leonardo's note-books—and these are almost our sole materials for judgment—not only that he either abandoned or neglected the idea of a "bat's wing" surface, but also that he came to no settled conclusions as to the use of a "jointed oar." Regarding it from the modern point of view, it is obvious that the air displacement created by the movement of a form of "oar" through the air must of necessity be utterly inadequate for the generation of a sufficient force to support a human being plus mechanical attachments. Consequently one need not be surprised at the variety, the vagueness, and the incompleteness of design displayed in many sketches and notes in manuscript B

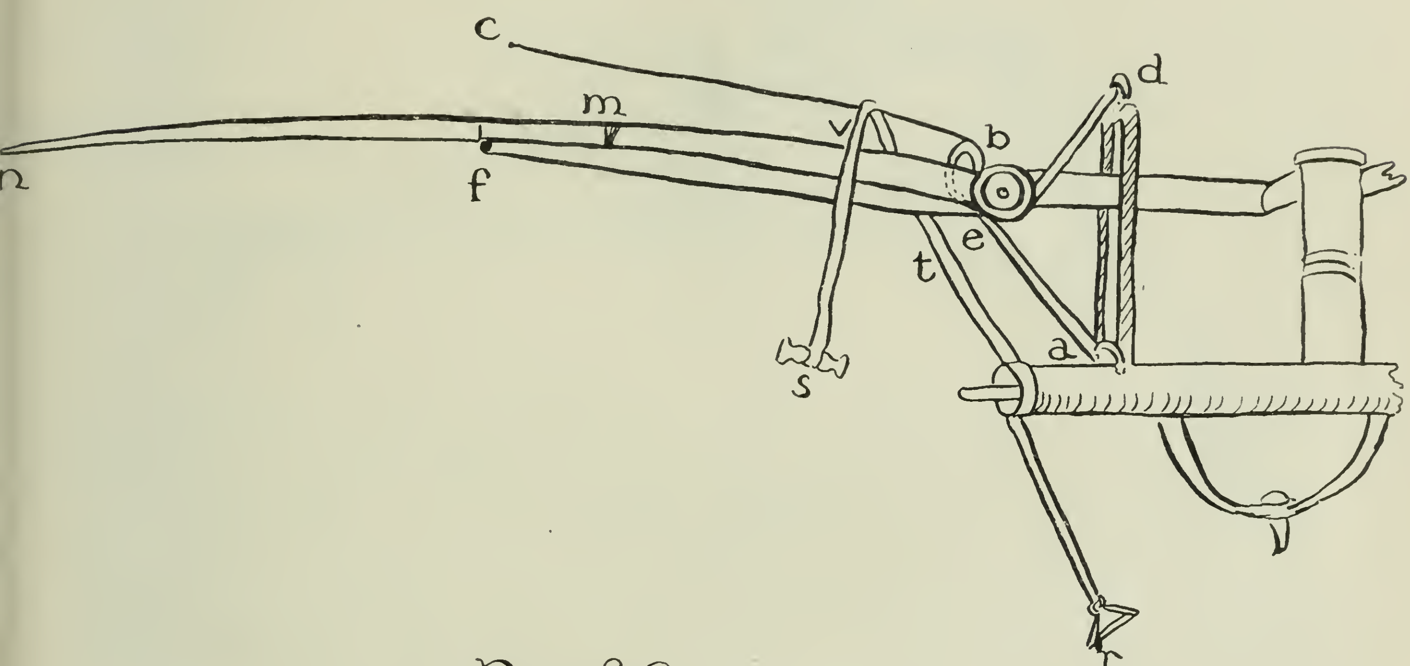

Fig. 20

and in the "Sul Volo degli Uccelli." They reflect the artist and the visionary vainly striving to link up with the engineer and the scientist. Da Vinci's scientific intuition was wonderful. His "sensing" of laws of nature was one of the most remarkable traits in his personality, and it carried him far—certainly far beyond his contemporaries. But there are serious limitations to this type of personality. Between the sensing of a law and an exact knowledge of its comprehension and its consequences lies a wide gulf of mathematical expression and equipment, and in this gulf it was inevitable that Leonardo would grope and flounder with little chance of success.

As a consequence the flying machines of Leonardo da Vinci are mainly of artistic and historic interest. Fig. 20 shows a typical example of a da Vinci wing—the left half only of the "machine." The following note accompanies the sketch: "*a b c* arranges that in rising the part *m n* is promptly raised; *d e f* arranges that in descending *m n* is promptly lowered, and the wing (thus) fulfils

* Sketches explanatory of these parts are shown in MS. B, folio 77, v.
† MS. B, folio 88, v.

its purpose; *r t* lowers the wing with the foot, that is to say, by extending the legs; *v s* raises the wing by the hand and the turn."*

Fig. 21 shows one of da Vinci's flying machines. The aviator is lying on a plank in front of which is a sort of pole. Fixed to this pole we see a rounded iron shank, and the jointed "oars" or wings are attached to this shank (Fig. 21). The wings are operated by the feet by means of stirrups *c* and *d*, the right foot lowering the wing and the left raising it. The design is difficult to follow, but that Leonardo intended it seriously is evidenced by the following interesting paragraph: "You will experiment with this instrument on a lake, and you will carry engirdled a long wine skin, so that in falling you will come to no harm."†

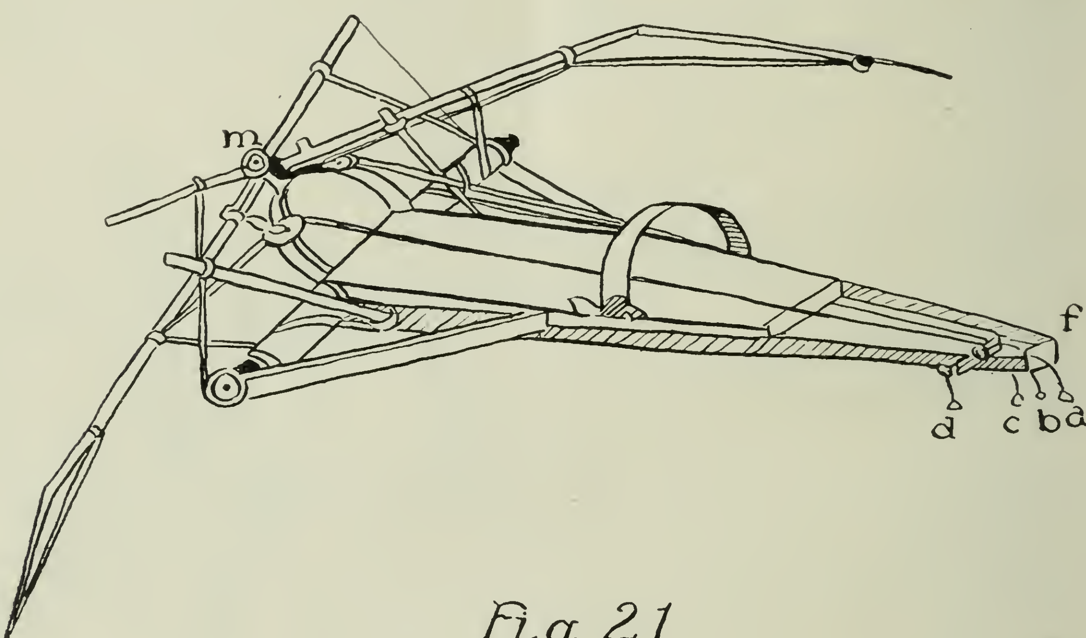

Fig. 21

Other designs‡ show double systems of oars, and he frequently favours the use of two ladders at the base of the machine to represent the feet of the "bird" and to serve the purpose of climbing into his instrument.

There is, however, one further design which calls for comment, since it constitutes a very real contribution to the science of aviation. It shows, in fact, that da Vinci may fairly lay claim to the invention of the helicopter; that is to say, to a type of machine capable of vertical movement upwards and downwards, and of hovering in any given position for any required time. Leonardo's sketch is outlined in Fig. 22. It consisted of a mechanism furnished with a helical wheel. This wheel is actuated by a twisted spring which, when released so as to unwind itself, sets the helical wheel in rapid rotation. Referring to this instrument, Leonardo comments, "I say that if this instrument made with a helix is well made, that is to say, of flaxen linen, of which one has closed the pores with starch, and is turned with great speed, the said helix is able to make a screw in the air, and to climb high. Take the example of a wide and thin ruler and directed violently into the air; you will see that your arm will be guided by the line of the edge of the said board. . . . One is able to make a little model of this of cardboard, whose axis should be of thin sheet iron, twisted with force;

* MS. B, folio 73, v.
† MS. B, folio 74, v.
‡ *e.g.*, MS. B, folio 80, r.

on freeing this, it causes the helix to turn."* Here then, beyond doubt, and for the first time in history, we have the principle of the helicopter.

What are we to conclude from all these activities? We have reviewed at some length the full scope of Leonardo da Vinci's researches and investigations in the field of aviation, and it is impossible to withhold our admiration for their breadth and their thoroughness. That he failed to achieve flight in no wise detracts from the value of his work. It is doubtful indeed if he ever even made the attempt himself. Jerome Cardan, the mathematician, whose father was a contemporary of Leonardo, who knew him and his work, tells us in his

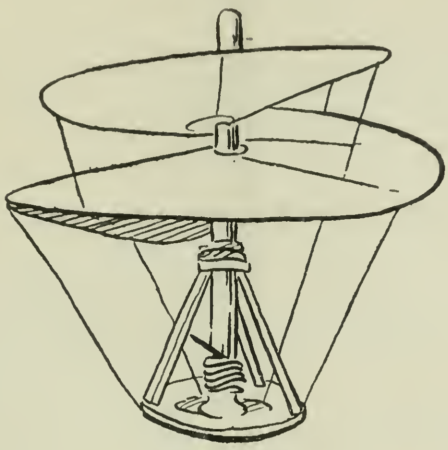

Fig. 22

" de Subtilitate " that " Leonardo da Vinci also attempted to fly, but misfortune befel him from it. He was a great painter." This appears to be the only reference to an actual attempt. Nevertheless, Leonardo's work in aviation was real enough, and having regard to the limitations imposed upon him by the knowledge of his days, it was *scientific*. At all times a student of nature and her mysteries, and gifted as he was with a bent for observation and deduction, he was able to combine with these qualities his mechanical genius as an engineer and his wonderful imagination as an artist. It is unfortunate that his manuscripts were scattered and lost to the world for so long.

(*To be continued.*)

* MS. B, folio 83, v.

ROYAL AERONAUTICAL SOCIETY

(Founded 1866).

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The Society exists for the furtherance of the Science of Aeronautics, and its activities fall into the following headings:—(1) Protecting the interests of the aeronautical profession by conferring a technical status on those qualified for such distinction. It acts as the professional society or institution of qualified aeronautical engineers. (2) Organising discussions and publishing papers on subjects of importance in connection with the various branches of aeronautical science. (3) Encouraging and assisting technical students who desire to adopt the aeronautical profession for their careers. (4) Providing an organisation wherein those interested in aeronautics from scientific or other motives, but who are not professionally connected with aviation, may meet together, have opportunities of study and keep themselves in touch with aeronautical affairs.

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The membership is divided into two categories:—

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Privileges

Members in any of the above grades are entitled to attend the Society's lectures, receive the monthly Journal free of charge and consult books in the Society's library. All grades except Associate Members are entitled to borrow books from the library. Students and Associate Members are not entitled to vote.

THE JOURNAL
OF THE
ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

No. 150 JUNE 1923 VOL. XXVII

NOTICES

Election of Members

The following members were elected at a Council Meeting held on May 15th :—

Student.—R. E. Bishop.

Associate Member.—C. F. Woodman.

The Pilcher Memorial Prize.

The Pilcher Memorial Prize for papers read at Students' Meetings during the Session 1922-1923 has been awarded by the Council to A. P. Rowe for his paper on "Aerial Navigation," read at the meeting of December 14th, 1922. This paper will be published in a forthcoming issue of the Journal.

Associate Fellowship Examination

The second examination for Associate Fellowship will take place in the Library on Monday, September 24th (Part I.) and Tuesday, September 25th (Part II.). Intending candidates should forward entry forms (accompanied by the prescribed fee) on or before Monday, August 27th, stating the subjects in which they desire to be examined.

Zaharoff Professor

All members will join in congratulating Professor Bairstow, their Chairman, on his appointment as Zaharoff Professor of Aviation in London University, which has recently been announced.

Library

The following books have been received and placed in the Library :—"A Treatise on Hydromechanics. Part II., Hydrodynamics," 2nd Edition, A. S. Ramsey ; "The Evolution of Climate," C. E. P. Brooks ; "A Text-Book on Wireless Telegraphy," R. Stanley ; "Generalised Linear Perspective," J. W. Gordon ; "A Dictionary of Applied Physics. Vol. III., Meteorology," Edited by Sir Richard Glazebrook ; "Planes and Personalities," A. C. Reid ; "Résumé des Principaux Travaux executés pendant la Guerre au Laboratorie Aérodynamique Eiffel, 1915-18," G. Eiffel ; "Notes on the Life of Frederick Marriott," G. R. S. Kirkpatrick ; "In Full Flight," E. Vine Hall ; "Aerodynamik," Fuchs and Hopf ; "Use of Aircraft in Warfare," R. B. Davies ; "Optical Methods," Goldsmith, S. Judd Lewis, F. Twynam ; "Face of the Earth as Seen from the Air," W. T. Lee ; "Météorologie Pratique. Etudes Elementaires," A. Baldit ; "British Standard Glossary of Aeronautical Terms," British Engineering Standards Association ; "America's Munitions," B. Crowell.

W. LOCKWOOD MARSH, *Secretary.*

PROCEEDINGS

Ninth Meeting, 58th Session

A meeting of the Society was held in the rooms of the Royal Society of Arts, Adelphi, London, on Thursday, February 15th, 1923, Professor L. Bairstow presiding.

The Chairman, referring to the paper to be read, namely, on "The Practical Aspect of Seaplanes," by Wing Commander T. R. Cave-Browne-Cave, C.B.E., said that this was the third of the series of lectures on seaplanes, a series which came into being mainly at the instigation of Wing Commander Cave-Browne-Cave. The lecturer was a member of the Council of the Society, and had thought that it would be advisable to have a connected series of papers of this description; it could be said that the policy adopted had been successful and would be rounded off by that evening's lecture. The author, in the beginning of the paper, explained what his position was, and that the paper represented other views in addition to his own, so that he (the Chairman) did not propose to enter into that subject, but he asked those present to note, when the paper was read, how the whole cause of seaplane development cried out loudly for further experiment. The conclusions in the paper were more definite than those they usually associated with pilots' opinions on the flying of aircraft, but that they were generally sound he himself had some reason to know. On the other hand, it was quite clear from the paper that the author had not been able to put into numerical form many things which he knew were true in a general way. He then called upon the author to read his paper.

Wing Commander Cave-Browne-Cave, dealing with the Chairman's remarks that he was putting forward views other than his own, said he believed that was not quite what the Chairman had intended to say, because he (Wing Commander Cave-Browne-Cave) definitely agreed with everything put forward in the paper, and the views expressed had pretty well the complete concurrence of the pilots with whom he had discussed them.

THE PRACTICAL ASPECT OF SEAPLANES

BY WING COMMANDER T. R. CAVE-BROWNE-CAVE, C.B.E., A.M.I.MECH.E., A.M.I.N.A., FELLOW.

Introduction

Many will wonder why the Council called upon one, other than a practical seaplane pilot of wide experience, to read a paper on this subject. That I cannot answer. The reason for which I agreed to write the paper was partly failure to get any one of the many having better qualifications to do the work, and partly because, having the pleasure of commanding the R.A.F. Establishment at which the marine experimental work is done, I do come into touch with the most expert pilots and designers, and have the opportunity of seeing certain points of view and experiences which may be of interest to members of the Society.

A most valuable cruise was carried out at the end of last summer by a squadron of four large flying boats under the command of Squadron Leader R. B. Maycock, acting as an R.A.F. unit in company with a mother ship. There was a destroyer and the seaplane floating dock in attendance. While it is undesirable to discuss in detail the comparison between the various types of machine

used, and the operations carried out, permission has been given to refer to many of the practical experiences met and to some of the deductions drawn.

The opinions expressed in this Paper have the general concurrence of the pilots of that squadron, and those at Grain, and carry weight for that reason.

The Paper deals with the practical aspects as seen from the Service rather than the commercial point of view, and it will be of great interest to see how experience in the use of seaplanes for commercial purposes modifies the conclusions reached.

The term seaplane includes the boat seaplane and the float seaplane, also those amphibians which are designed to be satisfactory seaplanes and not merely aeroplanes capable of using the water in emergency.

I have to express my indebtedness to the Air Council for permission to read this Paper, it being understood, of course, that nothing contained herein must be regarded *ipso facto* as the officially accepted view.

Flying

The behaviour of seaplanes in the air differs widely from that of aeroplanes. The difference is most marked in flying boats, but exists in all machines that carry a hull or floats.

In the more extreme case of the flying boat, the differences are somewhat startling.

The C.G. is unusually low by reason of the hull or floats, and the thrust line is high, so that the airscrews may be clear of spray. This involves a large change of trim on leaving the water, and a still larger change of trim between engine off and engine on. It is, therefore, usually necessary to adopt a depressing tail placed well in the slipstream, although this is most uneconomical in lift.

A long bow to the hull extending well forward is necessary to avoid diving if alighting steeply. The presence of this area forward renders desirable a long tail with large surfaces. This is usually impossible, and stability and control are therefore bad.

To avoid damage when on the water, it is usual to fit no ailerons on the bottom plane. Control in this respect is therefore bad also. Tail surfaces are necessarily at a considerable distance above the axis of the hull which carries them, and thereby lack rigidity due to the torsion of the hull.

Much of the trouble would be reduced if a good dihedral angle could be adopted, but the consequent raising of the wing tips would allow a very large angle to roll on the water, unless deep wing tip floats were employed.

The fins on the top plane give the same effect as dihedral.

These restrictions result in such alarming differences as the following :—

> The nose tends to rise in a sideslip.
>
> It is usually necessary to reverse a control in order to check a swing that has once been started.
>
> To compensate for switching off the engine, it is usually necessary to force the control almost to its extreme forward limit.

Under these circumstances, only the most mild manœuvres can be carried out with safety, and the operations of taking off and alighting, which require certain and accurate fore and aft control, present, under some conditions of sea, difficulties considerably greater than in the aeroplane of corresponding size.

The two pilots of a 700 h.p. flying boat sit abreast, and in bumpy weather are often working right up to the limit of their physical strength. If ever there was a case for the servo motor control it is in the large flying boat. The

ability of the two pilots to work so well together caused me to suggest that controls might be made independent, and worked by separate individuals on the orders of the captain, as is done in a large airship where the energy required in the controls is large. This has never been tried as far as I know.

In steady weather, however, all well-designed flying boats will fly as comfortably " hands off " as will the best aeroplane in the same pleasant conditions.

On the Water

An interesting record of the recent development squadron is that the number of hours actual taxiing on the water was only some 20 per cent. less than the total flying time.

An experienced pilot has expressed the opinion that there is as much difference in handling a seaplane on the water and an aeroplane on the ground as between an aeroplane in flight and one taxiing.

The difficulties of a forced aeroplane landing in unsuitable country have not as a rule any parallel in a forced landing at sea, although other troubles arise in bad weather. The seaplane has no similar advantage in its normal taking off and alighting. The waters it uses are in no respect private aerodromes reserved for the use of aircraft. They are often dotted with ships at anchor or under way and, what is often worse, with buoys and small boats. In addition there is often a tide which may be in any direction to the wind.

A good view ahead is therefore essential. Manœuvrability of a single-engined seaplane is usually bad because control is dependent upon the action of the slipstream on the rudder, and in order that this may be sufficient to overcome the disturbing air and water forces, considerable engine speed and consequently forward speed is essential.

A water rudder improves matters, but unless its area is made much larger than is at present usual, it is incapable of overcoming the large forces on the air structure.

A water propeller might be similarly ineffective.

A twin-engined machine handles much better on the water, because the airscrews being set wide out exert considerable turning moment. The principal difficulty is in checking the way of the machine. In order to approach a mooring buoy at safe speed it is necessary to use a drogue, even if engines are throttled to the slowest speed at which they will run. Two drogues may be used, and by their added resistance assist steering considerably if there is no wind.

A flying boat, when in tow of a motor boat, yaws wildly unless drogues are used or the tow is made fast to a span to the wing struts.

Towing is an operation in which much useful experience has been gained. A machine does not tow comfortably from a motor boat when a short tow has to be used and accurate manœuvres are called for. She yaws widely unless drogues are used or unless there is a fair wind in a suitable direction.

The use of a tow only a few feet long has recently proved effective.

Towing in the open sea is, however, much easier than anticipated. Greater speeds can be used, and if the speed, length of tow, and use of drogues are adjusted according to circumstances, a flying boat can be towed at considerable speed even in fairly bad weather.

An interesting recent development has been the towing of one flying boat by another. This proved quite easy, as the much greater available range of speed allowed the towing to be adjusted easily to the conditions of wind and sea.

A difficulty that is not always recognised is that in a congested harbour a seaplane pilot can never stop and think. He drifts so rapidly that he must

always be effectively under way unless he is tied up to a buoy or in tow. Small adjustments which are easily made on an aerodrome are therefore attended with greater difficulty.

Seaworthiness,

as applied to aircraft, covers a wide range of circumstances. The ability to get off the water depends on characteristics different from those which control ability to alight and to live safely on the surface.

The fundamental difficulty is the destructive effect of water on parts of the structure designed for air loading. Water is some 900 times the density of air, and even a spray which contains only 1 per cent. of water causes nearly nine times the loading due to air at the same relative velocity. The destructive effect of spray on propellers is therefore obvious.

The best method of mooring, anchoring or towing is by the arrangement of lines shown in the figure. The mooring shackle by which the boat is secured for any of these purposes is carried by a pair of wires made fast to the foot of an inter-plane strut on each side of the machine (in a twin-engine machine the engine struts are usually selected) and by a central wire having at its after end a lignum vitæ thimble running on a bridle attached to the bow point and to a point on the keel.

Secured in this way, the boat rides head to wind, and the bow is free to rise to the waves.

As indicating the seaworthiness of flying boats when properly moored, it is interesting to note the experiences of the machines of the Development Flight at Scillies. The flying boats were moored in an open anchorage, the mother ship lay under shelter inshore. She had to hoist in all her boats except two. The flying boats all reported next morning ready for a rough weather towing trial, which it was, however, decided to abandon.

The resistance offered by the air structure is very considerable, and a mooring of surprising strength is necessary. The lift of the planes becomes sufficient (at some 50 to 60 knots) to take the boat into the air. This has not infrequently occurred. The most striking case of which I have heard, but of which I have not yet managed to find an official report, was told to me by a reliable authority. In this case, which occurred on the Greek Coast, a flying boat was moored to a sinker of considerable though insufficient weight. The wind increased until the boat broke the sinker out of the sea bottom, rose into the air and glided ashore, knocking a hut over with the sinker before landing.

A perfectly authentic case occurred at Scapa Flow, where a three-engined Porte boat flew itself ashore with a set of four sinkers.

To reduce this tendency to fly at their moorings, a device was employed at Scillies during the war, and consisted of 9in. boards secured edge up behind the front struts of the lower plane, thereby destroying its lift.

With some of the flying boats which had low clearance under the wing, it was, for safety after a forced landing in a bad sea, desirable to strip the fabric from the bottom plane. This prevented the water exerting on the wing forces sufficient to break the structure and cause the machine to overturn.

The clearance under the wing of P.5 is only 5ft. 2in., and the free board at the after hatch is 1ft. 7in., but this was found on the last cruise to be ample.

Damage to a wing tip float, although apparently a trifling matter, may result in the complete overturn of the machine unless immediate steps are taken by sending men out to the other wing tip to keep the damaged float clear of the water.

It is usually found that alighting in a big sea is worse than remaining on

the water, unless damage has been done on landing. The machine is kept head to wind by the use of a drogue attached to the mooring bridle already described.

Difficulty is sometimes experienced in determining the height above the surface when alighting. This is worst when there is a glassy surface on the water and a low sun to windward, as the glare then is at least twice as bad as over an aerodrome in similar conditions.

A glassy surface with a white mist and no horizon is almost equally bad, and remarkable " virtual landings " 20ft. above, or below, the surface are seen.

Automatic landing with the aid of a hanging stick so connected to the control that it flattens the machine out at the right height above the water, functions very well and renders a forced landing in fog or darkness much safer than over land, unless the water be very rough or is that of a crowded harbour, when ships and buoys may be met.

The limiting factor is generally the ability to get off with a reasonable load.

The margin is often very small and even the slightest temporary increase in power may make all the difference between failure and success.

Except in the open where a big sea may be running, it is usually found that any wind, up to say 40 knots, assists the process of getting off. The most difficult conditions are a long swell and no wind. Under these conditions a boat has run two miles before getting off.

Success depends on an adequate horse-power for the weight carried, and on the sufficiency of longitudinal control to allow the pilot to maintain the right attitude to insure that the boat attains flying speed before she is lifted off the water and sufficient control to avoid stalling immediately she takes off.

A 10cwt. stockless anchor with nine fathoms of 7/16th chain cable holds a spherical iron buoy of some 470lbs. buoyancy. The buoy carries a large wire eye at the top, and this can be easily hooked by the machine. A two-fathom length of 2½in. wire is attached to the chain close under the buoy, and has a large eye spliced in its outer end. This is tied to the top eye when not in use, and being made fast to the boat's mooring shackle, allows her to ride clear of the buoy and free from damage.

The provision of anchor and mooring gear sufficiently light to be carried in the boat presents considerable difficulty. There is room for much experiment on the lightest form of anchor capable of a given holding capacity.

Seaplane Trolleys

The usual method of moving a seaplane on land is to rest it on a specially shaped trolley. With float seaplanes there is comparatively little difficulty, but the process of bringing the hull of a flying boat into its correct position on the trolley is attended with the risk of serious damage to the bottom. The positioning has to be done by men in waders round the trolley, assisted by men on the side gangways of the slipway who steady the machine with wing tip lines.

In the case of an 18-ton machine, such as N.4, which, with her trolley, draws nearly 5ft. of water, the process becomes impossible (except under ideal conditions).

The movement of the hull over the trolley, which is usually under water, often causes damage to the bottom, which is very difficult to make good.

The damage would be largely avoided with a trolley that supported the boat under the docking chocks now provided at the wing roots for dry docking (described later). These chocks are always above water, and it is possible to see

that the position is correct, or to use guide lines to draw the trolley into correct contact with the chock.

In order to overcome the difficulty of using a trolley on the slipway, a buoyant trolley was made with a view to placing it in position while both machine and trolley were afloat.

The water resistance of the trolley with its buoyancy chamber was so great that it was impossible to place it under the machine unless there was no tide stream, and the scheme was therefore abandoned.

Seaplane Dock

Quite the most important recent development in seaplane work has been the floating dock designed by the Admiralty to Air Ministry requirements and made at Sheerness Dockyard. The dock is like an ordinary floating dock, but has a forecastle structure which accommodates a crew and provides machinery and workshop spaces.

The dock is submerged, the machine floated into position above pedestals which are adjusted to lift the machine under the special pads fitted to the wing roots.

The operation can be carried out in any wind, and except with a heavy swell, presents no real difficulty.

On the second occasion of docking a machine at Grain, the boat was lifted clear of the water 16 minutes after the first line was thrown to the machine as she taxied up to the dock mouth.

This method of support, besides minimising risk of damage to the bottom, leaves the bottom exposed for repairs.

The experience of the Development Squadron shows that such a dock is necessary for repairs to the boats' hulls. Certain of these repairs can be temporarily done if a protected sandy beach is available, but for a squadron of flying boats, operating away from a permanent base, a dock such as this is absolutely essential if very rapid loss of boats, through inability to make good damage, is to be avoided.

The dock enables engines to be changed in 48 hours, but without it the vital process of changing engines would be virtually impossible, except at the permanent base.

In fitting out a mother ship for a squadron of flying boats, or in providing an advanced base, say, for commercial operations, it appears necessary that some means of lifting the boat's bottom clear of the water for repairs should be provided until some form of construction altogether more reliable than the present bottom has been evolved.

Points of Design

The improvement which is perhaps most desirable is in the effectiveness of air control.

The shortness of the tail and the lack of rigidity with which it is supported are consequences of carrying the tail on an extension of the hull. Increase of length would therefore involve considerable weight, and an even greater torsional movement.

The solution appears to be in carrying the tail on a fuselage or on two fuselages arranged immediately behind, and co-axial with the airscrews. A fuselage behind an airscrew detracts practically nothing from its efficiency. The tail may then be long and supported symmetrically, so that far less supporting structure and consequent resistance is involved.

With such an arrangement the hull would be cut off at the aft step, and the upper surface swept down to this line.

A tail in this position would be very well protected from water damage, and would have many incidental advantages when the boat was moored.

Bulkheads are desirable to limit the surging of water inside the hull when getting off or in flight, but also as a protection against sinking if the hull is damaged. The construction of a flexible bulkhead in a flexible hull is understood to present some difficulty. If it is agreed that the bulkhead need only be carried to a small distance above normal water level, it can conveniently be stepped over by the crew passing along the hull, and construction would be comparatively simple. Such a bulkhead could certainly be made sufficiently flexible to avoid "freezing" the flexible hull any more than is done by the step board.

Storage of petrol in tanks in the hull in close admixture with W/T and other electrical gear calls for careful consideration. The remarkable properties of streamline petrol tanks fitted under the top wing have been demonstrated. Even if petrol leaking from such tanks is ignited it burns harmlessly in a feather of flame astern of the tank. The rush of air is such that it is almost impossible to ignite petrol issuing in this way.

Storage of the full weight of petrol on the wings involves slightly increased strength of structure and slightly higher wing tip loads, but these are in most opinions fully justified by safety from fire, by gravity feed and the improved accommodation in the hull. The raising of the C.G. is definitely beneficial and the increase of moment of inertia about the longitudinal axis is very small unless the tanks are set wide out.

The central engine room inside the hull, with gear drives to the propeller, has most important advantages. The care, adjustment and control of the engines is greatly improved. Risk of fire is, of course, again introduced, and at present the difficulties of torsional resonance in the shafting and gearing do not appear to have been overcome.

The difficulty of maintaining an aero engine in proper running order when it is washed down with sea water every time the machine takes off can only be fully realised when experienced at first hand.

The disposition of crew is, as a rule, fairly satisfactory. A good view forward for both pilots is essential. It is impossible for either pilot to see over the other side of the boat, as the seats are abreast. A cockpit in the extreme bow is necessary for anchor work, mooring, etc., but this must be fitted with a watertight hatch. The new hulls now being designed by Messrs. Supermarine are admirable in this respect.

More attention to the protection of the pilot from spray is desirable. The boat must be designed so that the propellers are fairly clear of spray. If this care of the propellers is neglected they break. The pilot, on the other hand, has flying clothing, which is not proof against spray driven by a 50-knot wind.

The increase of seaworthiness in large machines is a point on which considerable doubt exists. The water clearances will increase, but the span and length also increase, so that the difference of level of the sea surface under various parts of the machine may increase as rapidly as the clearance. Those of you who have not experienced a sea which is logged according to recognised scale as "moderate," will probably have seen pictures of destroyers rolling about in such a sea. The design of a large machine which can live in a sea of this size, let alone the question of taking off and alighting, is a point which troubles the imagination considerably.

Great discussion ranges round the relative merits of flexible or rigid hulls, but the two general types differ in so many other important particulars that

comparison on the question of flexibility is impossible. The so-called flexible hull is not very flexible at those sections which are most highly loaded. It is difficult to believe that the external surface deflects under load to such an extent as to render the distribution of pressure more uniform. Any flexibility which may be left in the hull after the effect of the rigid step board and the wing root struts has been considered can therefore only act as a shock absorber between the impulsively loaded bottom and the inertia of the weights of the machine. It has been stated that some experiments in America recorded accelerations of 7g. on the bottom of the hull, but I have been unable to get any particulars of the method of trial or the accuracy of the accelerometer.

As compared with the "F" type the "P" boats have a smaller planing bottom and better fore and aft control, so that they attain their full flying speed before taking off. The smaller area of bottom is in itself conducive to less severe shocks when striking a wave, and the better control renders bad shocks still less probable.

On the development cruise the "P" type showed a marked superiority, except that considerable damage to the rigid step was experienced, due no doubt to the presence of a rigid step on a flexible hull. It is very probable, however, that a rigid hull of the same external form might have done equally well, and had no trouble with the step.

Various accessories, if not peculiar to seaplanes, are at any rate of special importance.

A bilge pump which can deal with dirty water and pass a large quantity with the few inches lift necessary is a requirement that is not easily met. Any form of reciprocating pump is apt to meet serious damage at the ends of the stroke if being worked at full speed under considerable stress of circumstances. Most pumps available have a small diameter and long stroke, and consequent maximum friction loss.

A power-driven bilge pump, or perhaps one which would clear out the bilge water while the seaplane is taxiing, would be of great advantage.

Engine starting in cold and wet weather is always a trouble, and there is a very strong case for the gas starter engine. Machines intended for work in winter, and moored out, would benefit from a well-lagged water tank into which the water could be drained and then pumped back when required. A small auxiliary engine for pumping, engine starting, warming water, and perhaps cooking food by the exhaust, would, in my opinion, be one of the most valuable uses for a little of the available lift.

Waterproof engine covers and a rigid cover to the front cockpit are, of course, absolutely essential.

The troubles experienced by the Development Squadron in maintaining the material of their machines in safe and efficient condition were in some respects abnormal, because of the age and initially poor condition of the machines. Their experiences are, however, of exceptional value.

The amount of water absorbed by the hull and wings was difficult to determine accurately, but it was certainly not less than 400-600lbs. on machines carrying only 2,500lbs. of fuel.

The amount of water which is taken up by the hull is, no doubt, only a portion of the total absorption referred to, but avoidance of this added weight becomes an important advantage of the metal hull. For large machines which will pass the majority of their time between flights moored out, the advantages of the metal hull are so important as to justify some increase of weight over the wooden alternative. There is, however, considerable doubt whether in any but small sizes the metal hull would actually be any heavier than the wood type.

Moisture had a further effect in making the planes zoggy, and eventually in destroying some of the 3-ply used.

Corrosion of steel and aluminium parts was very bad.

Much work was required in making good small local damage and cracking of the hulls, a most difficult process, generally impossible unless the boat can be dry docked.

All these troubles, though serious, are not nearly as likely to prove fatal as those resulting from trouble with trolleys on a slipway in bad weather.

We are therefore led to the conclusion that flying boats must, if possible, be made weatherproof, so that they can remain moored out and derive all the consequent advantages of safety and immediate readiness for use.

The large seaplane has certain inherent advantages which have been most clearly demonstrated by Mr. Fairey in his recent paper before the Air Conference. His conclusions are so essentially part of the practical aspect that they should be referred to in this paper.

The difficulties which a large aeroplane encounters in the process of landing become very serious with increase of size. Not only is the necessary size of aerodrome very large, but the actual strength of ground surface which is needed to carry such a large weight is difficult to find.

Even if it were possible to provide an aerodrome capable of meeting these requirements, the consequences of a forced landing where such conditions are not available are serious. The seaplane, as it increases in size, encounters no such increasing difficulties in landing, the indication being that the difficulties connected with rough water will decrease in the larger machines.

In the great majority of country over which aircraft will work, particularly on long distance routes, it will be far more easy to select courses which have a reasonable number of safe landings for seaplanes on rivers, lakes, or the sea, than for emergency aerodromes which meet the more exacting requirements of the aeroplane.

Conclusion

The practical aspect of the seaplane is one which requires most careful study, because it is in that that we realise the vital requirements of handiness on the water. These not only control and limit the actual use of the machines, but are so hard for a designer to realise unless he is also a seaman of intelligent experience.

The prospects for a carefully developed seaplane are, however, so promising that its study is certainly one of the most interesting branches of aeronautical engineering.

Discussion

General Bagnall-Wild (Director of Research, Air Ministry), opening the discussion, said that it was the first time they had had the seamanship side of aircraft put before them. It must be realised what seamanship meant. In the first place, officers and men must be trained for seagoing duties and learn the language of the sea.

As regards methods of launching seaplanes he was of opinion that the use of the trolley was elementary and was not desirable in that with a big seaplane there is a considerable risk of damage to the hull. In addition, in the future it appeared that all the smaller seaplanes must be amphibians. In front of the sheds there should be a " hard " so that these amphibians could land themselves.

For the bigger machines a dry dock at permanent stations, in his opinion, was essential. It would not be expensive to build a dry dock in tidal waters with suitable protecting aprons. Elsewhere, the floating dock would be quite capable of coping with the biggest machine. Big machines should be capable of being moored out for long periods, even six months at a time.

Mr. John H. Narbeth, C.B., C.B.E., M.V.O., R.C.N.C. (Chairman of the Admiralty and Air Council Joint Technical Committee on Aviation Arrangements in His Majesty's Ships), after thanking the Society for the opportunity given him of hearing the paper, said that, although he was a Naval Architect, he took a very deep interest in all that concerned aviation. Having been at Spithead when Commander Samson had flown from Sheerness, about twelve years ago, he had been fascinated with the flying boat and seaplane subject. The Air Ministry was to be very sincerely congratulated on having allowed Wing Commander Cave-Browne-Cave to make public the experiences of the manœuvres, and the paper seemed to be an exceedingly practical and important one which would give rise to a very great deal of thought and discussion. He hoped that, having made a start with these flying boat manœuvres, they would become annual events. He was present as a member of the public, and was simply voicing his own opinions; it certainly must not be thought that he was expressing Admiralty opinion at all.

Wing Commander Cave-Browne-Cave had said that there were four boats used, and they were of sufficient variety to indicate the choice of type in a very marked way. The Air Ministry had learned one thing quite definitely from these manœuvres, and that was which type of boat they should spend their money on, and no doubt considerable economy of public money would be effected as a result.

The paper clearly demonstrated two things. In the first place, it showed the very remarkable seaworthiness of these flying boats in broken water, and that was a subject upon which he had previously heard some very poor opinions as a result of experiences during the war. Seaplanes which had been at work on the North Sea had disheartened the pilots very much, but during last summer, when these flying boats were on manœuvres, there was a certain amount of wind, and the sea was often rough and never smooth, so that they had on that occasion a very fair opportunity of testing the seaworthiness of the present seaplanes, and a much more favourable report showing that seaworthiness had greatly improved. Secondly, the manœuvres had shown the great value of the floating dock. He had had the honour of being associated with the production of that dock, which dock bore witness to a very important feature in the development of aviation in this country, and that was that, whatever differences of opinion there might be between the Admiralty, the Air Ministry and the War Office as to policy, once a policy was settled and there was something to be done, the technical officers of the various departments combined with the greatest zest. In the production of this seaplane dock the officers and pilots of the Air Force had said exactly what they wanted, naval officers had given advice, and together with the naval architects, who had a very wide experience of floating docks of various types, they all met together, discussed the various types of docks, and agreed to put forward the proposal to build the smallest seaplane dock to take the largest flying boat then in sight. He believed it had been demonstrated that the country's money had been very well spent on that dock. The success of that dock, which was of a unique design, was so great that he was sure it indicated a broader path for the future development of the flying boat services.

The third paragraph of Wing Commander Cave-Browne-Cave's paper deserved to be printed in letters of gold; that was "The opinions expressed in this paper have the general concurrence of the pilots of that squadron, and those at Grain, and carry weight for that reason." We have had a great deal of scientific research, there is an enormous fund of knowledge at the disposal of our designers, and we have reached the point where what was wanted more than

anything else was to make the utmost use of the practical knowledge of the pilots. He was sure pilots could give a vast amount of information, and he was very pleased to know that such pains had been taken to gather from them their practical views, and also that the paper did really express their views. He hoped the Air Ministry would be convinced of the great utility of the flying boat and of the importance of carrying out these manœuvres annually, in order that flying boats of British design and manufacture might continue to be developed with increasing success.

Squadron Leader R. B. Maycock (Grain) disagreed with the author on one or two small details. Although pilots were able to agree on the fundamental aspects of the seaplane, they were still at war with regard to some of the lesser details which, under certain conditions, largely affected the effectiveness of the seaplane. In regard to some of the details, he had never heard of two pilots holding quite the same opinion. In summing up the paper, it seemed to him that perhaps the author had laid more stress on the impracticable side of the seaplane than on the practicable side. In that, of course, he was wise, because the meeting was held for the purpose of discussing the improvement of the seaplane and for general propaganda towards that end, but he believed that the seaplane was not really so severely handicapped in some respects as it might appear, especially when considering it in relation to its heavy sister, the bombing aeroplane. Generally speaking, he believed there was very little aerodynamical difference between them, even in rough weather. There were, however, three important factors about the seaplane which were disadvantageous as compared with the aeroplane. These were, first, the difficulty of taking off in a rough sea and wind, as against an aeroplane taking off from an aerodrome in a similar wind. The second was that the flying boat of the future must be moored out, and the question of corrosion and the weather effects on the machines if wholly exposed was a serious matter, and must always be put against it in comparing it with the machine which was housed, and it was a point which needed considerable attention. Every effort should be made to stop this corrosion. The third difficulty to be got over was the lack of power to enable the machine to overcome the set-back which the author had described when taking off when there was a heavy sea running. A power reserve is necessary to drag the machine on to the step and keep it there until flying speed is attained.

When these difficulties were overcome, and there seemed to be no reason why they should not be, the seaplane would be on absolutely the same level as the aeroplane of the same size, and would be an extraordinarily useful military and commercial machine. Landing in a protected harbour in any weather was no more difficult than landing an ordinary aeroplane on an aerodrome, and he did not think the sea would ever be sufficiently rough to prevent landing in a suitably-protected site. Forced landings on a rough sea were no more hazardous than forced landings by aeroplanes in difficult country or over London ; it was a matter of luck and good judgment whether the machine survived, and the risks were fairly equal. Personally, he would sooner land at sea than in London. There was another difficulty in comparing the seaplane with the land machine, and that was that an ordinary aeroplane could land at an aerodrome, and if it had no shed to go into it could be pegged down. The seaplane, under similar conditions, must have a buoy or anchor, and he would like to illustrate by means of slides the strength and weight of the moorings required. The first slide showed a line of moorings, and there were nine machines moored. Three of the machines, during a gale of 102 miles per hour, blew away, carrying their moorings with them. They were single moorings and had one-ton sinkers at the bottom; but the machines had picked the moorings up and had deposited themselves and the moorings on dry land.

They had landed upside-down, resting on the struts, and were broken up.

It must be remembered that in gales of 60 m.p.h. or over the drag on the moorings is approximately equivalent to the lift of the planes and therefore the strength of the moorings must be out of all proportion to the size and weight of the machines as calculated for surface craft. Two further slides were shown depicting the broken machines.

As to hard work on the part of the pilots when controlling large flying boats in the air, pilots had certainly talked of hard work under some conditions, but one heard the same thing from other heavy machine pilots. Once in the air the effort expended in lateral control is normal. The rudder, however, requires more owing to aileron drag and shortness of tail. With regard to the author's remark that his suggestion as to separate controls worked by individuals on the orders of the captain had never been tried, he (Squadron Leader Maycock) thought there can only be one pair of hands in control, because the pilots "felt" the position of the machine through the controls rather than "saw." Probably that was the reason nobody seemed anxious to try the suggestion. Dual control is fitted so that pilots can relieve each other during long flights and is seldom used in bumpy weather and then only in emergencies. Referring to the author's remark that "The difficulties of a forced aeroplane landing in unsuitable country have not as a rule any parallel in a forced landing at sea, although other troubles arise in bad weather," Squadron Leader Maycock did not see exactly what was meant by that. He believed the author meant that the risks of landing in a rough sea were not so bad as landing in difficult country, but that the difficulties which followed were a serious matter for the machine; once having landed at sea, then the trouble started. He himself considered that unless one had extraordinarily bad luck, where both engines had absolutely broken down, or the weather was so bad that one was being blown away from land and one had to give up all hope, it it would be better to land at sea; he would prefer to be at sea anyway. He was an enthusiast on seaplanes and perhaps this helped him in the belief that there was not very much between them and heavy aeroplanes. He recognised, however, that there were still quite a number of things which needed improvement, such as, for instance, the question of corrosion, especially if metal hulls were adopted; there were a number of these points to be met before they could really consider them to be of real value for military and commercial purposes.

Mr. W. O. Manning said that one thing which stood out clearly from the lecture was that what was wanted was a seaplane which could be controlled on the water as would a motor boat, would fly as well as the best aeroplane produced, and stand a big sea, and he thought there was hope that what was wanted could be obtained.

It is difficult to see how as much as 400-600lbs. of water can be absorbed in a flying boat, the larger of these weights being equivalent to over nine cubic feet, and it is difficult to avoid a suspicion that possibly there was some water left in one of the compartments of the hull when the machine was weighed.

Although we are very far from the ideal seaplane there are developments in progress which show promise of overcoming the difficulties referred to by the lecturer. Corrosion of metals especially should soon be a thing of the past.

He would like to emphasise the great value to seaplane design of the results obtained by the cruise of the four flying boats, under the command of Squadron Leader Maycock, and to express a hope that such cruises may be repeated.

In conclusion he would like to thank the lecturer for a paper which fills in a serious gap in the previously published information referring to seaplanes.

Mr. G. S. Baker (National Physical Laboratory) said it was an advantage to those who had to deal with the design of these boats to hear what the users had to say. With regard to the author's statement that a long bow to the hull was a necessity in order to avoid diving if alighting steeply, he would like to know

what the author considered was the minimum angle at which a seaplane should come into contact with the water in a bad case, in order to avoid diving. Of course they could not cater for any angle, but this angle should be known more or less clearly, if a pilot is to avoid disaster when he made a not too serious mistake. He congratulated the author on the apparent alteration in his opinion since his (Mr. Baker's) paper was read before the Society a fortnight previously. He had thought, from the author's previous remarks, that he did not agree with him (Mr. Baker), but his opinion appeared to be moving in favour of the large flying boat in the future. The author, in speaking of controls, had mentioned using a "Servo" motor. This involved a good deal of extra weight. He had recently seen a rudder on a 10-knot ship worked with a wire no bigger than $\frac{1}{8}$-inch diameter, and he believed German designers were beginning to adopt the same system for their controls. He believed something on those lines would be devised for controlling large machines in the future, instead of the brute force arrangements in use at the present time.

With regard to the reference made to flexible and non-flexible hulls, these terms were wrongly used. There was no definite division of flexibility and non-flexibility between the "F" boats and the "P" boats in many respects. The difference between them came in in the way the hull was built to absorb forces. He had seen the bottom of the so-called rigid "F" boat, panting in and out a quarter of an inch, and he had been out on a "P" boat many times and had never seen such a movement, although that hull was called flexible and the "F" boat hull was called rigid. The difference lay in this, that whereas in the "P" boats any force was rapidly absorbed throughout a fairly *large area*, in the "F" boats that force was taken up by a comparatively small area, resulting in the production of big stresses. The forces were local and concentrated, but neither of these hulls broke due to any weakness or to want of flexibility of the main structure.

Squadron Leader R. M. Hill said that the author had startled him a little, because he had taken as the most important improvement in design the improvement of air control. There was no doubt in the mind of the landplane pilot that on the whole the landplane did not suffer from such disabilities as the seaplane; and that, because the designer did not have to cope with so many difficulties, the landplane was a little easier to handle generally.

In a seaplane the high thrust line and low C.G. contributed to a large change in longitudinal trim as the thrust varied; when, therefore, the pilot opened or closed his throttle he had to make an anticipatory movement of the control to prevent the seaplane suddenly pitching. With regard to the depressing tail, he thought it would be found that if they made the aeroplane or seaplane longitudinally stable, the C.G. would of necessity be relatively far forward; the tail plane would then have to be at a negative angle to the main planes, so that when the slip stream acted on it the forces tending to neutralise the effect of variation in thrust would be in the right direction.

Lately it had been found possible to balance the controls of large aircraft in new ways and thus to make them very much better than it was formerly anticipated they could be made. He expressed his great interest in the paper, and felt sure that the extraordinary lucidity which the author had shown in getting right at the heart of the difficulties made one confident that the development of the seaplane was in the very best hands.

Captain D. Nicolson said that Wing Commander Cave-Browne-Cave had laid great stress on the sagging of the tail of some of the flying boats; this, he said, could easily be prevented. When he was assisting the late Major Linton Hope in the design of some of the hulls, they had saved as much as 500lbs. on the N.4 and about 250lbs. on the P.5 from the original weight given. This was partly done by cutting down the stringers to a minimum, and these were tapered

rather fine at the after end. Again, more stringers should have been carried further aft, the extra weight would have been very little and well within the calculated original weight specified.

With regard to towing, Captain Nicolson stated that he had had a good deal of experience in towing hydroplanes and flying boat hulls, and found the marine practice best by having a power boat behind the vessel being towed, as by this method all tendency to yawing would be stopped. If drags were used and a beam sea running, the drags always found their way over to the weather side, thus pulling the tow out of alignment.

Captain Nicolson stated that the amount of water absorbed by the hulls, as in the example quoted by Wing Commander Cave-Browne-Cave, was abnormal, and if the boats were not leaking it proved conclusively to him that the hulls were not kept properly. If the hulls were to be kept in very good condition the old varnish should be scraped off, or properly rubbed down and a new coat applied every month, or at least every two months. If this was too much work for the stations then the boats should be veloured once a year.

With regard to anchors, Captain Nicolson made many suggestions, amongst which he stated the canvas sea anchor was very light and yet effective.

There was one point not mentioned by the author, but which was of great interest, and that was the question of marine growth; and he would like to know if trouble had been experienced with such, as he had heard of many cases where in foreign waters the boats would hardly become unstuck after lying out for some time. If Commander Cave-Browne-Cave was faced with difficulties in this direction, Captain Nicolson thought the problem could be easily solved for home waters and would give the benefit of his experience if necessary.

Mr. A. Q. Cooper, speaking on the question of getting off from a rough sea, said that was very important, especially from the Service point of view. A machine which would be at sea perhaps for days could not keep in the air all the time, and if the machine could only get off from a moderate sea then its usefulness must be very materially restricted. He considered that a machine could get off a rough sea if it had sufficient power to rise quickly enough, and a machine which could use abnormally high power when getting off was very desirable.

Colonel S. Heckstall Smith, speaking as a sailor, referred to the author's remarks as to the value of having a central engine-room, his objection, however, being the danger of fire. In the very early days, when he was building motor boats and auxiliary motor boats, there was trouble in that way, and they had entirely got over it by keeping the carburettors outside the hull altogether. The difficulty of warming was got over by running the exhaust pipe alongside the induction pipe.

By keeping the carburettors outside and using long induction pipes there was no danger of fire, and further, he daresay it was luck, they actually got more revolutions out of the engine than when the carburettors were close to the engine. It is quite probable, however, that an actual improvement in power with some engines might be obtained, due to the large volume of atomised mixture on which the engine has to draw. The induction pipe which in fact gave the improved result was 15 feet long. The question of mooring was also interesting to him as a sailor. He had always found, with small racing yachts, say of ten tons displacement, where they wanted to keep weights down, that it was necessary to carry two small anchors to use in a tide-way rather than trust to one big one, because one anchor allows the ship to yaw, and swinging with the wind and tide may cause the anchor to trip. The yacht, of course, drops the anchors in the line of the tide and moors between them, frequently using a swivel to the anchor ropes or chains. Two small anchors might well be more

effective than one of the same weight as the two. With regard to keeping the hull well pumped out, he had found a form of diaphragm pump very light and effective and he had in fact installed such pumps in the first motor lifeboat which he had fitted with an engine—in this case, however, as he used two-cycle engines they lent themselves conveniently for the purpose, as all that was necessary was to couple the diaphragm pump direct to the crankcase, which gave required pulsation.

Squadron Leader Wright (Air Ministry) said that the term seaworthiness was used rather loosely. In his opinion it seemed necessary to examine the meaning of this term more closely. In order to grasp its significance it is necessary to sub-divide the term into five headings, namely: (1) Taxiing under power; (2) drifting at sea with one or more engines stopped; (3) resting at moorings; (4) taking off; (5) landing. Each one of these sub-divisions required special consideration in the design of a machine; and the behaviour of a seaplane when on the water depended on a careful compromise of the requirements of these five conditions. Dealing with the first, "Taxiing under power," he said that the chief problem when taxiing in a rough sea was to avoid damage to propellers. Condition 4, "taking off," required propellers which would give a high efficiency at relatively low air speeds to enable quick acceleration on the water to be possible, but high propeller efficiency under these conditions required larger propeller diameters than would be necessary in an equivalent land machine; but, on the other hand, a large diameter meant small water clearance in the centre of thrust was not to be unduly high.

It will be seen, therefore, that a very careful compromise between conditions one and four has to be made. With regard to "drifting at sea," it was generally necessary to employ a sea anchor to keep the machine's head to wind as their ability to withstand heavy seas under these conditions depended to a great extent on the maintenance of the seaplane in this position. Riding at moorings was a simpler problem than might be imagined, and Squadron Leader Maycock had spoken of the ability of seaplanes to ride out a gale provided that adequate precautions were taken to prevent them flying at their moorings.

"Taking off" required consideration of two conditions, namely, (A) taking off in a sea with a strong wind blowing and (B) taking off in a swell with no wind. Condition (B) was by far the most difficult state of affairs. Low weight per h.p. to provide quick acceleration is necessary to achieve this successfully. Landing in a rough sea called for a planing bottom of sharp "V" in the cross section if risk of damage to the hull is to be avoided. Air controllability with engines off is also an important factor as it is necessary to place the machine in the best position just before contact with the water is made. It is hoped that an opportunity will be available to analyse the problem of seaworthiness and obtain such data as is possible to enable the task of the designer in making this compromise easier.

Wing Commander Cave-Browne-Cave, replying to the discussion, said Mr. Narbeth had drawn a clear distinction between the seas experienced in the North Sea in winter and those experienced during the cruise last summer. The seas met with on that cruise, as a matter of fact, were really big. The weather was most difficult, there being bad mists and big seas and every form of unfavourable weather, so that from the point of view of demonstrating the seaworthiness and general suitability of the boats under all conditions the weather did everything required. Squadron Leader Maycock had drawn attention to the fact that he (Wing Commander Cave) had been pessimistic about the qualities of flying boats. One did not address the Royal Aeronautical Society in a mere eulogy; what interested such a Society was a discussion of the difficulties which existed and the ways of trying to get over them. This led to valuable suggestions like some of those made that evening. He had said in the paper that flying

boats were heavy to control. They certainly were heavy to control "in bumpy weather," a reservation which Squadron Leader Maycock had apparently missed. As to landing in unsuitable country not being parallel to landing in a rough sea, both seaplanes and aeroplanes got into difficulties in bad weather, but an aeroplane often got into difficulties in the case of a forced landing even in the best weather; for instance, when landing in a small field.

Mr. Manning had explained that what was wanted was something which was as good as a motor boat on the water and was a perfect flying machine. He himself did not think a motor boat was good enough; some motor boats were terrible in a sea in which existing seaplanes were quite comfortable, and he was perfectly certain that the Development Squadron could not have taken any motor boat through what those flying boats experienced. We wanted to go further than that. Mr. Manning had also asked whether, in regard to the water taken up, the compartments in the bottom were clear. They were as clear as it was possible to make them under practical conditions. They could not dry them out completely, but there was no casual water lying about.

Mr. Baker had asked what was the minimum angle at which it was thought desirable that the boat should be able to land. That was a very difficult question to answer, but he would endeavour to get a consensus of opinion among pilots in that connection. It was exactly the kind of point he certainly would not presume to pronounce upon. Mr. Baker had pointed out that a long bow was not really necessary in order to avoid diving. But why then did the long bow exist? From the flying point of view, very great benefit would result from reducing the length of bow. The author pointed out that shock could only be absorbed if there was deflection and he inferred from Mr. Baker's remarks that the deflection was less concentrated in the "P" than in the "F" type. He was very sorry to find that Mr. Baker had misunderstood what he had intended to convey at the last meeting of the Society. Mr. Baker apparently thought that he (Wing Commander Cave) did not think that the big flying boat was the type of big machine which would persist. As a matter of fact, he most certainly did think that it would. He believed that the flying boat was not only the big type for future marine aircraft, but he thought it was the most promising form of big aeroplane of any sort or kind. The point on which he had differed from Mr. Baker in the last lecture was whether one would make one's ocean passages in a flying boat or in an airship. The Society knew that he had definite views about airships. He did think that for passages of 1,500 and 2,000 miles or more the advantage was entirely with the airship. It was most interesting to hear what Mr. Baker had said about the rigidity of both types of boat at the step. That was a point that he had never heard expressed by any authority previously. It certainly appeared correct.

Captain Nicolson had mentioned the question of the stiffness of the tail of the Linton-Hope hulls. He quite realised that they could make the tail as stiff as they liked, but at the expense of weight. His contention was that if they carried the tail on a fuselage, or between two fuselages, they would make the tail rigid for less weight than would be necessary to give the same rigidity if they supported it overhung above the top of the hull. Captain Nicolson had also suggested that a motor boat towed astern of a flying boat would make her easier to tow. That meant using two motor boats, and he believed that every seaplane pilot regarded a motor boat in the neighbourhood of his machine as a potential danger. As to the types of anchors, the sea anchors used at present were perfectly satisfactory, and he did not think they could be improved very much. They held well and were of purely nominal weight. Those which held on to the sea bottom and prevented the machine moving at all were the difficulty. He had been badly startled when Captain Nicolson had suggested that the machines should be varnished once a month. With regard to scraping off the old varnish

before putting the new on, it was not practicable to do that on the inside of the boat. A great deal of water, he believed, was absorbed from the inner surface. Making the boats watertight would only be achieved by a method which could be carried out with a reasonable amount of care during the progress of actual operations, *i.e.*, any complicated business such as re-varnishing once a month was absolutely out of the question. All varnishes were permeable to gradual diffusion of moisture as was shown by the way the wood swelled and became tight. The majority of the absorbed weight was therefore unavoidable in a wood hull. So far as he knew, no trouble had been experienced with marine growths on the bottoms of the boats. (Later Note.—Considerable trouble was experienced in parts of the Mediterranean at certain seasons. Scraping once a month had sometimes proved necessary.)

Mr. Heckstall Smith had made suggestions which brought out the value of such discussions. His method of putting carburettors outside the boat appeared to him (Wing Commander Cave) a complete answer to the difficulty which was looked upon as a very serious one, that of putting the engines inside the boat. The fact that Mr. Heckstall Smith had managed to make a 15ft. induction pipe work was most interesting. He agreed also with regard to the two small anchors, and thought that probably the reason the seaplanes did not carry two small anchors instead of one was the fact that a tangle might exist in the cockpit in the absence of a proper arrangement of hawse pipes.

Squadron Leader Wright had spoken with regard to seaworthiness. That was an extremely complicated matter, and the only way of tackling it systematically was by dividing it into sub-headings in such a way as had been mentioned. They had had two or three discussions at Grain on the question of seaworthiness, and on every occasion those discussions could have gone on so long as the time lasted. It was a most complicated and interesting business.

A very hearty vote of thanks was accorded Wing Commander Cave at the conclusion of the discussion.

Mr. Oswald Short (*communicated*): Owing to being so far from London I regret being unable to attend this very interesting lecture, but having read the advance proof, I should like to congratulate the lecturer on the fact of his having presented in a simple and straightforward manner information which is of the greatest value to designers of flying boats.

I should like to make the following comments:—

I agree with the suggestion made by the lecturer that a rigid hull of the same external form as the P.5 might have done just as well and would not have led to trouble with the step. One cannot help but feel that any considerable movement of the hull bottom, considering the method of manufacture, must ultimately lead to trouble by chafing of the waterproof lining and by loosening of the fastenings in lateral stiffeners. The trouble with the rigid step is a case in point. To absorb the vertical momentum of a boat on impact with water in a satisfactory manner, a considerable amount of cushioning movement is necessary, and there appears no reason why the whole of the upper structure should not be resiliently attached to the boat hull in a similar manner to the attachment of seaplane floats.

With regard to the floating dock referred to by the lecturer, I am very gratified to note that a scheme which is very similar to one I urged upon the Admiralty and of which I prepared plans and a working model so early as 1913, and which was exhibited to Mr. Winston Churchill and others in that year, has at last been carried into practice and has proved its usefulness.

LEONARDO DA VINCI'S MANUSCRIPT ON THE FLIGHT OF BIRDS

BY IVOR B. HART, B.SC., A.F.R.AE.S.

Introduction

The manuscript by Leonardo da Vinci, of which a complete English translation is here offered (so far as the writer is aware, for the first time), is known as the "Codice sul Volo degli Uccelli e Varie Altre Materie" (Codex on the flight of birds and other matters). It is a small note-book of some 30 pages, and measures about 8.4 inches by 6 inches. It was written at Florence in the year 1505, between March 14th* and April 15th.† A close study of this manuscript has been made by G. Piumati‡ in a very fine edition containing an excellent facsimile of the original note-book, together with (*a*) a printed copy folio by folio of the old Italian script in which it was written, (*b*) a rendering into modern Italian, and (*c*) a French translation by C. Ravaisson-Mollien.

The reading of Leonardo da Vinci's manuscript has been a task of enormous difficulty with which is honourably associated the names of a small band of enthusiastic students, chief among whom may be mentioned J. Paul Richter, G. Piumati and C. Ravaisson-Mollien. Leonardo, from the time he was twenty years of age onwards, invariably wrote in a manner calculated to confound his would-be readers. We may summarise the characteristics of his script under four heads. (1) He wrote from right to left after the fashion of the Semitic group of languages, (2) his handwriting was of the kind known as "mirrored," *i.e.*, reversed in a manner such as would be produced by looking at a normal script through a mirror, (3) he employed an elaborate scheme of abbreviations, and (4) he omitted the use of punctuation. It is accordingly much to the credit of the patient workers to whom reference has been made above that, in spite of these difficulties, the writings of this great genius of the Italian Renaissance have been rendered available to the world of science and letters generally.

It is only within comparatively recent times that the vast collection of notes and sketches accumulated by Leonardo da Vinci has been given the attention it deserves. Unfortunately circumstances were such that after his death they were lost sight of, and it was only after the lapse of centuries that they once again came to light. This loss was a serious misfortune to science. Leonardo's work was in itself so fruitful and varied, and his outlook on nature was so vastly superior to those about him, that if only those who followed after him could have had access to his writings, and to his many anticipations of later discoveries in different fields of intellectual activity, there is no doubt that the course of scientific history would have been materially different in a number of important directions.

When da Vinci died at Amboise in the south of France, in 1519, he left a will bequeathing his manuscripts, instruments, paintings and sketches to the friend of his old age, Francesco Melzi, who was with him at his death. Melzi returned to his home in Milan, carrying this bequest with him. Here, in his villa at Vaprio, the manuscripts were guarded with tender and jealous care for the next

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* Amoretti, "Memorie storiche su la vita, gli studi, e le opere di Leonardo da Vinci," Milan, 1840, p. 99.

† See M.S., folio 18 v., below.

‡ "Codice sul Volo degli Uccelli e Varie Altre Materie," Publicato da Teodoro Sabachnikoff. Trascrizioni e note di Giovanni Piumati, Paris, 1893.

fifty years. Melzi however died in 1570, and it is from this time onwards that the tragic dispersal of the manuscripts may be said to have taken place. Attempts which, during Melzi's lifetime, had been made to obtain access to the note-books (notably by Alphonso I. of Ferrara and by Vasari, Leonardo's earliest biographer), and which had failed, were now renewed, this time with success.

Francesco's heirs were not interested in science, art, or letters, and when the family tutor, Lelio Gavardi di Asola, conceived the idea of disposing of some thirteen volumes of the manuscripts, he met with little difficulty. However, the Grand Duke Francesco of Florence, to whom it was intended to sell these volumes, died at this juncture, and Lelio Gavardi took them instead to Pisa. Fortunately, the man whom he approached, Ambrosio Mazzenta,* dissuaded Gavardi from pursuing his dishonest intentions, and the latter left the volumes with Mazzenta to be restored to the Melzi family. Orazio Melzi, the rightful owner, in his lack of interest, however, not only permitted Mazzenta to keep the volumes as a reward for his trouble, but told him further that he could help himself to any of the remaining bundles of matter lying about the attics at Valprio.

Others, too, had heard of the spoils to be gathered in from these attics, and they readily availed themselves of Orazio's free invitations of "help yourself." Amongst those seeking to acquire Leonardo's manuscripts was Pompeo Leoni, a sculptor and friend of Philip I. of Spain. It was his plan to seek favour from his royal master in exchange for these manuscripts, and accordingly he approached Orazio Melzi with promises of reward from King Philip if he would secure the return of the thirteen volumes given to Mazzenta. As it happened, Ambrosio Mazzenta had by now (1590) taken vows with the Barnabite Order, and had passed on his thirteen volumes to his brother Guido. It was Guido, therefore, who was approached by Orazio Melzi, and as a result seven of the volumes (one of which contained the small note-book on the flight of birds) were returned and handed on to Leoni. Of the remaining six, three were subsequently acquired by Leoni on the death of one of the Mazzentas, and another volume was given in 1603 to Cardinal Federico Borromeo, the founder of the famous Ambrosian Library at Milan (1609). The remaining two have long been completely lost sight of, although we know that one of them was given to Ambrogio Figino, a painter, and has been traced through various owners up to 1759; and the other was given to Duke Charles Emmanuel of Savoy, and was probably destroyed in an outbreak of fire at the Royal Library at Turin about the year 1667.

Actually Pompeo Leoni did not give his manuscripts to King Philip II. of Spain. Not only did he keep them himself until his death, but he went so far as to cut out portions of them to form, together with a number of loose drawings, a single large volume which, on account of its size (it contains 402 sheets and more than 1,700 drawings and sketches), he called the "Codice Atlantico" ("Codex Atlanticus"). Leoni died in 1610 and the manuscripts passed into the possession of his heir, Cleodoro Calchi, who in his turn sold some of them, including the Codex Atlanticus, to S. Galeazzo Arconati in 1625. Arconati kept these until 1636, in which year he presented to the Ambrosian Library at Milan the Codex Atlanticus together with eleven other volumes of da Vinci's manuscripts, thus making, together with the original volume presented by the founder of the library in 1603, a collection of thirteen volumes. To these a further one was added in 1647 by Count Orazio Archinto. One was probably subsequently stolen, since its description coincides with that of a volume bought in 1750 by Carlo Trivulzio. The period of restoration may now be said to have begun.

In Arconati's deed of gift, making over these volumes to the library, we find

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* The chief source of our knowledge regarding the history of these manuscripts is derived from "Alcune memorie di Giovanni Ambrogio Mazzenta intorno a Leonardo da Vinci e a suoi manoscritti, con illustrazioni del Prof. Gilberto Govi" (Il Buonarroti).

the first authentic record of the existence of the manuscript on the flight of birds. Inside the cover of the third volume of the gift we read: "At the end of this book there is another little volume of various mathematical figures and of birds of eighteen pages sewn in the same page in parchment."*

For the next chapter in the story of the wanderings of these manuscripts we come to the year 1796, when Napoleon Buonaparte, in his capacity of chief of the French army in Italy, commandeered various works of art, including the thirteen volumes of Leonardo's manuscripts, and these were transported to the library of the Institute of France in Paris, where according to the official records they arrived on the 25th of November.

Here the volumes were carefully examined by Venturi and to him the world is indebted for the first real description of the manuscripts.† For purposes of reference, Venturi assigned to each manuscript a capital letter beginning with A, and this nomenclature has since received general acceptance.

As a result of political changes, in the year 1815 representations were made to France on behalf of Lombardy for the return of da Vinci's manuscripts to Milan. For some reason or other, although France consented to this restoration, only the Codex Atlanticus (which alone of all these volumes was housed in the Bibliothèque Nationale instead of in the library of the Institute of France) was returned to the Ambrosian Library, and the remainder have been kept in Paris ever since.

The particular volume to which we have already referred as containing an appendix of the note-book on the flight of birds was lettered B in Venturi's series. Venturi, however, considered the note-book of sufficient importance to be regarded as a fourteenth volume in itself and he lettered it N accordingly.

The subsequent history of our small note-book is interesting in the extreme. In the year 1848 it was discovered that certain manuscripts, and amongst them the appendix to manuscript B, were missing. That they were stolen is fairly certain, and that the culprit was Prof. Giacomo Libri is indicated first by the fact that prior to 1848 he had had frequent access to these manuscripts and, secondly, that he was in undoubted possession of them in 1867. In December of that year Count Giacomo Manzoni of Lugo, whilst on a visit to Florence, was shown the manuscript amongst others "acquired" by Libri, and bought it from him a year later. Libri also disposed of some of the manuscripts to Lord Ashburnham in England, but these latter were returned to France in 1888.

On the death of Count Manzoni, in 1889, the note-book "On the Flight of Birds" passed to his heirs, and from them it was acquired in 1892 by M. Theodor Sabachnikoff, and the volume associated with his name and that of Piumati is the honourable result.

The note-book has since passed into the possession of the Library of Turin.

The genuineness of the manuscript on the flight of birds is beyond all dispute. Not only does it bear all the unmistakable characteristics of Leonardo da Vinci's general work, script, mode of presentation and style, but the note-book is autographed with the writer's name on the cover. Such doubts as have been thrown by M. Ludwig,‡ not so much upon the authorship as upon the originality of the paging as it at present exists, have been effectively disposed of by Piumati.§ Although a second set of page numbers has been written in by someone subsequent to the original writer, Leonardo's numbering remains intact and unmistakable.

* I Manoscritti di Leonardo da Vinci, "Codice sul Volo degli Uccelli," etc., Pubblicato da Teodoro Sabachnikoff. Trascrizioni e note di Giovanni Piumati (pp. 28 and 29), Paris, 1893.

† J. B. Venturi, "Essai sur les ouvrages physico-mathematiques de Leonard de Vinci," Paris, 1796.

‡ M. Ludwig, "Leonardo da Vinci, Das Buch von der Malerei," II. Band, p. 5.

§ Sabachnikoff, "Sul Volo degli Uccelli," etc., p. 35.

The reason for this duplication of numbering is to be found in the fact that the manuscript on the flight of birds is not complete. Comparison with Venturi* and Amoretti,† who were the first translators of some of the passages from Leonardo's works, show clearly that pages which were in the note-book originally are not there now. The pages in question are folios 1, 2, 10 and 17. Page 18 was also originally missing, but happily was rediscovered in England and secured by M. Th. Sabachnikoff in time to be included as an appendix to his book.‡ Its identity was assured from its dimensions and general agreement in every respect with the remainder of the note-book. The number of the page, absent through mutilation, has been identified from references to Venturi and Amoretti.

In submitting this English rendering, the writer desires to record the valuable assistance of his colleague, Mr. E. D. West, who ungrudgingly placed his knowledge and advice at the writer's service. It should be remarked that although the primary purpose of the translation is to further the study of Leonardo da Vinci's contributions to aeronautical science, every note in the manuscript has been included, even though not relevant to this primary purpose. Not only is this done for the sake of completeness, but it is also hoped that the result may be of interest to students of scientific history in general, and to students of Leonardo da Vinci in particular. It is a matter for regret that it has not been found possible to include all the diagrams and sketches in the original manuscript. Nevertheless, all those which seriously bear upon the text are given, and for assistance in the preparation of these the writer desires to express his indebtedness to his colleague, Mr. W. Laidler.

The writer's grateful thanks are due, finally, to Dr. Charles Singer, at whose suggestion these studies of Leonardo da Vinci were undertaken, and whose encouragement and helpful advice has contributed very materially to their achievement.

"ON THE FLIGHT OF BIRDS."

TRANSLATION OF A MS. BY LEONARDO DA VINCI.

NOTE.—*In the translation, text enclosed in brackets thus* [ ] *has been interpolated by the author; text enclosed in brackets thus* ( ) *denotes matter written in by Leonardo and then erased.*

[Cover—Interior [1].]

To stamp medals.—Boil emery mixed with brandy, or iron-scales with vinegar or ashes of walnut leaves, or ashes of straw finely triturated.

The diamond is crushed enveloped (within) in lead, and beaten with a hammer, and the lead spread out several times, and refolded (and *r*) and kept wrapped in paper so that the powder be not spilled and then melt the lead and the powder will rise to the surface of the molten lead, which may be then rubbed between two steel plates, so that it powder well, and then wash it with nitric acid, and the blackness of the iron will dissolve and leave the powder clean.

Emery in large pieces is broken by putting it on a cloth folded (in) many times and then striking it sideways with the hammer; thus it will break into flakes, little by little and then is easily crushed; if you put it on an anvil, you will never break it, being so large.

Who powders enamel, should do so on plates of tempered steel, with a steel pestle, and then put it in nitric acid, which dissolves all the steel which is used up and mixed with this enamel making it black, so that it remains purified and clean; if you powder it on porphyry, this porphyry wastes, is mixed with the

* J. B. Venturi, "Essai sur les ouvrages," etc., p. 18.
† Amoretti, "Memorie storiche su la vita, gli studi, e le opere di Leonardo da Vinci," Milan, 1804, p. 99.
‡ Sabachnikoff, p. 147.

enamel and spoils it, and nitric acid will never remove it (*s*), because it is not able to dissolve such porphyry.

It you wish to make a beautiful blue colour, resolve the enamel made with tartar and then remove the salt. Brass vitrified makes a beautiful red.

[Fol. 3—*r*.]

Instrumental or rather mechanical science is very noble, and useful beyond (*p*) all others, seeing that, by its means, all animated bodies which have movement, perform their operations; which movements proceed from their centre of gravity, which is situated at the centre, except with unequal weights, and the latter has paucity or abundance of muscles, and also, lever and counter-lever.

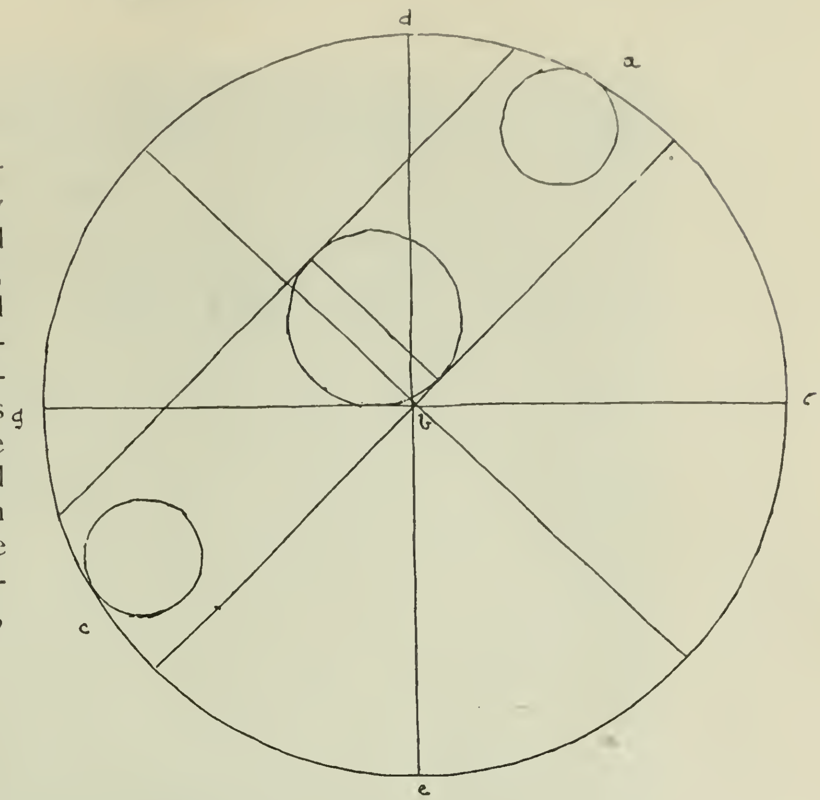


FIG. 1.

Here the balance *a b c* [Figs. 1 and 2] has more space in *b a* than *b c*, and it would appear that it also, with weights attached to its extremities would, after several oscillations, stop at the place of equality.

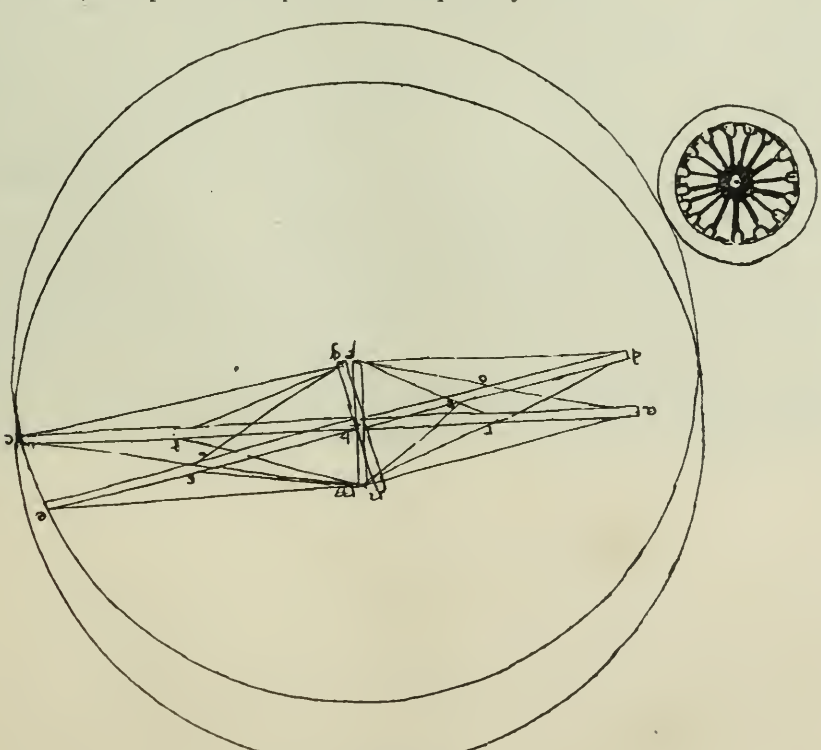
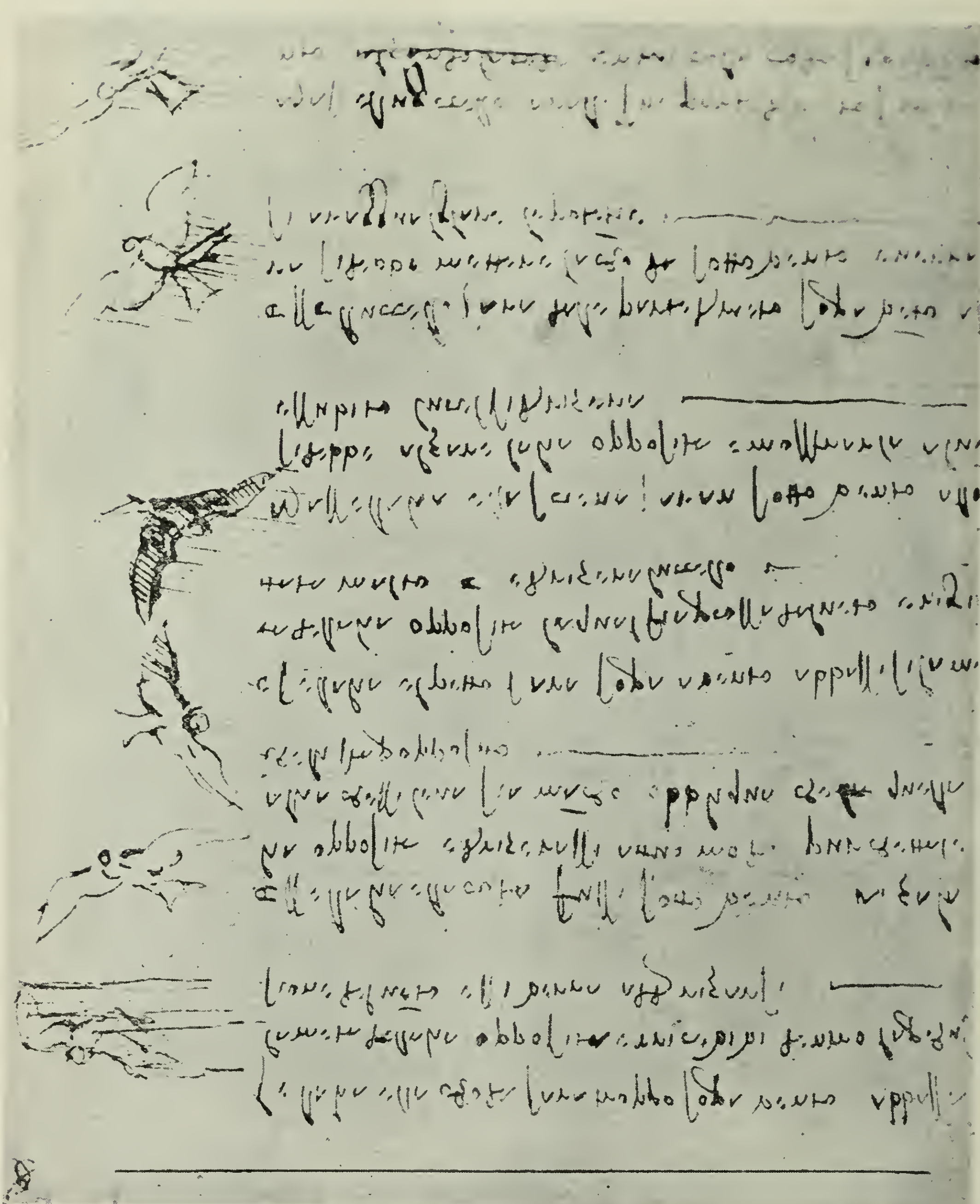
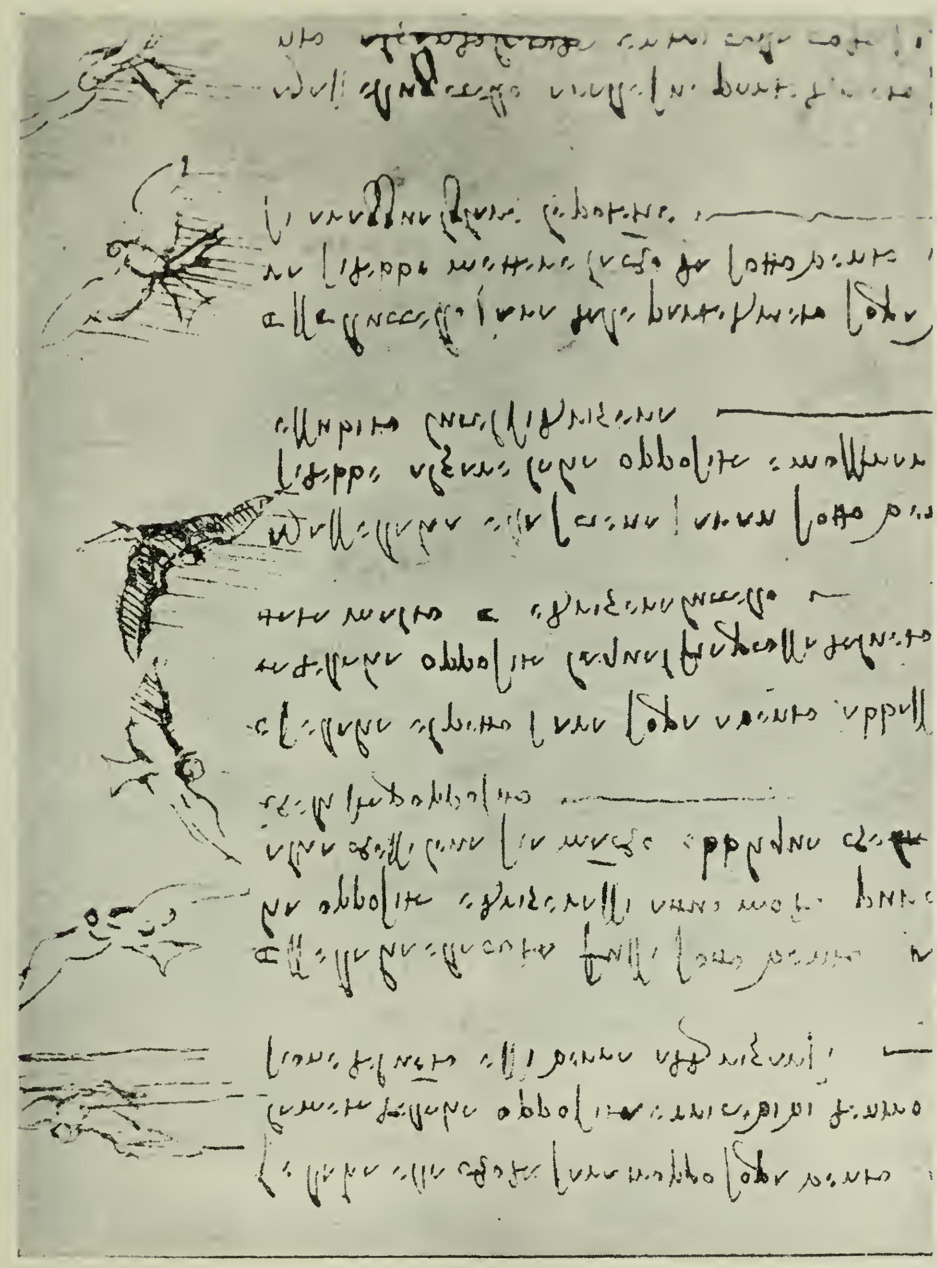
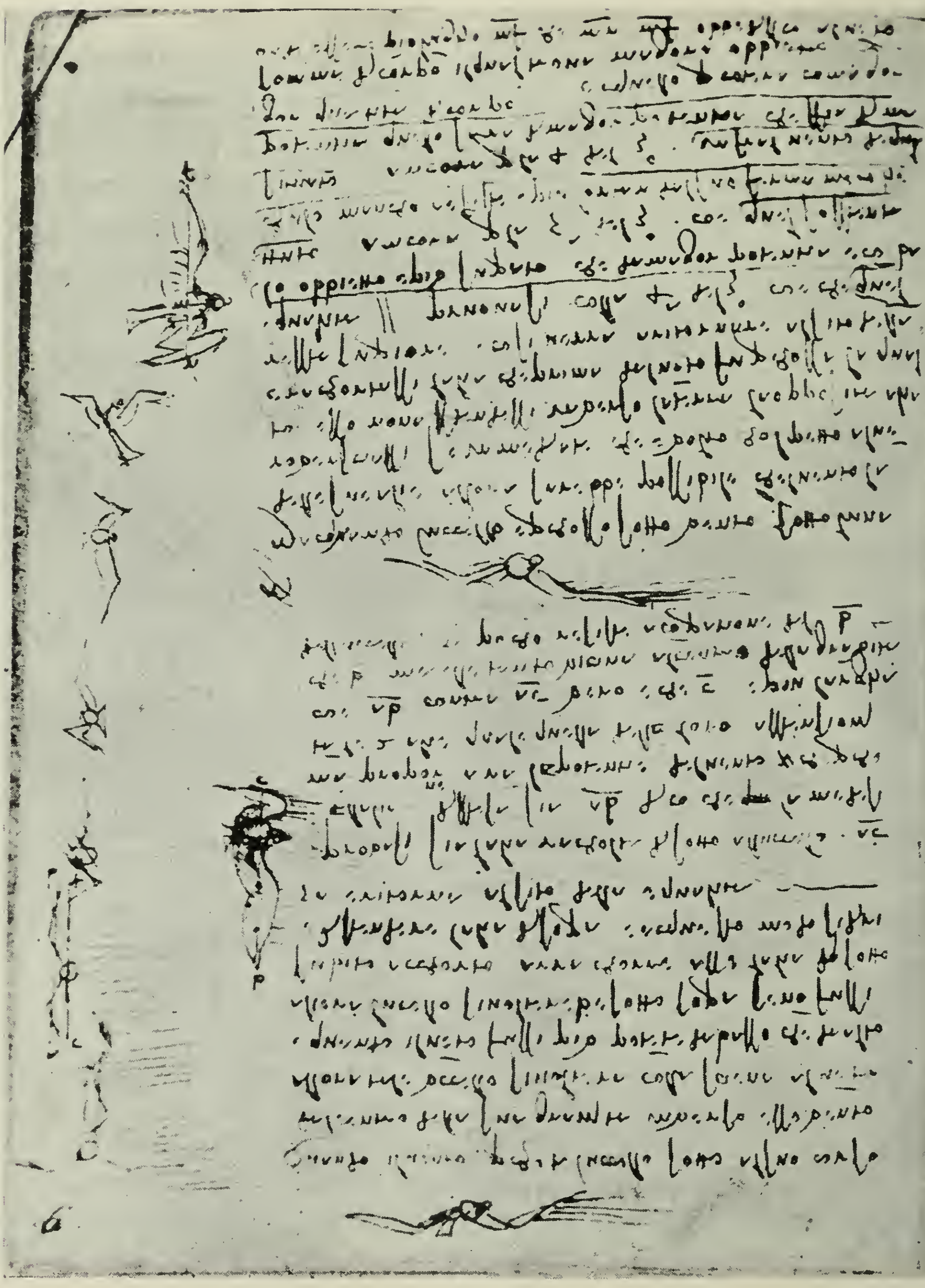


FIG. 2.







[Fol. 3—*v*.]

Different figures give in their obliquity different weights.

(If the rod (equipoised) is (*mo* fixed) able to revolve with one of its extremities round its other extremity. . . . The end of the rod will be prevented from revolv(ing) round its other extremity, to which will be fixed a straight cord, which will be attached to the opposite end under the centre of the above mentioned revolution.) At the end of this rod circular movement will be prevented round its other extremity by this cord, which in a straight line is fixed under the centre of the said circle and is attached to the extremity of the rod. As if the rod were the line *p q* [Figs. 3 and 4] and the end which is prevented from moving in a circle *q m* be the end *q*, and the straight cord attached under the centre (of) the circle, be *o q*, I say that the end (*q*) *q* of the rod will never reach the point *m*, if the cord does not break.

It is proved as follows: If the rod *p q* has to move its extremity *q* to *m*, it will follow the arc *q m* because such a rod is the radius of the circle *q m s*: and the stretched cord, *o q*, will not be able to follow the extremity of the rod from *q* to *m* unless its length increases by the part *n m*, because it also is the radius of its circle *q n s*: thence it is obvious that *q* cannot move.

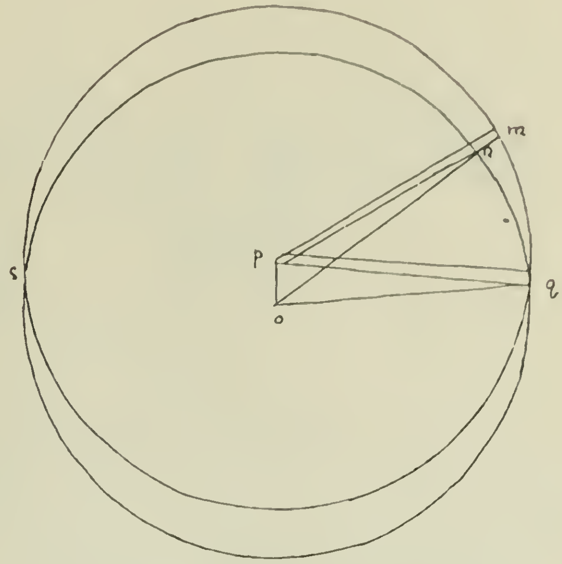


FIG. 3.

The objector says here, that the rod *p m* will bend until it makes with its extremities the length of the cord on [which it] will en(t)er.

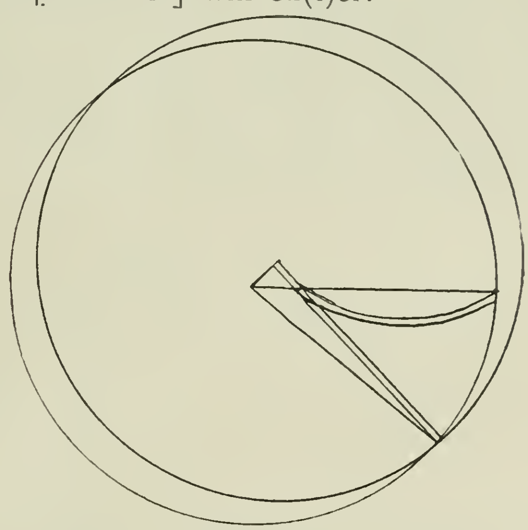


FIG. 4.

Here it is necessary, either that the cord should break so as to have the length of the rod or that the rod should bend so as to have the length of the cord.

[*The text below follows after the bottom of fol.* 4*r*.]

at *a* and gives 2lbs. at *c* because *a* itself also is centre of the revolution: therefore 1lb. at *b* pulls 2 at *a* and pushes 2 at *c* which makes 4lbs.

[Fol. 4 recto.]

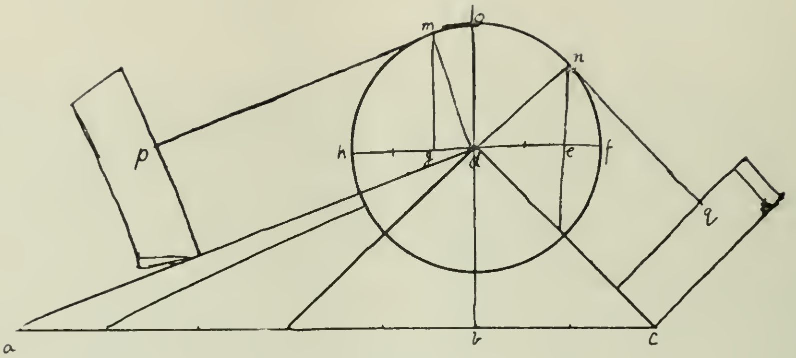


FIG. 5.

The weight q [Fig. 5], because of the right-angle n, above $d\ f$, at the point e, weighs 2/3 of its natural weight, which was 3lbs.; and is a force of 2lbs.; the weight p, which was also 3lbs., is a force of 1lb.; because of m, right-angle on the line $h\ d$ at the point g, then we have here one pound against 2lbs. And because of the obliques $d\ a$ and $d\ c$, on which the weights rest, which are not in the same proportion as these weights, that is to say, the one double the other, as the said weights their gravities change their nature, because the obliquity $d\ a$ exceeds the obliquity $d\ c$, or contains the obliquity $d\ c$ two and a half times, as $a\ b$ shows with $b\ c$, their bases, and comes to rest in the double sesquilateral proportion, and that of the weights was in double proportion, therefore the excess of the greater obliquity over the less is 1 and $\frac{1}{2}$; so that if the weights were, say 3 on each side they would rest in $d\ a$. . . .

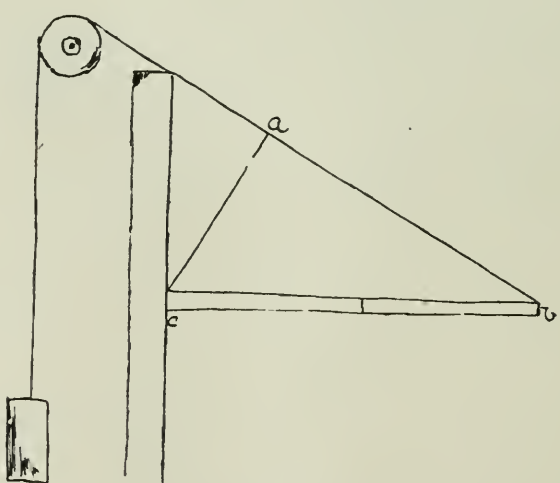


FIG. 6.

c [Fig. 6] is the pivot or centre of revolution and because $a\ c$ is half the lever $c\ b$, one pound at c gives a force of 2 pounds.

[*Continued on previous page.*]

[Fol. 4 verso.]

FIG. 7.

I ask in which part of the undersurface of the width of the bird, the wing presses the air more than in any part of the length of the wings. [Fig. 7.]

All bodies which do not bend, though they may be each in itself, of different sizes and weights, they will exert equal pressures on all of the supports that are equally distant from the centre of gravity, this centre being at the middle of the substance of such a body. [Fig. 9.]

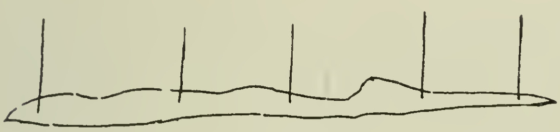


FIG. 9.

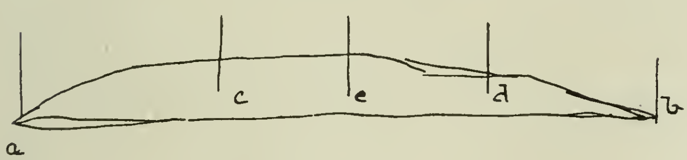
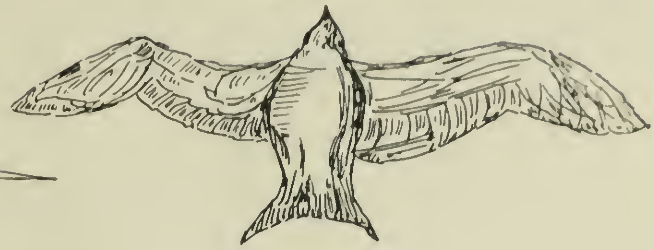


FIG. 10.

One proves how the above mentioned weight exerts equal pressure on its supports: let us assume that it is 4lbs. (I say) and that it is sustained by the support *a b*. I say that the body being unhindered in its fall except by the two supports *a b*, that these supports will sustain equal parts of this weight that is to say 2 and 2 and the same thing would apply to the 2 supports *c d*, if the 3 other supports were not there: and if the middle one at *e* only remained it would support the whole of the weight. [Fig. 10.]

FIG. 8.



Those feathers that are farthest from their points of attachment will be the most flexible. Therefore, the tips of the feathers of the wings will always be higher than their roots, so that we may reasonably say that the bones of the wing will always be lower in the depression of the wings than any part of the wing: and in the elevation these wing bones will be higher than any part of the wing. Because the heavier portion always guides the movement. [Fig. 8.]

But if the said body be flexible with different sizes (volumes) and weights still let the centre of gravity be at the centre of the substance it does not follow that the support which is nearest to the centre of gravity or to another inequality of gravity may not be more charged with weight than that which is over some lighter portions.

[Fol. 6 [5] *r.*]

A man with wings should be free from the waist upwards in order to balance himself as he does in a boat in order that his centre of gravity and that of the instrument might be able to balance and change when necessity required it according to the change in the centre of its resistance.

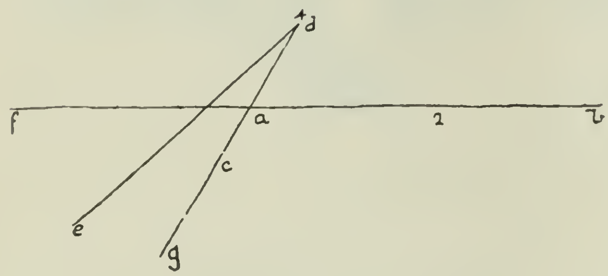


FIG. 12.

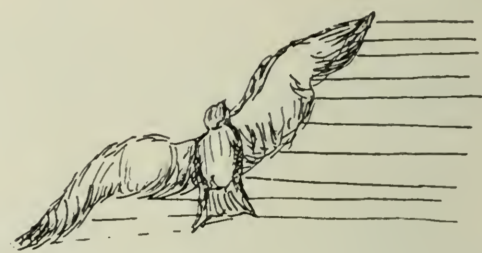


FIG. 11.

(Falling.) The bird being in the act of falling in the direction of its open wings [Fig. 11] with a force of 4 and the wind which strikes it underneath with a force of 2 makes its course straight : we will say then that the descent of such a bird will be by the mean line between the level of the course of the wind and the obliquity in which the bird was with the force 4. As : let the obliquity of such a bird be the line *a d c,* and let the wind be *b a*; I say, if the bird *a d c* had a force 4 and the wind *b a* had a force 2 that the bird would not go in the direction of the wind (either) in *f* nor by its obliquity in *g* but would fall by the mean line *a e* [Fig. 12] ; and one proves it thus.

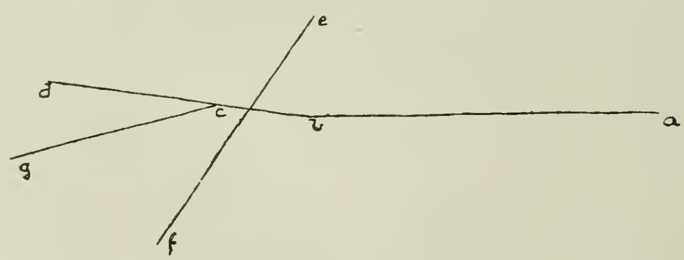


FIG. 13.

And if a bird make such an oblique descent [Fig. 13] with a force of 4 and if the wind that follows it have a force of 8. . . .

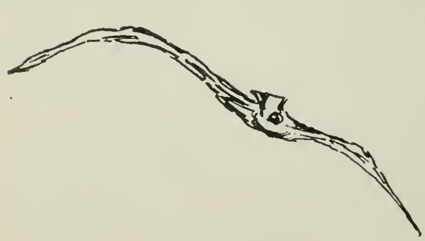


FIG 14.

When the bird wishes to turn to the right or the left by beating its wings then it will beat lower with the wing on the side to which it wishes to turn and thus *the bird will turn the movement behind the impetus* (*élan*) *of the wing which moves most.* [Fig. 14.]

[*Continued in Fol.* 6 [5] *recto.*]

[Fol. 6 [5] *v*.]

and makes the reflex movement under the wind on the opposite side.

When the bird by the beating of its wings wishes to rise, it raises the shoulders and it beats the points of the wings toward itself, and so condenses the air which lies between the points of the wings and the breast of the bird, the tension of which lifts up the bird.

The kite and other birds that move their wings very little seek the air currents, and when the wind is blowing high up then they will be seen at a great height and if the wind blows low down they remain low.

When there is little wind the kite beats several times with its wings during flight so that it may rise and obtain impetus, with which impetus inclining a little it goes a long distance without beating its wings; and when it has dropped [vol-planed] somewhat it repeats the movements and so successively: and this drop without beating the wings serves as a means of repose in the air after fatigue of the said beating of the wings. All birds that fly by spasms raise themselves by the beating of the wings and when they come down [vol-plane] they rest because they do not beat with their wings in descending.

[Fol. 7 [6] *r*.]

Concerning the 4 movements reflex and incident with varying aspects of the wind made by birds.

The oblique descent of birds, being made against the wind will be made under the wind and their reflex movement will be made on the wind.

But if such an incident [downward] movement is made to the east, the wind blowing from the north, then the north wing [upward] will remain under the wind, and in the reflex movement will do the same, so that at the end of this reflection the bird will find itself with its front to the north.

And if the wind descend to the south the north wind prevailing, it will make its descent on the wind, and its reflex motion will be under the wind: but this gives rise to a long discussion, which will be given in its due place because here it would appear that the bird would not be able to make a reflex movement.

When the bird makes its reflex movement against and on the wind then it will rise much more than it should from its natural impetus, seeing that it has the assistance of the wind which entering under it, serves as a wedge. But when it has completed its rise it will have used up its impetus and there will remain only the assistance of the wind which would turn it upside down because it strikes it on the breast and if it did not depress the right or left wing which will cause it to turn to the right or left, descending in a semi-circle.

[Fol. 7 [6] *v*.]

The movement of the bird ought always to be above the clouds lest the wing should be moistened to disclose a broader view and to avoid the peril of wind revolutions among mountain gorges, which are always full of whirlings and turnings of the winds. Beyond that, if the bird should turn somersault, you would have more time for righting it, with the directions already given before it reaches the ground.

If the point of the wing be struck by the wind and this wind enter under such a point, then the bird would be liable to be upset unless it use one of two remedies, that is to say: either that it force suddenly such a point under the wind or that it lower the opposite wing, from the middle on.

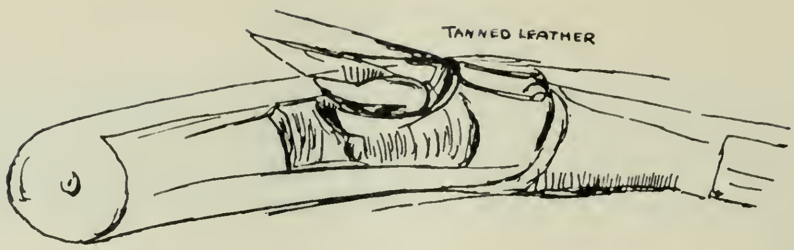


Fig. 15.

a b c d [Fig. 16] are 4 upper nerves to raise the wing (*b*) and act as strongly as the lower nerves *e f g h* on account of the overturning of the bird so that they might resist above as below although a single one of tanned leather thick and wide [Fig. 15] might perchance suffice; however, in the end, we must leave that to experience.

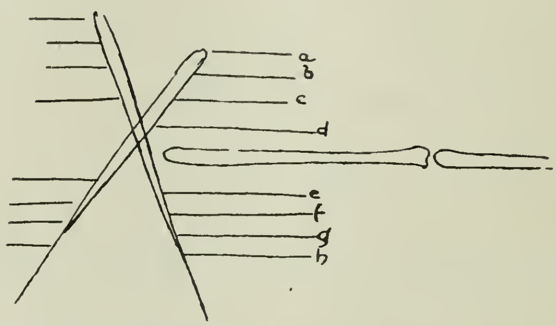


Fig. 16.

[Fol. 8 [7] *r*.]

The said bird ought, with the assistance of the wind to rise to a great height, and that will be its safety, even should it experience all the above mentioned revolutions, it has time enough to recover its balance provided that its limbs be very strong so that they may safely resist the fury and vigour of the descent with the above mentioned defences, its joints of strong tanned leather and its nerves [sinews] of raw silk cord of great strength; and let not anyone hamper himself with fittings of iron, because they burst easily in twisting, or waste away, for which reason they are not to be used.

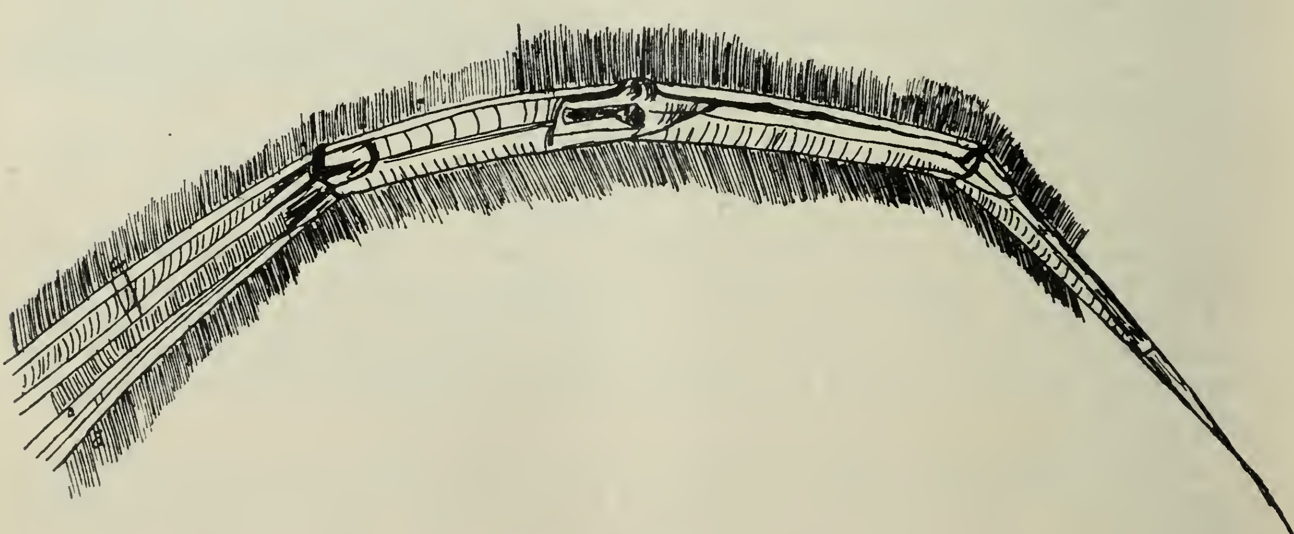


Fig. 17.

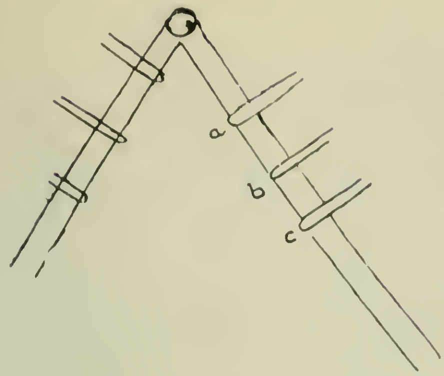


FIG. 18.

The nerve [sinew] *a* [Fig. 17] serving to stretch out the wing, should be of thick tanned leather, so that if the bird should be turned upside down, it would be able to overcome the fury of the wind which would strike in the wing and tend to close it, because that would cause the ruin of such a bird; but for greater security you will make precisely the same nerve outside as inside, and you will be beyond all suspicion and peril.

a b c [Fig. 18] are the attachments of the nerves of the 3 junctions of the fingers of the wings; *d* is the position of the motor of the lever *a d* which moves the wing.

[Fol. 8 [7] *v.*]

When the edge of the point of the wing is opposed to the edge of the wind for an instant, I put this wing under or on this edge of the wind and the same thing happens to the point or sides of the tail, and similarly to the shaft of the shoulders of the wing.

The descent of a bird will be always by that (part *w*) extremity which will be nearest to its centre of gravity.

The heaviest part of a bird descending will always be in front of its centre of resistance.

3rd. When without the assistance of the wind the bird remains in the air without beating its wings, in the position of equilibrium this shows that the centre of gravity is concentric with the centre of resistance.

4th. The heaviest part of the bird which descends with the head underneath, will never be above or level with the height of the lightest part.

If the bird fall tail first, by throwing the tail back it will restore itself to the place of equilibrium, and if it throws it forward it will overturn itself.

1st. When the bird which is in the position of equilibrium shall act so that the centre of resistance of the wings be behind the centre of gravity, then such a bird will descend head first.

2nd. And that bird which being in a position of equilibrium shall have the centre of resistance of the wings more forward than the centre of gravity of the bird then such a bird will fall with the tail turned towards the ground.

[Fol. 9 [8] *r.*]

If the wing and the tail are too much on the wind, lower half the opposite wing, and therewith receive the force of the wind and equilibrium will be restored.

And if the wing and the tail were under the wind raise the opposite wing and it will be corrected to your desire, provided that such a wing as is raised be less oblique than (the) that which is opposite to it.

And if the wing and the breast are on the wind, one must depress half the opposite wing, which will be struck by the wind and forced upwards which will right itself.

And if the bird is so that its hind quarters are on the wind, then the tail ought to be forced under the wind, and thus one will be able to equalise the powers.

But if the bird has its hind quarters under the wind (raising the tail) let it enter with the tail on the wind, and it will right itself.

[Fol. 9 [8] *v*.]

When the bird is on the wind turning the beak with the bust (*s*) to the wind then the bird would be turned over by the wind, if it had not depressed the tail, and received in the tail a large portion of the wind: thus doing it cannot be turned over. One proves it by the first of the mechanical elements which shows that things placed in balance which are struck beyond their centre of gravity, force down the opposite parts placed on this side of the centre before mentioned. For example, let the quantity [surface] of the bird be *d e f* [Fig. 19] and its centre of revolution be *e* and the wind which strikes it be *a b d e* and *b c e f*: I say that a greater volume of wind strikes in *e f* the tail of the bird beyond the centre of revolution, than in *d e* on this side of the said centre: and for this cause the said bird cannot be upset above all, while holding its wings to the wind by the edge.

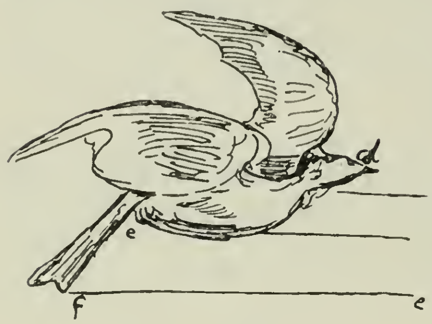


FIG. 19.

And if the bird is so that its length is under the wind it is liable to be turned upside down by the wind if it does not immediately raise its tail up. One proves it thus:—Let the length of the bird be *d n f* [Fig. 20], *n* is the centre of its revolution: I say that *d n* is struck by a greater amount of wind than *n f*; and for this reason *d n* will respond to the course of the wind, giving place to it, and moving downwards, thus raising the bird to the position of equality.

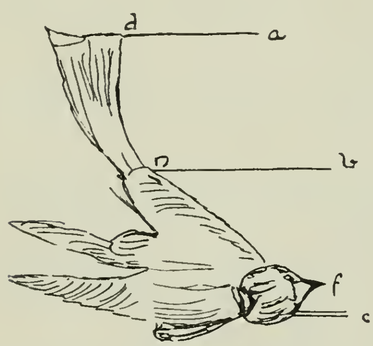


FIG. 20.

That the wing does not utilise all the pressure of the air and that this is true, note that the interstices of the primary feathers are spaces much wider than the width of the feathers themselves: therefore you who study flying should not calculate on the entire surface of the wing, and note the different varieties of wings for all flying creatures.

[Fol. 10 [9] *r*.]

When the wind strikes the bird under its course from its centre of gravity towards this wind, then such a bird will turn itself with its spine to the wind, and if the wind were more powerful below than above then the bird would turn upside down, *if it did not immediately take care to draw in the under wing and stretch out the over wing; in this manner it rights itself and returns to the position of equilibrium.*

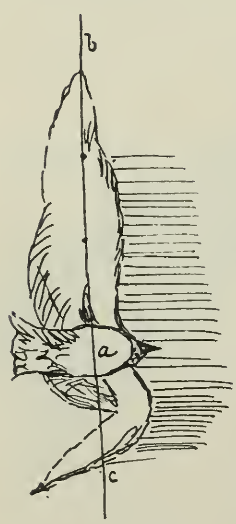


FIG. 21.

One proves it thus:—Let *a c* [Fig. 21] be the wing withdrawn under the bird, and *a b* be the extended wing. I say that the forces of the wind that strike the 2 wings will have the same proportion (ratio) as that of their extensions, that is to say *a b* to *a c*. It is true that *c* is wider than *b*; but it is so near the centre of gravity of the bird that it offers little resistance in comparison with *b*.

FIG. 22.

But when the bird is struck under the wind beneath one of its wings, then it would be possible for the wind to upset it, unless, immediately after it had turned with its breast to the wind, it stretched the opposite wing towards the earth, and shortened the wing which had been first struck by the wind, which wing remains superior and thus it will restore itself to equilibrium. One proves it by the 4th of the 3rd that is to say, *that object is the more affected which is attacked by the greater force;* again by the 5th of the 3rd that is to say *this support resists less and is situated farther from its fixed point;* again by the 4th of the 3rd *between winds of equal force* [*velocity*] *that will have the greater force* [*power*] *which has the greater volume* and that one will strike with a greater volume which finds a greater object; so that *m f* being longer than *m n* *m f* will obey the wind. [Fig. 22.]

[Fol. 10 [9] *v.*]

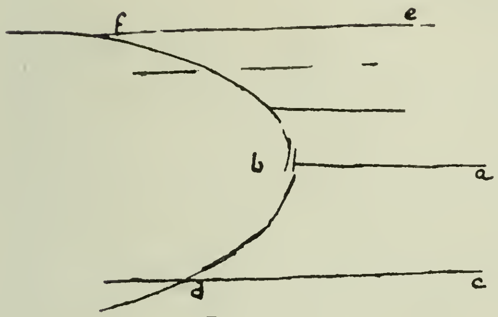


FIG. 23.

Struck from above, (up) the force of the wind which strikes it from above, has not full play, seeing that the wedge of wind, which is separated from the middle of the shoulder downwards [beneath], lifts the wing up with almost as much force as that exerted by the upper wind to depress the wing. One proves it: let *f b d* (and *b a b e*) [Fig. 23] be the shoulder of the wing: and *e f c d* (and) the sum total of the wind which strikes this shoulder of the wing, of which wind half is *a b c d* which strikes from the summit of the shoulder *b* down to *d*: and because the line *b d* of this shoulder is oblique, the wind *a b c d* forms a wedge at the contact *b d* and raises the shoulder: and the upper wind *a b e f* which strikes the obliquity *b f.* makes a wedge there and pushes the wing down, with the result that these 2 said contraries prevent the shoulder from immediately entering beneath or above the arrival [course] of the bird, according to whichever its necessity requires: whence this necessity is met by putting a shaft on the round shoulder which acts as a buckler and immediately cuts the wind in such manner as the necessity of the bird demands as one shows in *m n* [Fig. 24].

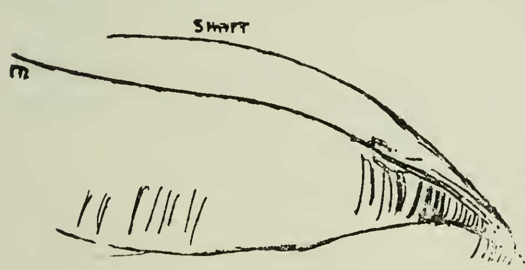


FIG. 24.

But if the wind strike the bird on the right or left wing, then it must enter below or above such a wind with the point of the wing that is struck by the wind [Fig. 25], which change occupies as much space as is taken up by the points of the wings: which change being under the wind, the bird turns with its beak to the wind and if it is on the wind, the bird

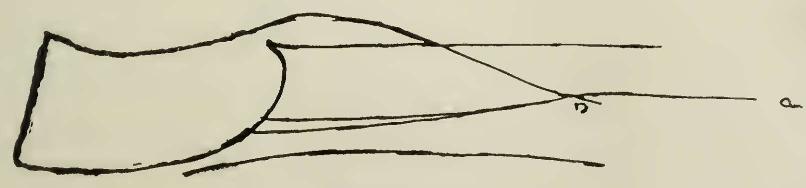


FIG. 25.

will turn with the tail at will: and here arises the peril that the bird will turn upside down, if nature had not provided for it by placing the weight of the body of the bird lower than the place of extension of the wings as one will show here.

[Fol. 12 [11] *r.*]

And lying is in such ill repute, that if it is said very great things concerning God it would (detract from the grace of the) Deity; and truth is of such excellence that if it praised trifles it would make them noble.

Without doubt, there is a similar proportion of truth to untruth as there is of light to darkness and this truth is in itself of such excellence that even if it be spread over humble and base matters, it exceeds beyond comparison the incertitudes and lies spread over great and very high discourses; because that still though our spirit may have lying for its fifth element, nevertheless the truth of things remains as the supreme nutriment for fine intelligences; but not for wandering humours.

But you who live by visions the sophistries and rogueries of boasters (please you more) in great and uncertain things please you more than those certain, natural and not of such great height.

The incident movements with their reflex movements are of 4 kinds, of which one, incident and reflex, is rectilinear, having lines of equal obliquity; the other is again rectilinear but the obliquities are different; the 3rd has the incident movement rectilinear and the reflex curvilinear; the 4th has the incident movement curvilinear and the reflex a straight line. Of these straights and curvilinears each of them is divided into 2 parts, because the first may have its incident movement rectilinear directly opposite to the chord of the arc made by the curvilinear reflex movement, and again this reflex may bend to the right or to the left of the incident rectilinear movement.

When the bird flies by beating its wings it does not fully extend its wings, because the points of the wings would be too distant from the lever and the nerves [sinews] which move it.

If in (lowering) the descent of the bird it rows backward with its wings, the bird will move rapidly; and this happens because the wings strike in the air which successively flows behind the bird, to fill the void that it has left.

[Fol. 12 [11] *v.*]

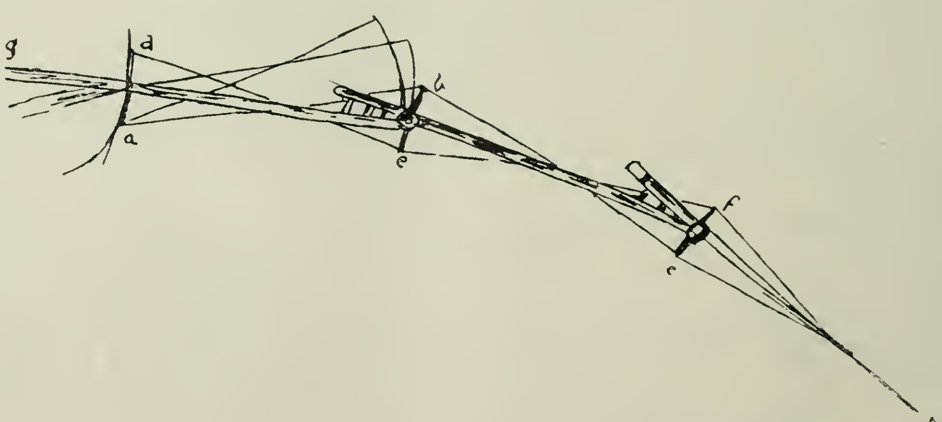


Fig. 26.

When *g* (*i*) descends *p* rises. [Fig. 26.]

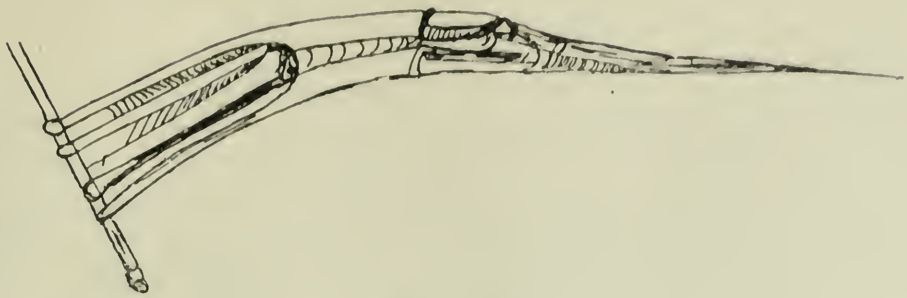


Fig. 27.

[Outline of a leaf here.]

[Fol. 13 [12] r.]

[Continuation of Fol. 9 *v.*] such a movement will bend and make it a semi-circle; then such a bird will be at the end of this movement with its beak turned towards whence such a reflexion began: which, if it be made against the arrival of the wind the end of the reflex movement would be made much higher than was the commencement of the incident movement, and this is the manner in which the bird rises, without beating its wings and in circulating; and the remainder of the said circumference would finish (with) by the sense of the wind, by incident movement always with one of the wings low, and similarly one side of the tail; and it then makes a reflex movement towards the flight of the wind and remains at the end with the beak turned to the flight of this wind and then makes incident and reflex over again, against the wind always circulating.

When the bird wishes to turn itself suddenly on one of its sides, then it quickly pushes the point of the wing on this side towards its tail, and because *all movement tends to its maintenance* or rather, *all moved bodies continue to move as long as the impression of the force of their motors remains in them,* the movement then of such a wing thrust with violence towards the tail reserving still at the end a part of the said impression, not being able by itself to follow the movement commenced at first, will move all the bird with itself, until the impetus of the moved air may be consumed.

The tail pushes with its face, and the wind struck by it, makes the bird move suddenly in the contrary direction.

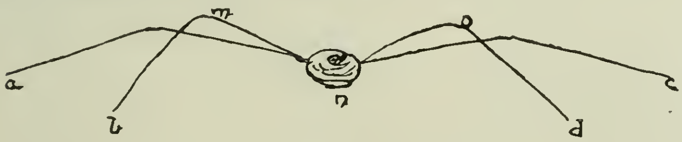


Fig 28.

When the bird shall be in the position *a n c* [Fig. 28] and shall wish to rise, it will elevate the shoulders *m o* and will thus be in the position *b m n o d* and the air will be pressed between the sides and the point of the wings, so that it will be condensed and cause an upward movement, and give rise to an impetus in the air, which impetus of the air will push the bird upwards by its condensation.

[Fol. 13 [12] *v.*]

To escape the peril of ruin.

The ruin of such instruments [machines] may happen in two ways, of which the first is that the instrument break; the second is that the instrument turn on its edge, or near its edge because it ought always to descend by a great obliquity or almost by the line of equality. . . . As to the protection of the instrument from rupture, one avoids it by making it of the greatest strength by no matter what line it may be able to turn itself, that is to say: either by the edge (or sense about the) falling either with the head or the tail first, or even with the point

of the right or left wing, or by the halves or quarters of the said lines as the design shows. As to turning by any side whatever of the edge, one ought to prevent it even in the beginning by making the instrument in such a fashion that in descending, under whatever aspect that may come about the prevention should be ready; and this will be done in giving its centre of gravity on the centre of the weight carried by it, always in a straight line, and one of the centres very distant from the other; that is to say, that in an instrument 30 fathoms wide these centres may be 4 fathoms apart, and that one, as mentioned, be situated under the other and the heavier underneath, so that in the descent, the heavier portion might always be the guiding portion of the movement. Further than that, if the bird should fall head first (1) with such obliquity as would turn it upside down, the latter would not be able to happen, because the lighter portion would be under the heavier, a thing which is impossible in a long descent as one proved in the 4th of the mechanical elements.

And if the bird fall with the head underneath (with the body) with part obliquity of the body turned to the earth, then the sides of the wings underneath ought to turn flat against the earth and the tail be raised towards the loins (back), and that the head and the underneath of the jaws be turned also towards the earth, whence its reflex movement will commence immediately in such a bird, which will cast it back towards the sky; on this account such a bird will, at the end of this reflexion begin to fall backwards, unless in its rise, it lowers one of its wings a little, which

[Continued on Fol. 12 recto.]

[Fol. 14 [13] *r.*]

Here, the big fingers of the wings are those which hold the bird still in the air against the movement of the wind: that is to say, the wind moves on which it supports itself without beating its wings and the bird does not change its place. The reason of this is that the bird arranges its wings on such an obliquity that the wind, which strikes it underneath does not cause a wedge of such a nature as would lift it up, but lifts it up however just as much as its weight would press it down, that is to say: if the bird fall with a force of 2 the wind would lift it up with another force of 2 and because equal things do not surpass one another, this bird remains in its place without rising or falling. It remains for us to speak of movement which drives it neither forward nor backward: and this happens if the wind accompanies it or rather pushes it out of its place with a force of 4, and the bird with the same force inclining with the said obliquity against the wind: here also the powers being equal such a bird will not move forward nor be driven back, the wind being equal. But because the movement and powers of the winds are variable, and the obliquities of the wings ought not to be changed, for if the wind should increase, and it overcame the obliquity, in order not to be pushed upwards by this wind. . . .

[In Margin.]

One will carry snow in summer, taken from the high crests of the mountains and one will let it fall in places of festival in the summer time.

The wind does not enter, in the above mentioned cases, as a wedge under the obliques wings, but reaches the wing only at the edge which tries to descend against the wind, whence it strikes the edge of the shoulder, which shoulder acts as a buckler for the rest of the wing: and here the descent of the wings would not have any defence if the great finger *a* [Fig. 29] were not there which then faces front, and itself receives all the force of the wind directly on it or less than directly, according to the more or less great force of the wind.

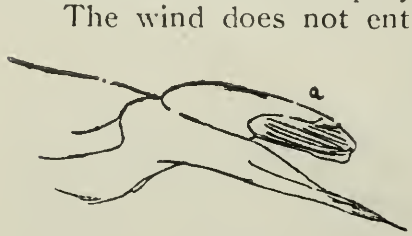


FIG. 29.

[Fol. 14 [13] *v.*]

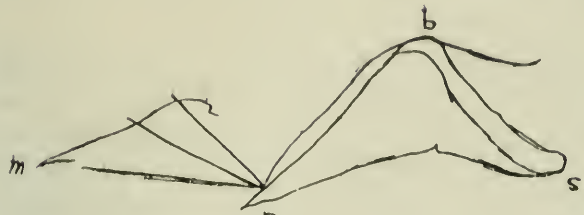


FIG. 30.

The big (thumb) finger *n* [Fig. 30] of the hand *m n* is that which, when the hand is lowered, is lowered more than the hand, so that it closes and prevents the escape of the air pressed by the descent of the hand, so that in this place the air is condensed and resists the oars of the wings and therefore nature has provided such a finger with a bone of great strength, to which strong nerves [sinews] and short feathers are joined. The short feathers have greater strength than the other feathers of the wings of birds will have, because that by them the bird supports itself on the air already condensed with all the force of the wing and of its strength, because it is that by which the bird is moved forward: and this finger does the same work for the wings that the claws do for the cat when it climbs on the trees.

But when the wing recovers a new force with its return upwards and forwards, then the big finger of the wing puts itself in a straight line with the other fingers, and thus with its cutting extremity divides the air and does the work of a shaft which always cuts the air by some movement, high or low, that the bird would rise.

The second shaft in the opposite portion, beyond the centre of gravity of the bird, and that is the tail which, if it is struck by the wind underneath, since it is beyond the said centre, it will cause the forward portion of the bird to lower. And if this tail be struck on top the forward portion of the bird will rise. And if this tail twist a little, and turn its under surface obliquely to the right wing, the anterior portion of the bird is turned to the right side. And if it turn this side obliquity of the under surface of the tail to the left wing, it (the bird) will be turned with its forward portion toward the left side: and in each of the two manners the bird will sink. . . . 4

[In Margin.]

4. But if the tail, placed obliquely, be struck by the wind on its upper surface, the bird (it) will be turned, in turning the tail slowly from this side where the superior surface of the tail shows its obliquity.

-vity

[Fol. 15 [14] *r.*]

The pivot of the shoulders of birds is that which is turned by the muscles of the chest and of the backbone; and it is there that the discretion arises of lowering or raising the elbow according to the will or necessity of the animal which is moving.

I conclude that the mounting of the bird without the beating of wings, is caused by nothing other than its circular movement which, when it starts from the arrival of the wind sinks until it reaches the place where the reflex movement begins after which and so circulating, it has described a semi-circle and its face turned to the wind, and follows the reflex movement, on the wind still circulating until, with the help of the wind it makes its greatest height between its lowest and the arrival of the wind and is left with the left wing to the wind; and from this greatest height again circulating, it descends to the last incident movement being left with the right wing to the wind. As if to say:—The wind goes from *a* to *c* [Fig. 31] and the bird moves from *a* and sinks from *a b c* and in *c* it makes the reflex movement

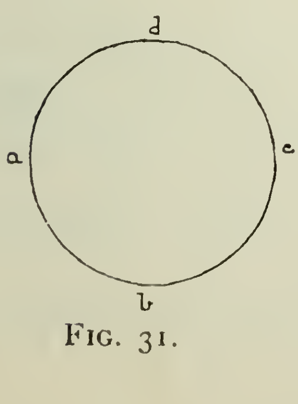


FIG. 31.

as in *c d a*, and by the favour [help] of the wind it is much higher at the end of the reflex movement, which end of the reflex movement is started perpendicularly over the said commencement of the incident movement.

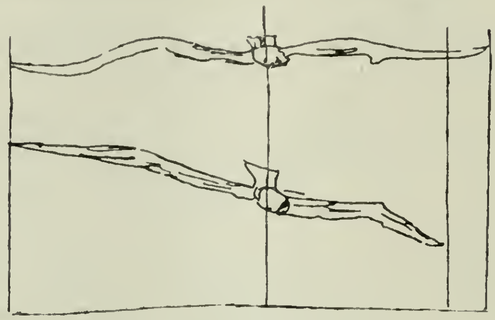


FIG. 32.

The equal resistance of the bird's wings always springs from the fact that they [the wings] are equally distant, at their extremities, from the centre of gravity of any bird. [Fig. 32.]

But when one of the extremities of the wings is nearer the centre of gravity than the other extremity, then the bird will descend from that side where the extremity of the wing is nearer the centre of gravity.

[The last syllable —*vity* is found on Fol. 14 [13] *v.*]

[Fol. 15 [14] *v.*]

The hand of the wing is that which gives the impetus; and then the elbow is turned with the edge forward so as not to hinder the movement which created the impetus; and when this impetus is afterwards created, the elbow is lowered and made oblique and being oblique it makes the air on which it lies as in the form of a wedge on which the wing raises itself, and if the movement of the bird were not so performed, during the time that the wing moves forward the bird would sink towards the end of the impetus; but it is not able to sink because as much as the impetus lacks so much the percussion of the elbow resists this descent and lifts up the bird.

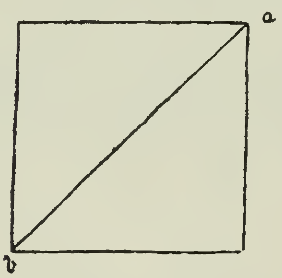


FIG. 33.

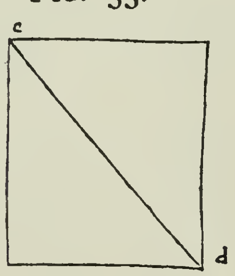


FIG. 34.

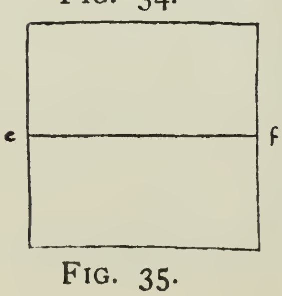


FIG. 35.

Let us say, that the impetus be as 6 and that the bird weigh 6 and that in the middle of the movement the impetus become 3 and that the weight still remain 6; here the bird would sink by half-movement, that is to say, by the diameter of the square, and the wing oblique with the contrary aspect, also by the diameter of this square does not allow such a weight to sink, neither does the weight permit the bird to rise; consequently it moves in a straight line. That is to say: the descent of the bird during the said half-movement would be by the line *a b* [Fig. 33] and, because of the obliquity of the wings with contrary aspect would have to rise by the line *d c* [Fig. 34]; whence, from the above-mentioned causes, it moves on the place of equality *e f*. [Fig. 35.]

The elbows of the animal are not lowered just at the commencement because in the principal flight of the impetus the bird would jump upwards, but they are lowered just so much as is necessary to prevent the descent according to the will and discretion of the bird.

When the bird wishes to glide suddenly upwards it lowers the elbows immediately after it has created the impetus.

But if it wishes to descend, it holds the elbows up firmly after the creation of the impetus.

[Fol. 16 [15] r.]

You are to remember that your bird ought not to imitate anything but the bat, because the membranes [web] form an armour or liaison to the armour, that is to say, mistress of the wings.

And if you imitate the wings of the feathered birds, the wings are more powerful in bone and nerve, through being pervious: that is to say, the feathers are disunited permeable to the air. But the bat is aided by the membrane which binds the whole and is not pervious.

Of the method of balancing oneself.

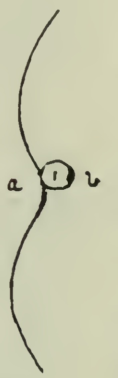


FIG 36.

It is always the heaviest portion of the body which guides its movement.

Then the bird finding itself in the position *a b* [Fig. 36], *a* being lighter than *b*, where the motive power is disposed it will always keep above *b*, so that it will never happen that *a* will precede *b* except by accident, which will not last long.

The bird which has to rise without beating its wings, places itself obliquely against the wind, presenting the wings to the latter with its elbows in front, with the centre of its gravity more towards the wind than the centre of the wings, whence it happens that if the obliquity of the bird would sink with a force of 2 and that the wind strikes it with a force of 3, this movement obeys the 3 and not the 2.

[Fol. 16 [15] *v*.]

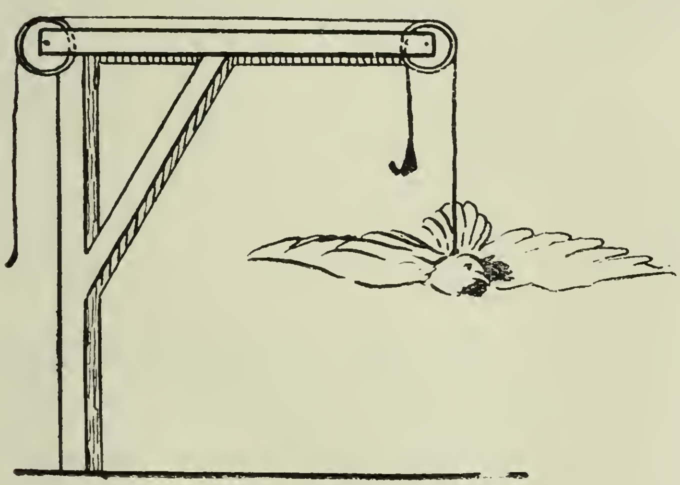


FIG. 37.

This [Fig. 37] is done to find the centre of gravity of the bird, without which instrument, this instrument would have little value.

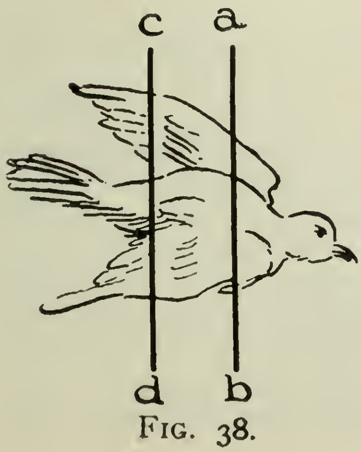


FIG. 38.

When the bird sinks, then the centre of gravity of the bird is outside the centre of its resistance; as if the centre of gravity were on the line *a b* and the centre of resistance on the line *c d* [Fig 38].

And if the bird wishes to rise, then the centre of its gravity remains behind the centre of its resistance.

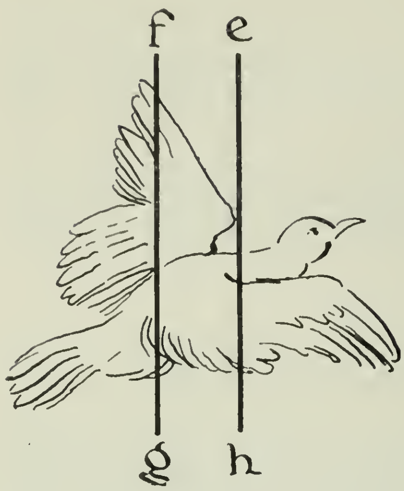


FIG. 39.

As if the centre of gravity mentioned might be in *f g*, the centre of resistance would be in *e h*. [Fig. 39.]

The bird is able to rest in the air without holding its wings in the place of equality because not having its centre of gravity at the middle of the pivot, as balances have, it is not constrained by necessity to hold its wings at an equal height, as in the said balances. But if the wings are outside this place of equality then the bird will descend by the line of obliquity of these wings; and if the obliquity is compound, that is to say, double, as for example, that the wings decline to the south, and that the obliquity of the head and tail declines to the east, then the bird will descend with the obliquity to the south-east. And if the obliquity of the bird be double that of the wings then the bird will descend in the middle between the south-east and the east, and the obliquity of its movement will be between the two said obliquities.

[Fol. 17 [16] *r*.]

Persuasion in haste (or persuasion of the enterprise) *which removes the objections*.

If you say that the nerves and muscles of the bird are, beyond comparison, of greater power than those of man seeing that all the flesh of so many muscles and pulps of the breast is made for the benefit and augmentation of the movement of the wings, with this bone of the breast all in one piece which provides the bird with great power, with the wings a tissue of great nerves and other very strong ligaments of cartilage and a skin made very strong with diverse muscles: here one replies that so great a force is provided for power because that beyond just sustaining itself by its wings it must at will double and triple the movement in order to escape pursuit or to follow its prey, whence, in such an effect it is necessary for it to double or triple its force, and

Wine-skins, where a man, falling from a height of 6 fathoms, would not injure himself, falling either in water or on land: and that these wine-skins, fastened together in the fashion of beads, are surrounded by others.

Man, also, has a greater amount of strength in his legs, than is necessary for his weight: and that this is true, place a man upright on some mud and note how deeply his foot sinks. Then place another man on his back and you will see how much deeper he sinks. Then, having removed the man from his back, make

beyond that, carry as much weight in its claws, in the air, as equals its own weight: as one sees the falcon carry the duck, and the eagle the hare, by which thing it is very well demonstrated where such super-abundant force is distributed: but it needs little force to sustain itself and to balance them on the currents of wind and to direct the shaft in its paths: a little movement of the wings is sufficient and as much more slow movement as the bird is larger.

him leap straight up in the air, as far as he can, and you will find the imprint of the foot deeper from the leap, than with the man on his back: therefore it is here proved in two manners, that a man has more than double the force that is requisite to sustain himself.

[Fol. 17 [16] *v.*]

If you fall, see that you strike the ground with the double wine-skin that you hold under you.

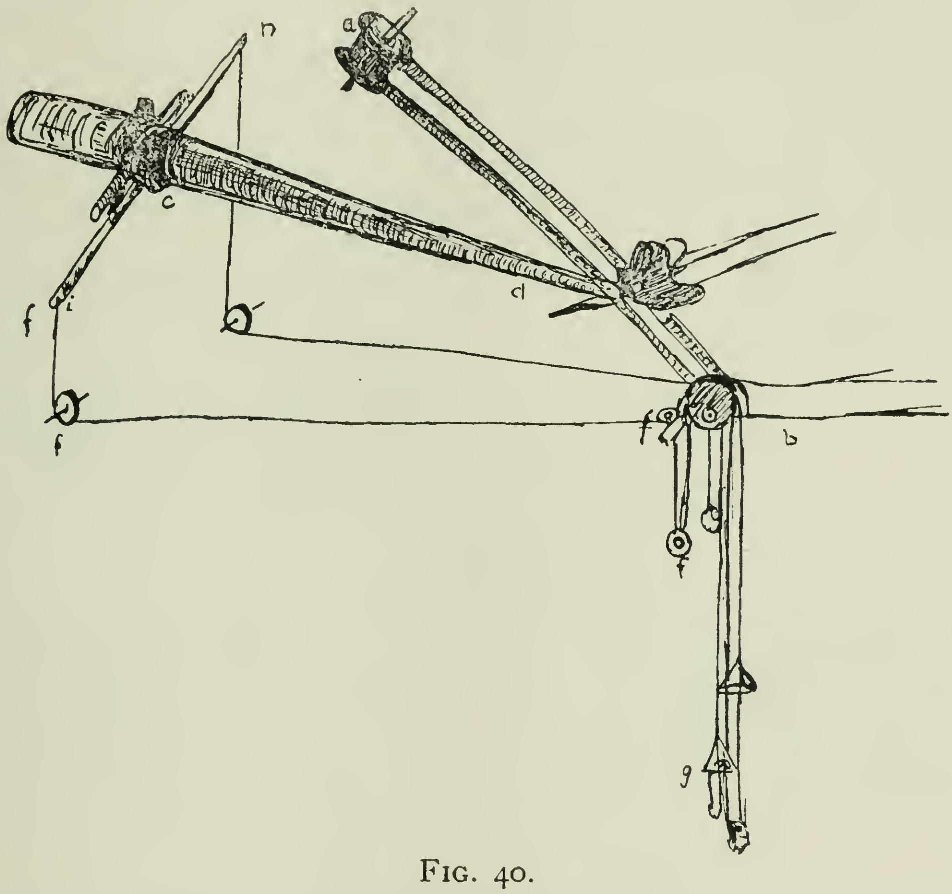


Fig. 40.

Because the wings have to row below and behind in order to keep the instrument up, and that it may move forward, the movement of the lever *c d* [Fig. 40] is made by an oblique way, guided by the strap *a b*.

I could make it so that the foot which presses the stirrup *g* might be that which beyond its ordinary function should draw down the lever *p*. But this would not be according to our design because we require that the lever *f* may rise or sink before the stirrup *g* move from its place, in order that the wing in throwing itself forward, or lifting itself up (during the time in which the impetus, already acquired is moving the bird forward, without the beating of the wings) may be able to put the wings in the air by the edge, because if that were not done, the face of the wings would strike the air, hinder the movement, and prevent the impetus from carrying the bird forward.

[Fol. 18 (recto).]

Always in the elevation of the hand, the elbow is lowered and presses the air, and in the lowering of this hand the elbow is raised and remains by the edge lest it hinder the movement by means of the air that strikes therein.

The lowering of the elbow at the time when the bird moves the wings forward by the edge a little on the wind guided by the impetus already acquired causes the wind to strike under this elbow and form a wedge on which the bird with the said impetus, without beating its wings, mounts up and if the bird is 3lbs. and the breast 1/3 the width of the wings, the wing will feel but 2/3 of the weight of such a bird.

The hand feels a great fatigue towards the big finger, or rather shaft of the wing, because it is this part which strikes the air.

The palm of the hand goes from *a* to *b* always between angles almost equal declining and pressing the air, and at *b* turns immediately by the edge and goes behind rising by the line *c d*, arrived at *d* it is turned suddenly in front and goes sinking by the line *a b* and in turning does so always around the centre of its width. [Fig. 41.]

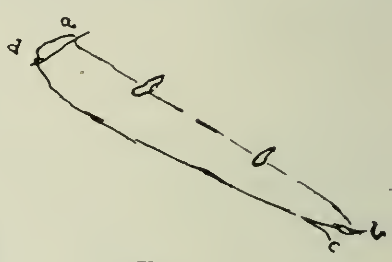


FIG. 41.

The recoil of the hand, by the edge will be made with great speed and the pressure behind, in front, will be made with that speed which the last power of the motor requires.

The course of the point of the fingers is not the same going as returning but the return is through a higher line: and under this is the figure made by the superior and inferior line, and oval with a long and narrow curve.

[Fol. 18 (verso).]

1505 Tuesday evening on the 14th of April Laurent came to live with me, he says he is 17 years old. And on the 15th of the said April I had 15 florins of gold from the chamberlain of San Maria Nova.

From the mountain, which bears the name of the great bird, the famous bird will take its flight, and will fill the world with its great fame.

To raise a tree by *p* and *r s* sustain [Fig. 42].

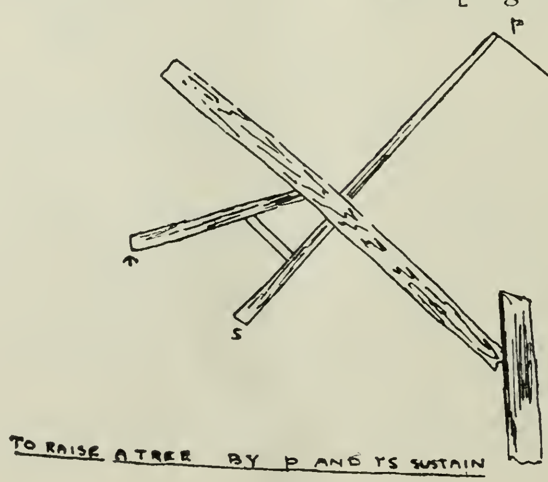


FIG. 42.

[Back Cover—Inside.]

lad,	48	
husks	44	
in straw	23	
key	.6	148
myself	28	111
chicken	.2	——
	——	037
	111 (8)	
	(11)	
	.28	
	——	
	83	

The great bird will take its first flight, on the back of its great swan, and filling the universe with stupor, filling all writings with its renown, and glory eternal to the nest where it was born.

REVIEW OF AIRSCREW THEORIES

BY A. R. LOW.

The following letter from Dr. Enrico Pistolesi, the well-known Italian aeronautical engineer, continues the discussion :—

Dear Sir,

I have read in No. 196, February, 1923, of the Journal of the Royal Aeronautical Society, the interesting communication read by you and the important discussion following.

While expressing the pleasure of a modest worker like myself in the captivating argument, I permit myself to send some of my publications, from which you will see that the ideas so lucidly expressed by yourself were expounded by me in a paper in January, 1921, to the Ass. Ital. di Aerotechnica, and since developed at length in succeeding papers.

The latest is actually in press, and it will be my care to send it to you as soon as published.

These reproduce substantially my "Course for Aeronautical Constructors" at the Royal Polytechnic of Turin. They have some analogy with Mr. Glauert's work, but were (in their formulation, if not always in date of publication) prior to it and entirely independent of it. Further, they show a much more advanced stage of working out, and are capable of immediate application in practice.

Believe me, etc.,

ING.-DOTT. E. PISTOLESI.

Enclosed :—

(1) La Teoria dei Vortici in Aerodinamica, 25th January, 1921.

(2) Nuovi Indirizzi e Sviluppi della Teoria delle Eliche, January, 1922.

(3) I Propulsori Elicoidali e i Recenti Progressi dell'Aerodinamica, 15th October, 1922.

Commenting on the above, in (1) Dr. Pistolesi gives a brief statement of the Kutta-Joukowski transformation, and of the Lanchester-Prandtl trailing vortex system, and of the Karman stable system of transverse vortices. He then considers Prandtl's application of the theory to the limiting case of an airscrew with a very large number of small blades, and obtains the inflow correction of $v/2$ and $w/2$, applied to the coefficients for infinite aspect ratio. He remarks that although the rotary inflow effect may be physically eliminated by the interference of the equal and opposite circulation round the blade, yet it must be taken into account in correcting the results obtained on the simple Drzewiecki theory. It will be seen that his position is so far identical with Mr. Glauert's, and, of course, in agreement with Prandtl's original result.

In (2) and (3) Dr. Pistolesi develops a very complete discussion of airscrew theories covering substantially the same ground as the writer's "Review." The treatment is strictly mathematical, and the learned author shows a satisfying mastery of the technique of the various methods.

Reference is made to the work of Froude, Rankine, Drzewiecki, Soreau, to the development of the "inflow" corrections by Fage, Riach, de Bothezat, to the work of Durand and Warner, and to the application of the cascade method by McKinnon Wood. Finally, the author examines these methods in the light of

the Lanchester-Prandtl vortex theory, particularly with regard to Betz's paper "Screw Propellers with Minimum Energy Losses" (Göttingen, 1919).

One point is left outstanding—the validity of applying the results of the limiting case with a very large number of blades to a screw with two, three or four blades. On page 217 of (3) he quotes Betz's integrals for the velocity components due to a spiral vortex trailing from the blade tip, found in a slightly different form by the present writer.

The evaluation of these integrals will finally decide the true inflow correction for a screw with a small number of blades.

A. R. Low.

CORRESPONDENCE

To the Editor of the Journal of the Royal Aëronautical Society.

Paris, le 28 février, 1923.

Messieurs,—Nous avons noté, dans votre numéro de février, 1923, que M. le Major A. R. Low, membre de votre Société, se réfère, dans son étude générale sur la théorie de l'hélice, à une étude de M. Margoulis qui aurait été publiée en supplément par "L'Aerophile." Vous voudrez bien noter que c'est "L'Aeronautique" qui a publié ce travail en supplément à son numéro de mars. Nous avons d'ailleurs eu le plaisir d'en adresser un exemplaire à Mr. A. R. Low.

Veuillez agréer, Messieurs, l'assurance de nos sentiments distingués.

H. BOUCHÉ.

REVIEWS

Mechanical Testing (Vol. II.)

Batson and Hyde. (Chapman & Hall.)

In this book the authors have collected a large amount of information which is not to be found in any other single volume. As a treatise on the methods of testing prime movers, machines, structures, etc., it is rendered more complete by the inclusion of descriptions of many commercial forms of apparatus, and the unique opportunities enjoyed by the authors for examining and using such apparatus in the course of their work in the Engineering Department of the National Physical Laboratory has enabled them to give practical working hints and advice of a type which is frequently missing from the instruction sheets of the manufacturers.

Although the volume deals with mechanical tests in general (or perhaps more correctly with those mechanical tests which have been referred to the N.P.L.), there are few sections which do not affect some aspect of aeronautical work. The first nine chapters, dealing with various forms of dynamometer and apparatus for the measurement of power, torque, thrust, gear efficiency, etc., are of direct interest to engine users and testers. So also are the chapters on the testing of balance (static and dynamic), lubrication and bearings.

The effect of vibration on aeroplane structures is attracting much attention at present, and the chapter on vibration tests contains a considerable amount of pertinent matter. The vibration tests of streamline wires (described on page 187) do not produce the type of failure which is most commonly found in service. As has recently been demonstrated, the combined effect of torsional and bending movements, generated and sustained under certain conditions of air flow, probably cause the typical diagonal fractures which occur near the end of the flattened portion of the wire.

Extended experience in the devising of suitable forms of test for lock nuts indicates that simple methods of the type suggested on page 189 give misleading and anomalous results. It is no uncommon experience in such tests to find that two ordinary nuts locked together give apparently better results than can be obtained with certain well-known forms of lock nut which have been used in thousands on railways, etc., with satisfactory results.

The chapter dealing with columns and struts, although including experimental work on spruce struts and on steel tubular struts (closed and split), contains no reference to the important work of Southwell, Robertson or Wylie, and is therefore incomplete from the aeronautical standpoint.

Two chapters are devoted to descriptions of wind channel and hydraulic apparatus.

The use of springs for instrument and test apparatus generally is discussed in Chapter X., and many useful tips are given. Table III., which gives the relative energy-storing capacity of various types of spring, might have included helical springs of hollow material, which are more efficient on a weight basis than those made from solid material.

Reference is made to tension meters, and two types are briefly described.

No form of accelerometer is included though the measurement of acceleration forces is within the scope of the book.

The remaining portions of the book, although apparently extraneous to aeronautical practice, will repay examination, as the design principles of much of the apparatus and the methods of many of the tests are capable of wider application.

WM. D. D.

THE JOURNAL

OF THE

ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

No. 151 JULY 1923 VOL. XXVII

NOTICES

Chairman Elect

At a Council Meeting held on June 5th, Mr. A. Ogilvie, C.B.E., Fellow, was elected Chairman for the year 1923-1924. Mr. Ogilvie will assume office on October 1st.

Donations

The following additions to the Library have recently been acquired:—

A collection of negatives and lantern slides illustrating the early experiments of the late Sir Hiram Maxim—presented by Lady Maxim.

Three books of cuttings containing a large number of letters from, and other information relating to the work of, the late Laurence Hargrave, collected by the late Mr. H. Crosland Taylor—presented by Mrs. Crosland Taylor.

Associate Fellowship Examination

The second examination for Associate Fellowship will take place in the Library, on Monday, September 24th (Part I.), and Tuesday, September 25th (Part II.). Intending candidates should forward entry forms (accompanied by the prescribed fee) on or before Monday, August 27th, stating the subjects in which they desire to be examined.

Journal

It is regretted that owing to the large amount of extra work thrown upon the staff by the International Air Congress, which was held from June 25th-30th, the appearance of this number of the Journal has been delayed.

W. LOCKWOOD MARSH, *Secretary.*

PROCEEDINGS

Tenth Meeting, 58th Session

A meeting of the Society was held at the Royal Society of Arts on Thursday, March 1st, 1923, Professor L. Bairstow, C.B.E., F.R.S., in the chair.

The Chairman, in calling upon Major F. M. Green to read his paper on "Air Travel, with Special Reference to the Helicopter," said that many of those present would be able to agree with the very critical review of the problem given by the lecturer, and it would be an advantage perhaps to the progress of aviation if the Society gave a technical opinion as to the value of the helicopter as a means of air travel.

Major Green, before reading his paper, apologised for its being short, but pointed out that it was meant to be a basis for discussion. If there were points which he had misrepresented he would be very glad to receive advice, and he was certain that a full discussion would benefit the progress of aeronautics.

AIR TRAVEL WITH SPECIAL REFERENCE TO THE HELICOPTER.

BY MAJOR F. M. GREEN, FELLOW.

A striking development of the present century has been the use of air travel. A new method of going from place to place has come into use which is quicker than any former means. It is only since the war that definite air services have been available to the general public, and in consequence it is only right to believe that its development is in an early and crude state. At the present moment, although there are a number of types of aircraft engaged in this work, they are all similar in principle. It is suggested from time to time that we are possibly working on wrong lines and that air travel can be carried out more effectively using machines of quite other types. The suggested alternatives to the present aeroplane are airships and another form of heavier-than-air craft generally called the "helicopter" or direct lift machine. It is not proposed in this paper to discuss the relative merits of the aeroplane and the airship, but to state some of the outstanding disadvantages of the types of aeroplane now in use and to consider whether the "helicopter" offers a hopeful solution.

Objects of Air Travel

The usual object of air travel is to go from place to place quicker than is possible by any other means. Apart altogether from its greater speed, the aeroplane saves time by travelling more directly from point to point, and by avoiding changing for sea journeys. Nevertheless, the aeroplane must be able to maintain a high speed through the air in order to make effective progress against adverse winds. Experience has shown that the slowest cruising speed that is practicable for most routes exceeds 80 miles an hour. If the route chosen is likely to be fairly free from high winds and if the existing methods of travel are very slow, a slower speed might be useful. In a general way, however, the speed must be high to make it worth while using a method of travel which for some time to come must remain expensive.

Safety

Let us agree that it is useless to run an air service with machines with a cruising speed below 80 miles an hour; we may next consider what are the desirable attributes of a machine suitable for air travel. I think that safety must be the most important attribute of any aircraft. Later on we shall consider the question of cost of running, but it cannot be doubted, even if we neglect other considerations, that an aeroplane which is not reasonably free from chances of accident is not likely to be economical in service.

Reliability

Assuming that we have an aircraft of sufficient speed and safety, the next requirement is that, when a start is made, the passenger shall be reasonably certain of reaching his destination. The value of speed disappears very rapidly if, in more than a very small percentage of cases, the journey has to be finished by other means, or if a long stoppage has to be made en route. It has been said that air travel is either the quickest or the slowest means of travel—the quickest when everything goes right and the slowest if anything goes wrong.

Closely allied to the certainty of arriving at one's destination is regularity, for a transport service is of little use unless it can be trusted to operate at regular intervals.

There must also be a certain degree of comfort, or perhaps it is better to say a minimum degree of discomfort, otherwise the number of passengers likely to use air travel must be limited.

It is scarcely possible to give in figures the actual values of the requirements mentioned, though it is clear that if we are to make air travel into an ordinary commercial undertaking we must use aircraft in which the chance of accident to a passenger is no greater than the risk by train, motor-car or boat, while the time for the journey must be less than the best achieved by any combination of these means of travel. Regularity, reliability and comfort must approach the standard of the train.

The advantages of increased speed will encourage a proportion of the ordinary travelling public to pay a higher fare which will almost certainly be needed for a number of years. How much extra they will pay must depend upon the extent to which the service possesses the qualities mentioned. In my opinion, it is unwise to concentrate entirely on reducing the cost of running the service; rather we should endeavour to encourage passenger traffic by increasing the safety, speed, regularity and reliability of the service.

If we agree with the foregoing views our attention may be chiefly directed to safety combined with a certain minimum speed, and it is chiefly from these two aspects that the present-day aeroplane and its possible developments will be considered. I know of no example of a helicopter or direct lift machine which has achieved the smallest measure of success as a means of air travel, so that the comparison between the advantages of the two types is difficult. An attempt will be made to see what chance the helicopter has of becoming a rival to the aeroplane, and in order to do this we must touch on the first principles of mechanical flight. I do not propose to go deeply into the matter in a scientific way, as the experimental work on the helicopter is limited.

First Principles

Heavier-than-air machines obtain their support from the air by giving to it a downward velocity. The momentum developed downwards of the air per second is a direct measure of the lift obtained. This applies to both aeroplane and helicopter. With the aeroplane the air is driven downwards by means of planes of suitable shape which are drawn through the air by the reaction of an airscrew. It may be said that the name airscrew is misleading, for an airscrew develops

its tractive force not by screwing its way through the air (as the name seems to imply), but by projecting air backwards. Here again the reaction of the airscrew can be measured in terms of the momentum in the air displaced per second.

In the case of the helicopter the machine is sustained by projecting air downwards by means of wings or airscrews revolving in a plane which is more or less horizontal. The mechanism for obtaining support in the two cases is thus similar, but there is the wide difference that whereas the aeroplane uses the velocity of its lifting surfaces to carry it directly towards its destination, the helicopter planes or propeller blades, whichever you like to call them, have their main motion round a centre which is either fixed relative to the air or requires additional energy to move it. Diagram 1 shows this graphically—in the one case the planes of the aeroplane move from A to B in a straight line; in another they travel along a tortuous path. The energy required is roughly proportional to the length of the line joining A and B, hence it can be seen that the helicopter is at a very serious disadvantage.

Power Used for Support

The planes of a type commonly used in present-day aircraft have a ratio of lift to drag of about 15 to 1 when flying at their usual cruising speed. At 80 miles an hour in air of normal density we find that each effective horse-power spent on the planes alone corresponds to resistance of 4.7 lbs. and thus enables a weight of 71 lbs. to be supported. Taking a propeller efficiency of 75 per cent.—a figure usually obtained in practice—the weight supported per engine brake horse-power is 53 lbs. If we increase the speed of the aeroplane and use smaller planes, keeping the lift-drag ratio constant, the horse-power expended in flight will increase in direct proportion to the increased speed. This does not mean that as far as the planes are concerned more energy or more fuel will be used in travelling a given distance, but it does mean that the engine horse-power must be greater.

In the case of the helicopter it is generally understood that it is not possible to construct a direct lift airscrew that can lift nearly as much as 53 lbs. per horse-power, and if it were possible to do so the weight of the revolving planes themselves would be likely to exceed the weight lifted. The reason for this is simple —the support from the lifting screw is obtained by virtue of the downward momentum of the air in the slipstream. The air has a certain kinetic energy imparted to it which is entirely lost as far as the flying machine is concerned. The kinetic energy is proportional to the square of the velocity of the air in the slipstream, and it may be shown that to achieve a lift of 53 lbs./horse-power implies that the downward velocity must nowhere exceed 21ft./sec., and would in practice need to be less than this. This means that an airscrew to lift 1,000 lbs. must have a diameter of between 30ft. and 40ft., which is scarcely practicable.

This calculation is quite elementary, and is given below.

Lift of helicopter = mass of air dealt with per second × downward velocity imparted

$$= Mv.$$

Horse-power = kinetic energy lost per sec./550 $= Mv^2/2 \times 550$ minimum.

$$\therefore \text{Lift/H.P.} = Mv/Mv^2/1100 = 1100/v$$

M is probably not greater than $\rho v\ A$ where $A =$ "disc area" of helicopter.

$$\therefore \text{Lift}/A = Mv/A = \rho v^2,\ \rho = .00237.$$

If $W =$ weight of helicopter,

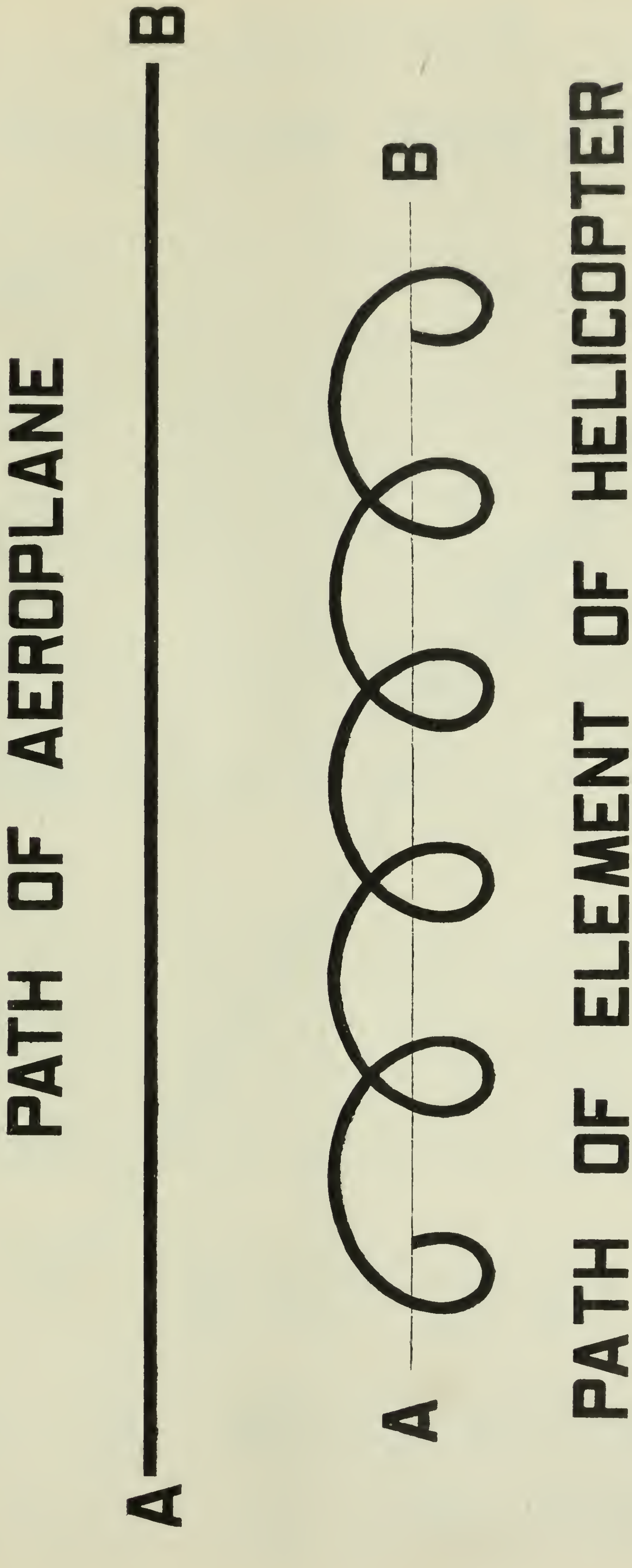
$$v = 1100/W/\text{H.P.}$$

$$\therefore W/A = .0023 \times (1100)^2/W/\text{H.P.})^2$$

$$\therefore \text{for } W = 1000$$

$d = .67\ W/\text{H.P.}$ where $d =$ diameter of helicopter in feet.

and for a lift of 53 lbs./H.P. $d = 35$ feet minimum.


PATH OF AEROPLANE
A
B
PATH OF ELEMENT OF HELICOPTER
A
B

Power Required for Flight

In order to bring the size of lifting screw to a reasonable figure we must increase the downward velocity of the air and consequently the lift in lbs./horsepower must be less. It seems, therefore, that the power required for keeping the aircraft in flight is likely to be much greater in the helicopter than in the aeroplane (see Fig. 2). The resistance of the body, landing gear, and controlling organs of the helicopter is not likely to be less than that of the aeroplane, and we may assume it to be the same. There is, however, an additional resistance due to the framework, driving mechanism and so forth of the revolving propeller blades which is likely to be greater than the resistance of the structure and wiring of the conventional aeroplane.

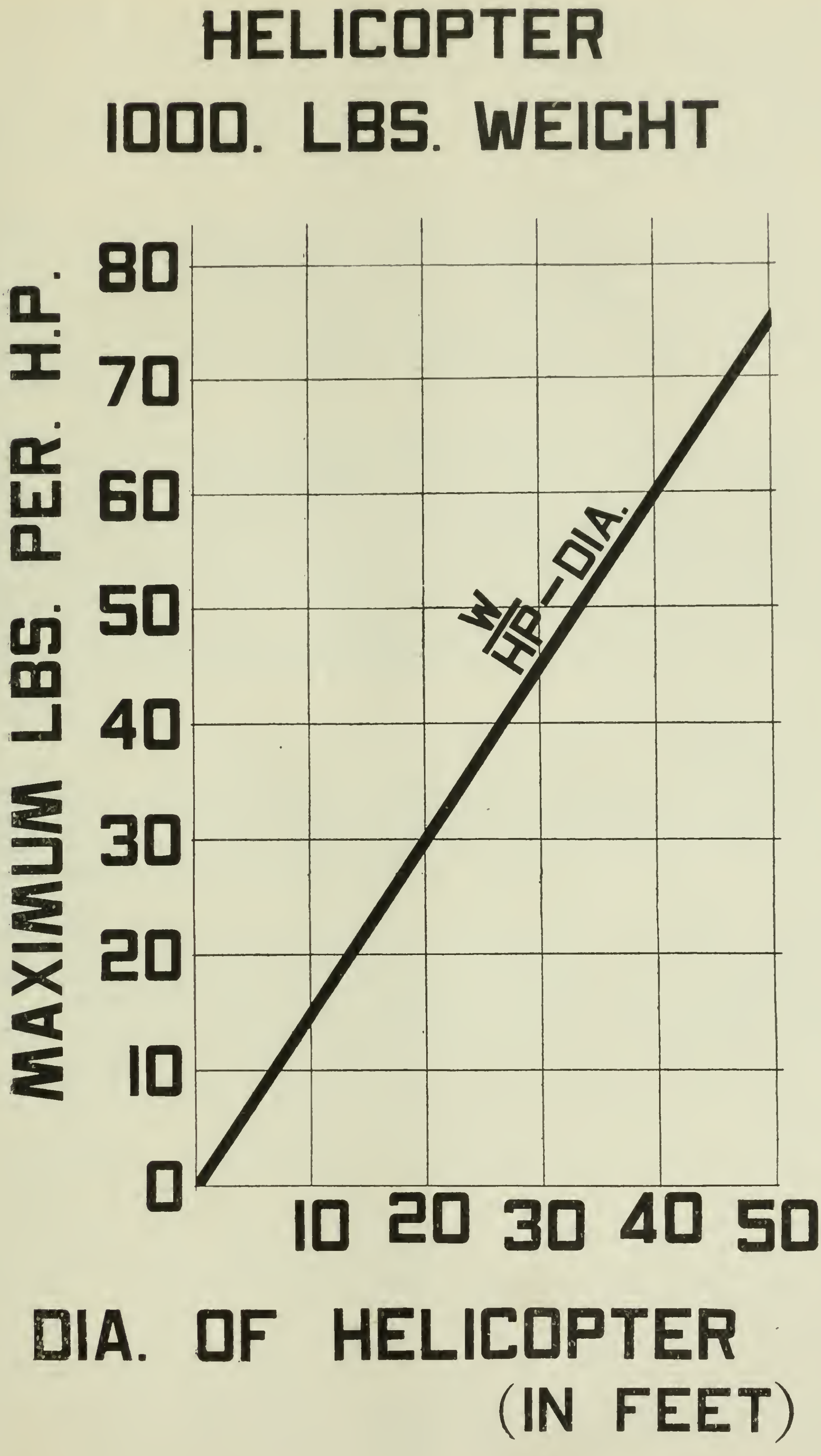
The effectiveness of the lifting screw will be seriously disturbed by any forward motion imparted to the helicopter, for it will mean that at one point of its rotation the propeller blade will have an additional velocity imparted to it by the forward motion of the whole machine, while at another this speed will be reduced by an equal amount. It has been proposed by many inventors to make the angle of the blades vary throughout each revolution. This, however, involves additional mechanism, and the construction of it is scarcely likely to be simple or light. In any case there will be an additional resistance to be added to the whole resistance due to the movement of the revolving propellers through the air.

From the foregoing reasons I am convinced that the power expended in flight in a helicopter, flying at speeds found to be useful for aeroplanes, will be very much greater than for the aeroplane, and I believe that, apart from all other disadvantages, this fact alone will render the machine quite impossible for passenger carrying, at any rate until engines of much less weight per horsepower and materials with much greater specific strength are available. With present-day materials it is my opinion that it is extremely unlikely that it will be possible to make a direct lift machine carrying any useful load which will be able to fly as fast as 80 miles per hour, which is the slowest cruising speed that makes flying worth while in most cases.

Safe Landing

If the conclusions of the last paragraph are correct, it seems that as a means of air travel the helicopter has little or no future, and the fact that it may be possible to rise and land in confined spaces is of little value if the ability to fly from place to place at a reasonable speed is absent. In itself, the advantage of a vertical rise and fall is unlikely to be as great as might be supposed. The difficulty of effecting a landing of a direct lift machine in a wind is certain to be great. In the ordinary aeroplane the presence of a wind helps rather than hinders matters both in getting off and alighting. The case of the helicopter is worse than that of the airship, where considerable skill is necessary to effect safe landings in a high wind.

The idea of being able to lower your flying machine to the ground by the careful working of the throttle is a pleasing one, and at first sight it seems vastly to be preferred to the aeroplane method of approaching the ground with a forward speed of 50, 60 or more miles an hour. The fact remains, however, that aeroplane pilots do not find much difficulty in landing at these speeds so long as they are not forced to alight in unexpected places. The usual reason for forced landings is, of course, the failure of the motive power, and it will be interesting to see how the helicopter compares with the aeroplane in this emergency. The aeroplane method is to glide down to the ground at a speed somewhat above the stalling speed and to use the kinetic energy of the machine to level up and fly parallel to the ground for the last few moments before alighting. So long as there is room, this method presents no difficulties. In the case of engine failure in the helicopter the situation is rather different. Neglecting all difficulties of


HELICOPTER
1000. LBS. WEIGHT
MAXIMUM LBS. PER. H.P.
80
70
60
50
40
30
20
10
0
10
20
30
40
50
W/HP–DIA.
DIA. OF HELICOPTER
(IN FEET)

stability and assuming that the helicopter will keep more or less on an even keel, the whole machine will descend vertically in relation to the air, and its fall will be checked chiefly by the resistance of the supporting propellers. When the engine stops the propellers will either, after stopping, be driven round in the opposite direction or they may have their pitch reversed and travel in the same direction. The terminal velocity of the whole machine unfortunately must be high as the propellers do not offer very much more resistance when spinning than when stopped.

A reference to the experiments on the resistance of an airscrew on an aeroplane will show that the increase of resistance is only of the order of 10 per cent. when it is spinning at the velocity which gives the maximum resistance, as against when it is stopped. It is possible that a specially-designed screw would have a somewhat higher resistance than this, but there seems no reason to suppose that much can be gained. The helicopter, therefore, if unchecked, will strike the ground much faster than is convenient or can be readily dealt with by even an elaborate form of shock-absorbing gear. In addition, the whole machine will be moving relative to the ground at or nearly the velocity of the wind.

There is only one means of checking the fall of the machine known to me other than by the use of parachutes. The method is similar to that used by the aeroplane pilot—the supporting propellers will be revolving at a fair speed and will have in consequence a kinetic energy in virtue of their rotation. If at the last moment the propeller blades are reversed in angle, then their kinetic energy can be used to check the fall much in the same way as is done on the aeroplane. Whether this can be considered a feasible method or not is a matter of opinion, but it is the only method which I can suggest in case of engine failure on the direct lift machine. In any case, the results with forced landings are likely to be serious, for even if the rate of fall is checked there is sure to be some forward velocity due to the wind.

Landing in Fog

Landing in fog when it is not possible to see the aerodrome, the helicopter machine certainly does appear to possess its chief advantage. On foggy days there is usually little or no wind, and it ought to be possible to allow the helicopter to descend vertically at a sufficiently slow rate to avoid serious shock, even if the ground is completely invisible. We have as yet devised no safe method of landing aeroplanes in fogs, and although there is no reason to suppose we shall not eventually manage this, it is likely to be a difficult matter and will require a great deal of organisation. As is known, the usual method of landing an aeroplane is to arrange its flight path so that it approaches the ground at as small an angle as possible. The aeroplane is flown at a speed exceeding the stalling speed, and either the throttle is opened slightly near the ground or else the angle of the planes is increased and the kinetic energy of the whole machine is used to supply the power.

It is generally assumed that if the angle of an aeroplane is increased so that it flies above its stalling angle, disaster is almost certain unless there is room to dive the machine and to regain sufficient speed to fly below the stalling angle. On aeroplanes as usually made, this is to a large extent true. The controls are insufficient to enable the pilot to manage the aeroplane when flying at an angle above the stalling angle. It is as well to mention that by the stalling angle is meant that angle at which the planes exercise their maximum lift coefficient, and when any increase in angle will not increase and may decrease the total lift coefficient. There appears no reason why it should not be possible to fly at angles much above the stalling angle. This subject has been the matter of research during the past year, and it has been found possible to fly an aeroplane at angles of incidence vastly greater than the stalling angle. It seems not unlikely that in the future it will be a safe manœuvre to glide an aeroplane at an angle

of incidence of as much as 45 degrees, and still maintain control. The drag of the planes will be very large, and the path of the aeroplane will be about 45 degrees to the horizontal; consequently the fuselage of the aeroplane will remain nearly horizontal. In the case of a forced landing, or a landing in the fog, it might even be possible to bring an aeroplane down to the ground at an angle of 45 degrees, in which case the forward speed will be so much reduced that the aeroplane will run a very small distance after landing; also owing to the steep angle the errors of judging distance on the ground will be very much smaller. If the engine has not broken down it will probably be possible to straighten up the machine by means of the engine and effect a more or less normal landing. If the engine has broken down, or if it is impossible to see the ground, then it will be necessary to provide a landing gear designed to absorb a much bigger shock than is now customary. In any case it is almost certain that the vertical velocity on landing will be considerably lower than will be possible with a helicopter machine with the engine stopped.

Stability

Another aspect of safety is the question of stability. It is, of course, possible to make an ordinary aeroplane stable in flight, and so long as the pilot is not undertaking any severe manœuvre, such as is sometimes made necessary by engine failure, the risk of accident from loss of stability is unimportant. The stability of a direct lift machine has, I believe, never been worked out numerically. It will certainly be a matter of considerable complication, and it may be anticipated that the structural design to meet the various forces that may occur on the direct lift machine due to sudden manœuvre will vastly increase the difficulty of designing what in any case must be a somewhat complicated mechanism.

Structural Safety

From the point of view of structural safety the helicopter is likely to present grave difficulties. The aeroplane depends for its support upon a system of planes which, except for small movements of the control surfaces, are fixed. In the helicopter the equivalent of the plane structure is dependent upon a number of bearings and working joints which will certainly increase the difficulty of making a safe structure that is reasonably light.

Conclusions

The brief discussion of the problems contained in this paper is meant to represent the argument which occurred to me when considering whether or not it was worth attempting the design of a direct lift machine. It is perfectly true that there are many considerations which would prevent a private constructor starting on such an undertaking which would not, and should not, influence the minds of those directing the official policy of a country in matters of aeronautics. The private constructor must be influenced by financial considerations to a greater extent than would be the Research Department of the Ministry. At the same time, there is only a limited amount of money that can be spent on research and experiment in aeronautics. We have been informed at the last two Air Conferences that experiments on full scale have been, and are being, carried out by the Air Ministry, but we have not been supplied with any details either of the way in which the experiments are being made or of the results obtained, with the sole exception that a year ago we were informed that free flight had taken place. Recently there have been rumours of large prizes offered by the Government for any machine capable of doing certain performances, which include hovering. I do not know the precise arguments which led the Air Ministry to undertake work of this description, but for the reasons given in the former part of the paper it seems to be improbable that any useful result will be obtained unless we can make vast improvements in the technique of the production of power and the making of light structures; such advances would also improve the design and performance of the ordinary aeroplane.

It will no doubt be argued that there are peculiar advantages that might be gained in war from a machine capable of hovering, but if this is so it is suggested in all seriousness that a balloon or an airship is a far more promising method of obtaining the required result. It is likely to cost less and to be safer. The object of air travel is to get from place to place, and it seems highly unlikely that the helicopter type of machine will ever afford a useful means of doing this.

Discussion

Mr. J. Lawrence Hodgson, B.Sc., Assoc.M.Inst.C.E., opening the discussion, said that in the early years of the war he had carried out some very careful experiments on model propellers (including tests on propellers whose plane was inclined to the direction of motion) in order to determine whether it was practicable to build a helicopter. The results of some of those tests were given in a paper which he had read before the Institution of Automobile Engineers, entitled " Tests on Model Propellers," in 1916.

The broad conclusions he had come to were entirely in agreement with those that Major Green had advanced, although his tests had shown that it was quite possible, even with our present development of engines and structures, to build a helicopter which would get off the ground and fly.

A point on which he was in disagreement with Major Green was his statement that when a helicopter was flying horizontally it required more horse-power than when it was hovering. Actually it required much more horse-power to get a helicopter vertically off the ground than it did to fly it at 40 m.p.h. This was because, when a helicopter was travelling forward, the propellers were continually engaging undisturbed air. This is illustrated by the tests summarised in Table I. These tests were taken in water with a flat-bladed propeller. The size of the blades was 0.75in. x 0.4in. (the long axis being set radially). Their thickness was 0.01in. The distance between the centres of the two plates forming the blades was 2.25in. The blades were set at an angle of 15 degrees to the plane of motion. The propeller was immersed in water which was flowing parallel to the plane of the propeller at the velocities tabulated. In each series of tests the thrust was kept constant by diminishing the revolutions and the torque as the velocity of the water past the propeller was increased.

It will be seen that the thrust per horse-power is very much increased when the fluid in which the propeller is working is moving parallel to the plane of rotation of the propeller. There is a similar, though smaller, increase when the fluid moves at a small angle to the plane of the propeller. When, however, this angle is increased to more than about 20 degrees, the tests show that the thrust per horse-power ceases to increase as the relative velocity between the propeller and the fluid in which it works is increased.

The tests quoted are two out of a considerable number which were taken in 1915 on flat-bladed propellers, whose plane of rotation was inclined at various angles to the direction of motion of the fluid in which the propellers worked.

Table II. gives some of the actual observations, and Table I. the faired-up results.

TABLE I.

Vel. of water at right angles to the propeller axis in feet per sec.	Thrust per horse-power in lbs. Thrust 0.08 lbs.	Thrust 0.16 lbs.
0	190	135
1	250	164
2	315	200
3	395	245
4	480	283
5	570	330

TABLE II.

Vel. of water at right angles to the propeller axis in feet/sec.	Thrust 0.08. Torque (in. lbs.).	Thrust 0.08. Revs. per min.	Thrust 0.16 Torque (in. lbs.).	Thrust 0.16 Revs. per min.
0	0.042	648	0.0843	915
2.26	0.0309	480	0.0642	738
3.75	0.0281	397	0.0575	619
4.90	0.0253	285	0.0574	558

The fact that the thrust per horse-power of the propellers *does* increase as the helicopter moves forward is one that should be clearly recognised by the opponents as well as by the advocates of the helicopter. If Major Green had added to his diagram comparing the flight paths of the wing of an aeroplane and the blade of a helicopter screw, the path of the blade of the propeller actuating the aeroplane, the comparison would not have been so greatly to the disadvantage of the helicopter.

If one imagined a helicopter which had horizontal motion before it got off the ground (see Fig. 20 of the paper* before the I.A.E.), such a machine could get off the ground by running along and gradually climbing, and would require very little more horse-power than the equivalent aeroplane. Similarly, if the engine broke down and it was still possible to maintain the stability of the machine, it could glide down as the aeroplane does. His tests (which he believed were the first ever taken on propellers having a negative speed of advance) further showed that if the engine-power of a helicopter was not all contained in one unit—supposing it had four engines, three of which broke down—it would be possible, by maintaining the screws rotating at one quarter of their full horse-power, to come down vertically at by no means dangerous velocity. Yet, in spite of all this, when one took into consideration the extraordinary complexity of the helicopter, the difficulty of balancing, the difficulty of landing accurately without damaging the propellers, or of landing at all in a confined space when any wind was blowing, the conclusion he had definitely come to was that—since the aeroplane had been developed, it was foolish, when the amount of money available for research was limited, to try to develop the helicopter, which would necessarily be a much more cumbersome and complex mechanism for achieving flight. This was stated as follows in his paper before the Institution of Automobile Engineers :—

> "The final conclusion come to was that a helicopter which would fly successfully could be built, if desired. Such a machine appeared, however, to have many disadvantages as compared with the aeroplane, such as the great weight of the gearing necessary (at least 1½lbs. per horse-power), the unsuitability of any of the ordinary forms of power transmission for use on a lightly-built and easily distorted framework, the very great liability to mechanical breakdown under gunfire, the enormous gyroscopic forces induced in the propellers if the inclination of the machine were suddenly changed, the serious danger of straining the propellers if the landing were at all faulty, and the almost imperative necessity for the use of gyroscopic control with its attendant complications.
>
> "The most practical solution of the helicopter problem seemed to be that shown in Fig. 24, where two aeroplanes circle round an observation car, which, by means of gearing, is prevented from rotating.
>
> "This solution of the helicopter problem, while eliminating most of the difficulties above enumerated, did not seem to be sufficiently promising to be worthy of further development."

Continuing, Mr. Hodgson said he believed certain Governments (he would not say which) were experimenting on the lines essentially of two aeroplanes tied

* Reprinted in Appendix I. of the present Paper.

together at the wing tips and circling round a stationary observation car for the purpose of maintaining a definite position in order to take observations. He thought expenditure of public monies on such experiments was very much to be regretted.

Mr. A. Fage said that he considered Major Green had made out a very good case against the helicopter, and he believed a case which could be strongly supported at the present time. In connection with the descent of a helicopter with the engine cut out, he wished to refer to some experiments recently made by Messrs. Lock and Bateman at the N.P.L. The results of these experiments had not yet been fully analysed, so that he spoke with some reserve. The experiments seemed to show that even with the blades at a small negative angle, the resistance of a helicopter was unlikely to be greater than that of a flat plate of the same disc area. This meant that if for a helicopter of 1,000 lbs. the speed of descent were limited to 16ft. per second, the diameter would have to exceed 60ft.

Major Low recalled his experience of helicopter design as far back as 1912, when he had to work out in detail a general design put forward to his firm (Messrs. Vickers) by a well-known aeronaut.

The engine was given and the dimensions of the airscrews, and the problem was to design the framework drive, etc. When the details were worked out on orthodox lines with the minimum factor of safety that could be accepted, the structural and power plant weights far exceeded the most optimistic estimates of the lift to be expected.

The instruction was then changed to that of getting out a light enough framework regardless of such trifles as factors of safety, but this having been done, it was reported that in the estimation of the design staff the whole apparatus would collapse as soon as the engine was opened out. This restrained the original designer from going ahead on his own responsibility, and the matter was finally dropped. At the time aeroplanes were being designed with a reasonable performance as a matter of routine.

Major Green's paper showed that the very same difficulties were still present, and that although improvements in glider and power plant weight coefficients had made very considerable advances, yet relatively the helicopter was hopelessly handicapped, and always would be.

He would like to draw the conclusion implied in the paper and in the remarks of a previous speaker. If they moved the principal weights outward and made them move with the moving blades or wings of the helicopter, then in descent at a vertical speed comparable with that of an aeroplane, and with the wings supposed adjustable to the best gliding angle, at the last moment before landing the incidence could be altered to that of maximum lift by stages, the energy being supplied by the rotating masses, and a landing made with no vertical velocity in a manner exactly analogous to an aeroplane. But the design would approximate in this extreme case to two normal aeroplanes attached at the wing tips and flying round each other in narrow circles. He did not require to explain there how hopelessly inferior such a combination would be. Major Green had also omitted to consider the effect of horizontal movement in increasing the effective lift of a helicopter, but even with this apparent advantage there would be the countervailing disadvantage of greatly increased stresses, requiring great increase in weight, and moreover if a helicopter had to run along the ground to attain flying speed, farewell to the machine that was to rise vertically from a restricted space. Major Green had done a great service if his paper moderated ill-informed enthusiasm, and dispelled the fascination exercised by the idea of hovering over the lay mind.

Mr. F. Handley Page said he had not heard the paper read, but when he had read the title "Air Travel, with Special Reference to the Helicopter," he

had thought what a marvellous opportunity this was for the proposed new Civil Aviation Company. Those who might have had doubts as to what were the objects of the Company, or on what its funds would be expended, would at last find some object sufficiently large on which the capital could go. (Laughter.) He did not know whether anyone had taken part in the discussion who was a very great advocate of the helicopter, because he could quite imagine someone pointing out that twenty years ago Lord Kelvin had said that flying was impossible with an ordinary aeroplane, and someone might say that twenty years from now the helicopter would be a possibility. He personally thought it was somewhat dogmatic, with all due deference to the lecturer, to say that the helicopter, although perhaps not so efficient as the aeroplane, would not be of some use twenty years hence.

There was one very interesting semi-helicopter which he had seen in Madrid a short while back, and he did not know whether members of the Society had seen a description of it. This consisted of an ordinary aeroplane fuselage at the centre of gravity of which was fixed an inclined vertical shaft, the angle of inclination being a few degrees back from the vertical. On this inclined shaft was what appeared to be a four-bladed propeller, between 15ft. and 20ft. in diameter, the blades consisting of fabric-covered planes of approximately 1ft. chord, the cross section of each being ordinary aeroplane section. These planes were set at zero angle of incidence to the plane of rotation. Instead, however, of this propeller being driven round positively by the engine, it was free to rotate. The fuselage was fitted with an ordinary engine propeller, and when the machine commenced to run along the ground to take off like an ordinary aeroplane, the blades of the horizontal propeller started to rotate, and owing to the forward component of the vertical reaction on the blades, the blades rotated in a way which seemed at first contrary to what one would expect, namely, the leading edge of the aerofoil section moved forward. Owing to the increased speed obtained by the velocity of rotation, a higher lift was obtained than if the planes were stationary. To equalise the lift on the two sides, a very ingenious device was incorporated by the inventor. The blades were pivoted at their interior end, approximately 2ft. from the axis of rotation, and the interior end was held in position by rubber shock-absorber cord. In consequence of this arrangement on the side in which the velocity of rotation and translation were the same, the blade was allowed an upward movement, thereby diminishing the effective angle of attack. The reverse effect took place on the other side, so that the angle of attack was increased and the lift correspondingly increased. By this means the lifts on the two sides were equalised.

This machine, although somewhat complicated, had actually flown, and seemed to alight very slowly. Whether, however, such a device was really good and worth the complication involved is open to question.

Colonel Heckstall Smith said he was afraid the lecture would have a most depressing effect on the Press, because one thing the Press really loved was a helicopter, and he would like the lecturer to let the members of the Society know, if possible, what results had been distributed among designers as the result of the almost encouraging expressions of opinion made by the Air Ministry at the Air Conference before last, held, he believed, more than a year ago at the Guildhall. We had been told by the Under-Secretary of State for Air that "Substantial progress had been made towards the successful solution of one of *the chief problems* of aeronautics—that of vertical flight by means of a helicopter." The emphasis on "one of the chief problems" is mine, but the importance of such an expression in view of this lecture is noteworthy. Further we were told that "efforts are proceeding and public money is being spent" and our hopes were raised, so much so that the papers were full of it, but from that day to this he did not think anything further had been heard. It would be well if the Society could obtain some information as to what had been done, so that designers who

might have been encouraged to take up what the Air Ministry had suggested was such a good line of thought might know whether it was still a good line of thought. At the same time, he hardly considered that, after Major Green's lecture, anyone connected with designing would be very much encouraged to go on in that direction.

Major WIMPERIS said that it would seem that the more successful a helicopter design proved to be, the greater would be its similarity to two ordinary tractor machines fastened together and rotating round a common centre. If this were so, since we have plenty of good tractor machines now, we could, he supposed, by combining a pair of them so as to rotate about a common centre, have the best form of helicopter already. Major Green had called his paper "Air Travel, with Special Reference to the Helicopter," but the helicopter's special reference was not to air travel, but to "hovering." No one would think of fastening two tractor machines so as to face in opposite directions as a means of air travel, though they might certainly do so if "hovering" were the object.

The CHAIRMAN, referring to the fact that not much had been heard about the Air Ministry experiments on helicopters, said the members of the Society would like to know exactly what scientific advice the Air Ministry had taken before putting them in hand.

Major GREEN, in reply to the discussion, expressed his disappointment that there was no one present who had sufficient faith in the helicopter to speak on its behalf. Mr. Hodgson's remarks were very interesting to him, and he was sorry that he had not come across Mr. Hodgson's paper, because it seemed to him that had he done so, he would not have read his own paper at all. Mr. Hodgson must have covered the ground much more adequately than he himself, and also had some actual experiments on which to base his figures, whereas he himself had not made experiments but had had to depend on first principles and such information as he could collect. With regard to the helicopter taking less power when moving forward, he could quite imagine that, because they were approaching nearer and nearer to the aeroplane. Anyhow, he would be surprised if the helicopter, when actually moving forward, did better than the figures given in Diagram 2, because in that case he had taken no account of interference in the ordinary way, and he had assumed that all the air moved by the propeller blades was moved downwards with uniform velocity. It was very unlikely, unless they approached so near to the aeroplane that the apparatus would cease to be a direct lift machine at all. He believed, in any case, that Mr. Hodgson and himself were in complete agreement, and he was pleased to have Mr. Hodgson's confirmation of the results which he had arrived at by other means.

With regard to flattening out, of course, if they put the weight further and further outside, they did approach Major Wimperis's two aeroplanes tied together. That was a helicopter of sorts, but as a means of air travel it did not seem to possess any great advantages. It seemed to him that, if they could find a skilful pilot, he could, by banking his machine sufficiently, go round in such small circles in an ordinary aeroplane that he could almost hover. With two machines tied together they neutralised the centrifugal forces, which enabled them to go round in small circles, but he thought it was unlikely to be a success as a form of travel.

He had not heard Mr. Fage's remarks, but he gathered he had said that the fact that the planes were revolving did not help very much. The suggestion that perhaps they could design a special sort of airscrew did not seem to be justified.

Major Low had given some very sound advice. He believed he could explain to Major Low why the helicopter had such a fascination for the inventing type of people, and possibly for the official mind. When quite a small boy he had read a book called "The Clipper of the Clouds," which described an airship which was a sort of boat with a lot of lifting screws. That had made a big impression on

his mind, and he supposed on those of other people, and he thought that was really why so much attention was given to this direct-lift business.

With regard to Mr. Handley Page, he did not quite follow the advantages of the machine he had sketched, but if it had achieved flight it had achieved something. Mr. Handley Page had suggested that he (Major Green) had been rather dogmatic, but he believed Mr. Handley Page would withdraw his remarks if he read the paper carefully. He (Major Green) had not said the helicopter could not fly, but merely that, as a means of air travel, for the reasons given in the paper, there did not appear to be much hope for it, and therefore there were other things it would pay better to develop than to spend our limited resources on the helicopter.

He had anticipated such a question as that asked by Colonel Heckstall Smith. Therefore, he had written officially to the Air Ministry asking whether he might be supplied with any information with regard to direct-lift machines which they had advertised as having achieved free flight rather more than a year ago. He had received the usual reply—that nothing further could be added to the information already given. That suggested two reasons. One was that it was a frightfully important experiment that was being carried out, and the other reason he left his hearers to imagine. When he had mentioned to one man that it was stated at the Air Conference that the helicopter had achieved free flight, the reply was "So has my umbrella." A friend of his had visited a cinema on the day previous to the meeting, and had seen a picture of a machine which carried two gentlemen. According to the pictures the machine had achieved a free flight of two minutes; it had got off the ground to the extent of two or three feet, and had moved in an aimless way, and the pilot apparently landed without smashing himself. He did not dispute that free flight had been achieved, but in his paper he was dealing with the helicopter as a means of air travel.

He joined with Professor Bairstow in asking on what scientific advice the experiments had been made on direct-lift machines, but he was afraid that the reply would be that it was not in the interests of aviation that the information should be divulged. He daresay that would be right.

A hearty vote of thanks to the lecturer concluded the proceedings.

Dr. WATTS (*communicated*): Major Green has expressed very clearly what I think is the considered opinion of most technical men in this country who have seriously considered the problem of helicopters.

I have often illustrated the fundamental difference between the helicopter and the aeroplane by likening them to two men who one night came out of Trafalgar Square tube station both feeling very cold. They most correctly decided that in the circumstances the only way to get warm was to run. But one—the helicopter man—ran round and round Trafalgar Square; the other man ran straight home. They both got warm, but one arrived home and the other arrived nowhere.

There is a point which I think the lecturer has not sufficiently emphasised, that is the performance of a helicopter at any respectable altitude, and I think I might usefully quote from my contribution to the discussion on M. Damblanc's paper before this Society.

There referring to M. Damblanc's proposals, I am reported thus:—

"The lift he obtained worked out at 13½lbs. per horse-power. This was not an exaggerated figure. He (the speaker) would expect a higher figure. He thought 20lbs. gross lift per horse-power might easily be obtained. If not, the helicopter would be of no particular value. The weight of the helicopter proposed by M. Damblanc was 2,640 lbs.; the lift at ground level was about 3,000 lbs. The ceiling could therefore be little more than 5,000 feet. At the ceiling all the thrust would be required merely to support the machine and the translational velocity would therefore be zero. Under these conditions its powers of manœuvring would be almost negligible, and he put it to the meeting that an aircraft with only a ceiling of 5,000 feet, with no trans-

lational velocity at that height and with little power of manœuvring, had no serious military value. It would be particularly vulnerable to anti-aircraft fire and to attack from quite ordinary aeroplanes."

I feel quite safe in saying that if a helicopter ever becomes possible it will only be possible for very low altitude work.

For the reasons which Major Green has given and those which I expressed in the discussion alluded to, I very much deplore the expenditure which has been and possibly is still being incurred by our Air Ministry in research on helicopters, and I should again like to quote from a contribution I made to a discussion at the Second Air Conference, 1922. I said :—

"The research which is now being carried out on helicopters is a research which, in the interests of economy, might well be stopped, without involving any serious loss of Great Britain's position in aviation. I am sure that in making that statement I shall carry with me most of the technical men in this room.

"Generally speaking, in deciding its policy of research, the Government have been guided by its Advisory Committee in the past or the Aeronautical Research Committee in the present, or by its own technical advisers in the Air Ministry itself. I do not think that the research on helicopters which we are told is now proceeding has ever been suggested or asked for either by the past Advisory Committee or by the present Aeronautical Research Committee. I would go further and say that the research was started against the advice of the Air Ministry's own technical advisers. I speak with some knowledge of the facts since I was myself one of those technical advisers at the time when this research was initiated.

"I do not wish to enter into technical details, but there seems to be a large amount of misunderstanding concerning the difficulties to be overcome in the evolution of a helicopter of any commercial or military value. Both at this Conference and in the newspapers of other countries we have heard it almost triumphantly announced that a helicopter has succeeded in rising a few feet under its own power. This result seems in the mind of many to justify the thousands of pounds which have been spent. The Director of Research has suggested to this Conference that it is an encouraging result.

"The helicopter is nothing more or less than a propeller exerting a thrust vertically instead of horizontally, and I confidently assert that for many years we have had sufficient knowledge of propellers to have predicted this result without the expenditure of money which has taken and is still taking place. The technical men of this country have not asked for research on helicopters, not because they doubted whether such a machine could rise vertically from the ground, but because they could not see of what use it would be, or how it could be controlled and rendered stable when it had done so. I do not, as I have said, wish to go into technicalities, but I would suggest that the Secretary of State for Air should seek the advice of his Aeronautical Research Committee or any other external authority on the question whether research on helicopters is justified in the present stringent financial circumstances."

Captain DE HAVILLAND (*communicated*): A reasoned estimate of the capabilities of the helicopter has been badly wanted, and is now supplied by Major Green's paper.

It is true that there are many experiments which could be justifiably undertaken with the aid of public funds which no private concern could entertain in the present condition of the industry.

The kind of experiment referred to is that in which there are many difficult practical problems to be solved—generally at considerable cost—but in which there

is a definite gain in view; a gain (of which some estimate may be made) over existing methods of doing the same work.

It is this preliminary case for the helicopter which never seems to have been made out, and any attempt to do so leads to conclusions similar to those reached by Major Green, who, in order to make discussion of the problem possible, has certainly not exaggerated the difficulties.

It is difficult to avoid the conclusion that the whole experiment is a waste of money.

TESTS ON MODEL PROPELLERS

BY J. LAWRENCE HODGSON, B.SC., ASSOC.M.INST.C.E.

Reprinted from "Proceedings of the Institution of Automobile Engineers."

Appendix I.

The investigation summarised in the two preceding papers formed part of a rapidly carried out research which was made at the commencement of the war in order to determine whether a machine on the helicopter principle could be built, and whether, when built, it was likely to prove of any special value for belligerent purposes.

The method of flight upon which the idea of the helicopter is based was probably invented by Nature far earlier than the "flapping wing" method. Microscopic rotifers of very ancient ancestry, whose waving *cilia* enable them to hover, and to move freely in three dimensions, may be discovered in stagnant pools.

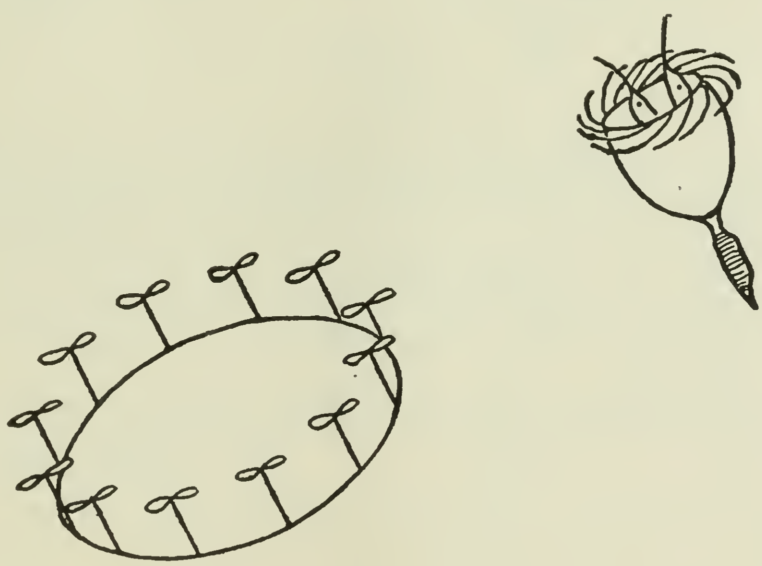


FIG. 16.

Fig. 16 shows one of these creatures, and also the diagram of an equivalent arrangement in which propellers are employed.

Leonardo da Vinci was probably the first who seriously considered the helicopter principle as a possible basis for achieving flight. The actual machines which he attempted to construct were, however, based upon the "flapping wing" principle.

Many years ago Jules Verne's book, "The Clipper of the Clouds," gave a popular exposition of the possibilities of the helicopter.

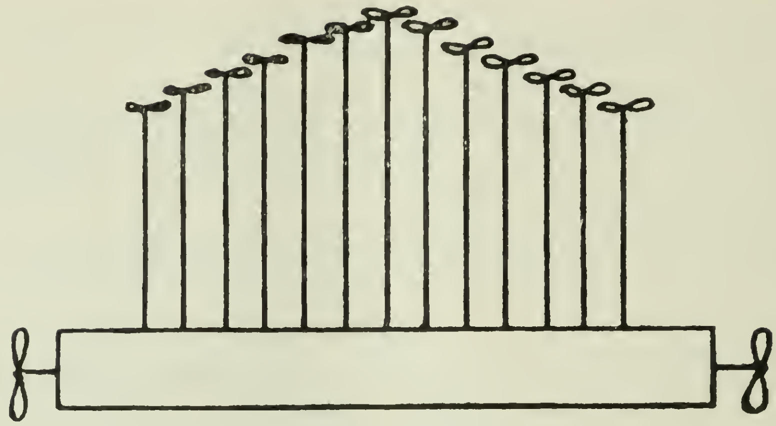


FIG. 17.

The machine which was therein described, Fig. 17, was faulty in conception, in that it should have travelled broadside on, instead of lengthways, in order to avoid interference effects between the lifting propellers, while the forward motion could have been more easily obtained by tilting the whole machine in the plane of motion. Such a re-modelled " Clipper of the Clouds " is shown in Fig. 18.

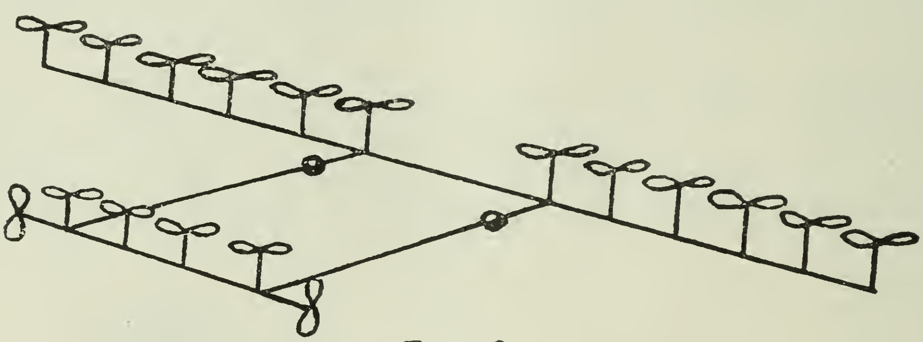


FIG. 18.

If the torque and the gyroscopic effects are to be balanced, and the machine is to be able to tilt itself, the least number of propellers that can be employed is 4.

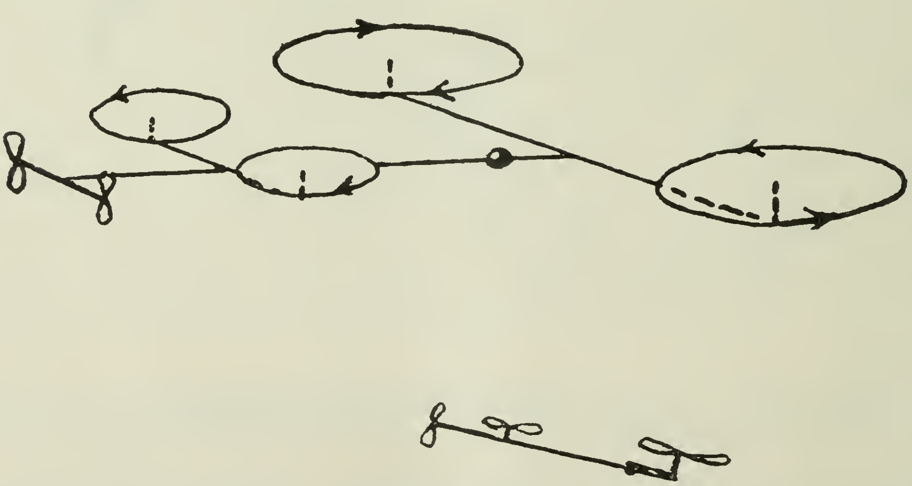


FIG. 19.

A four-screw helicopter is shown diagrammatically in Fig. 19. In this figure there are two additional screws for steering, and for turning the machine when it has no velocity of translation.

It was assumed that such a machine, if built, would have a central power plant of, say, three or four independent engines, and that there would be some means of transmitting to the propellers all the power which was developed by the power plant in the event of the failure of one or more of the engines. It was therefore of interest to determine the speed of vertical falling when the power delivered to the propellers was too small to enable the machine to rise vertically. For this reason the values of the revolutions and the torques at various *negative* speeds of advance, given in Figs. 4 and 5, were determined. Calculations based upon the results there given show that quite a large proportion of the engine

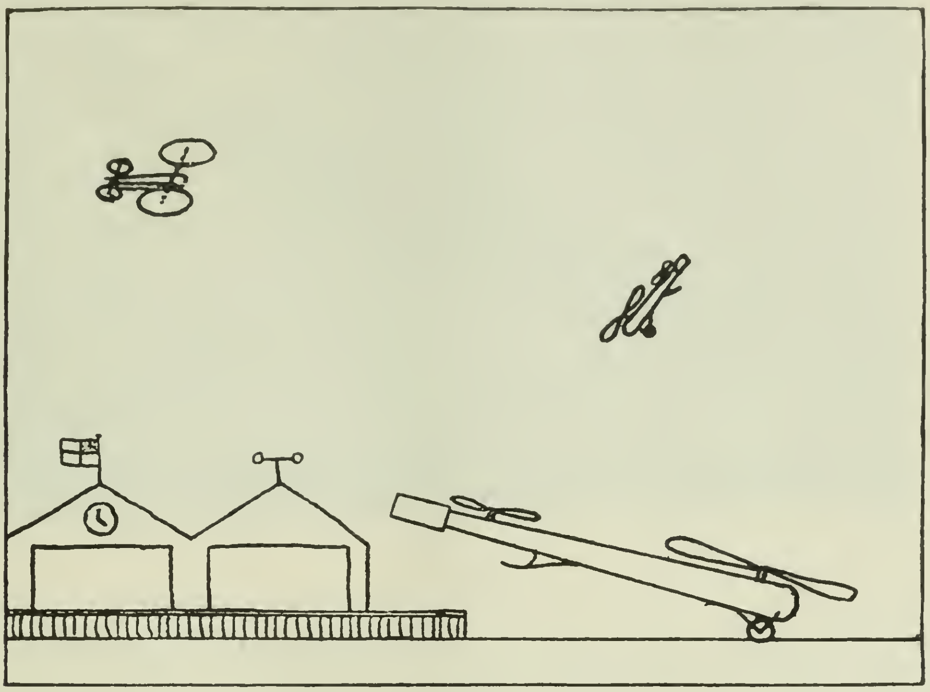


FIG. 20.

power necessary to hover may be lost without impairing the power of the machine to descend vertically at by no means dangerous velocities.

A number of tests taken with the apparatus shown in Fig. 15, with propellers inclined to the direction of motion, showed that it would be possible to glide down with safety even if the engine power failed altogether; provided, of course, that the balancing arrangements were such that the stability of the machine was not thereby impaired, and that the machine was fitted with the necessary landing wheels.

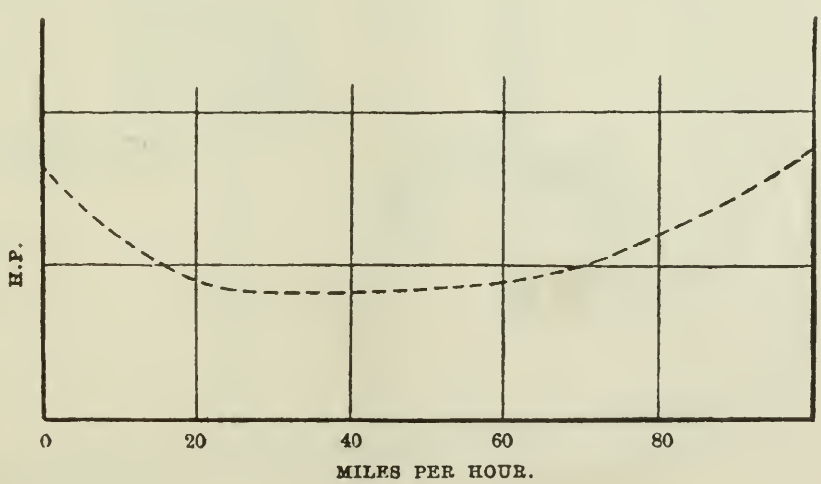


FIG. 21.

The same series of tests upon inclined propellers also showed that it would be possible to construct helicopters of very much smaller size than the unwieldy hovering machine first considered; provided that these smaller helicopters ran along the ground so as to obtain an initial velocity before rising. Such machines are indicated in Fig. 20.

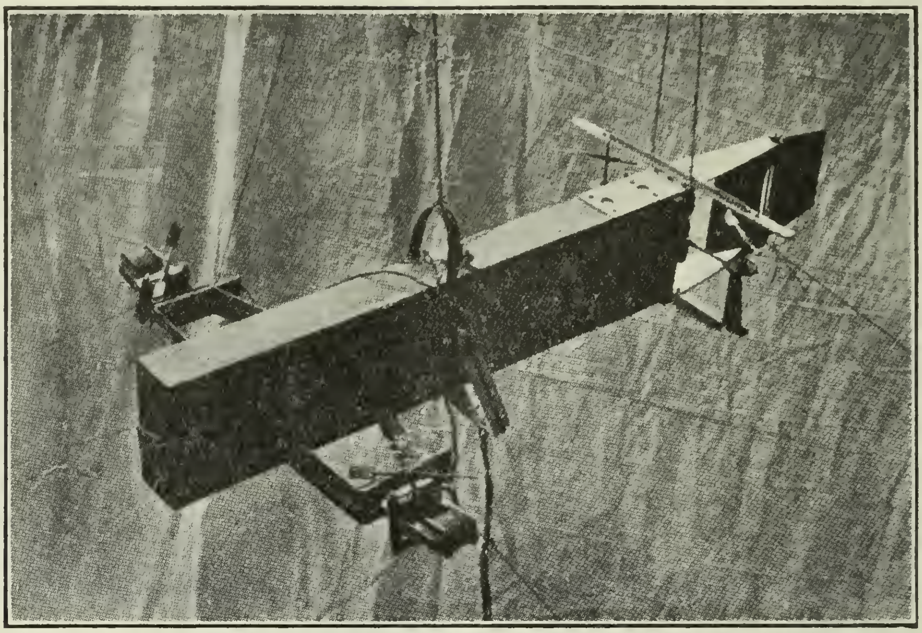


FIG. 22.

Another interesting point which was brought out by the tests on inclined propellers was that the horse-power required to enable a helicopter to hover was sufficient to propel it horizontally at over 80 miles per hour, and about double that required to propel it horizontally at 40 miles per hour (see Fig. 21). This is due

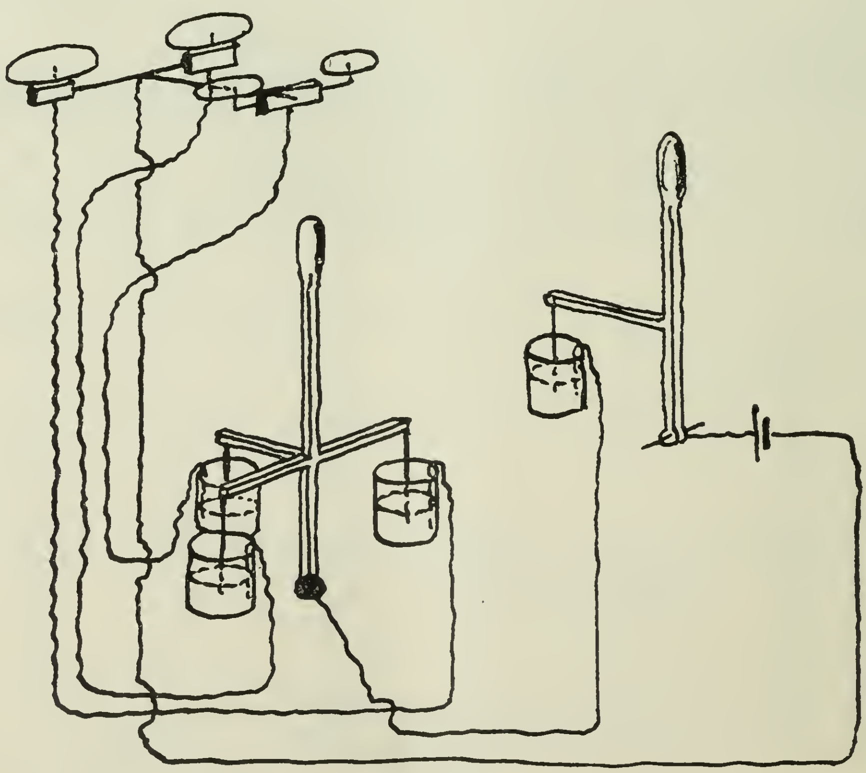


FIG. 23.

to the greater volume of air "engaged" by the propellers when the machine travels forwards.

In order to test the difficulty of balancing a helicopter when in flight by means of a hand control apparatus, the electrically-driven model shown in Fig. 22, Plate XV., was made. This model "flew" in water—hence its rather bizarre proportions—and was controlled by means of liquid resistances, as indicated in Fig. 23. It was found to be possible to control a model in this way very easily.

In spite of this, however, it was felt that if an actual machine were built, some form of gyroscopic control would be necessary.

The final conclusion come to was that a helicopter which would fly successfully could be built, if desired. Such a machine appeared, however, to have many disadvantages as compared with the aeroplane—such as the great weight of the gearing necessary (at least 1½lbs. per h.p.), the unsuitability of any of the ordinary forms of power transmission for use on a lightly built and easily distorted framework, the very great liability to mechanical breakdown under gunfire, the enormous gyroscopic forces induced in the propellers if the inclination of the machine were suddenly changed, the serious danger of straining the propellers if the landing were at all faulty, and the almost imperative necessity for the use of gyroscopic control with its attendant complications.

CONSOLIDATED WOOD

A NEW MATERIAL FOR VARIOUS INDUSTRIES

BY W. R. TURNBULL, M.E., F.R.AE.S.

When engaged on war work as Chief Inspector for Fredk. Sage & Co. at Peterboro', England, in the spring of 1918, I observed that when Honduras mahogany was compressed across the grain, and parallel to the annular rings of the wood, a "first" or "natural" elastic limit was reached, after which the deflections *increased* more rapidly than the load, as has been commonly observed in all testing of all materials.

If, however, we continue increasing the load, crushing does not occur as one would expect, but after a period, as the load is increased the deflections *decreased*, and this decrease was more and more accentuated up to the point at which the load crushed the sample under test, which point was *many times* (15 to 17 times or more) the "natural" elastic limit of the wood.

This is illustrated in Curve "A," Fig. 1, which shows the general characteristics of three methods of compressing wood with typical results, viz. :—(*a*) Wood compressed across the grain and parallel to the annular rings, (*b*) across the grain at right angles to the annular rings, and (*c*) "end compression"—the load being parallel to the grain on the end of the fibres.

I also found that if the load was thus carried, in compression parallel to the annular rings, far beyond the elastic limit, but was stopped before the crushing point was reached, and the wood was removed from the press, this wood, which I term "consolidated wood," will "recover," and in two or three days assume a permanent "set," after which any change in its dimensions will be extremely slow (see Note on "Recovery").

After ten weeks the samples of Honduras mahogany, thus "consolidated," were subjected to various tests, and it was found that this "consolidated wood" *was much stronger* in every way than the natural wood from which it was made, and moreover the consolidated wood had *little tendency to shrink* when exposed to a high temperature (see Note on "Effects of Heat").

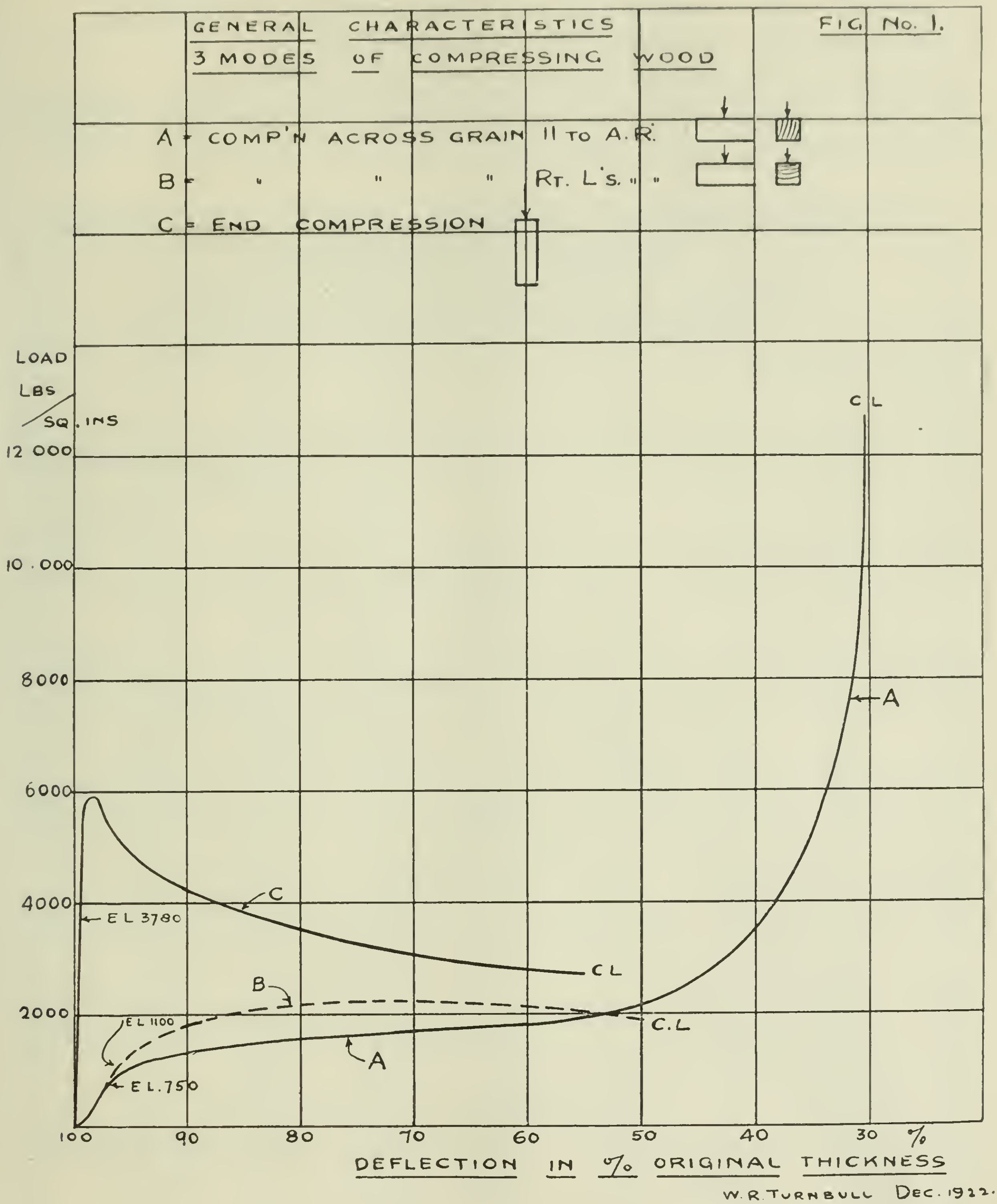
I have since found that these properties of Honduras mahogany are not peculiar to this wood alone, but that all woods, probably, when consolidated have the same properties to a greater or less degree; but it is possible that deciduous ("hard") woods have the properties to a greater degree than the "soft" woods, although this is a matter for further investigation, as my tests have been limited to six woods only.

The reason for the increase of strength in wood treated in this way is rather obscure, but in some tests, in which samples of seasoned birch were consolidated (in a mould) to 15,000 lbs. per square inch, it was noted that a gum was squeezed from the ends of the samples, and it is therefore believed that this gum acts as a natural cement, which holds the fibres in closer contact than that in which they naturally grow, and thus a stronger and denser wood is formed.

When wood is consolidated the density is increased, but in general the proportionate increase in density is considerably less than the proportionate increase in strength. Compare Figs. 2, 3 and 4.

I have been unable to carry out a very extended research in this interesting subject, but I here present the results as far as my work has gone.

For convenience of reference and comparison the results are best shown by the curves of percentage increase plotted against the amount of previous consolidation in lbs. per square inch in Figs. 2, 3 and 4.



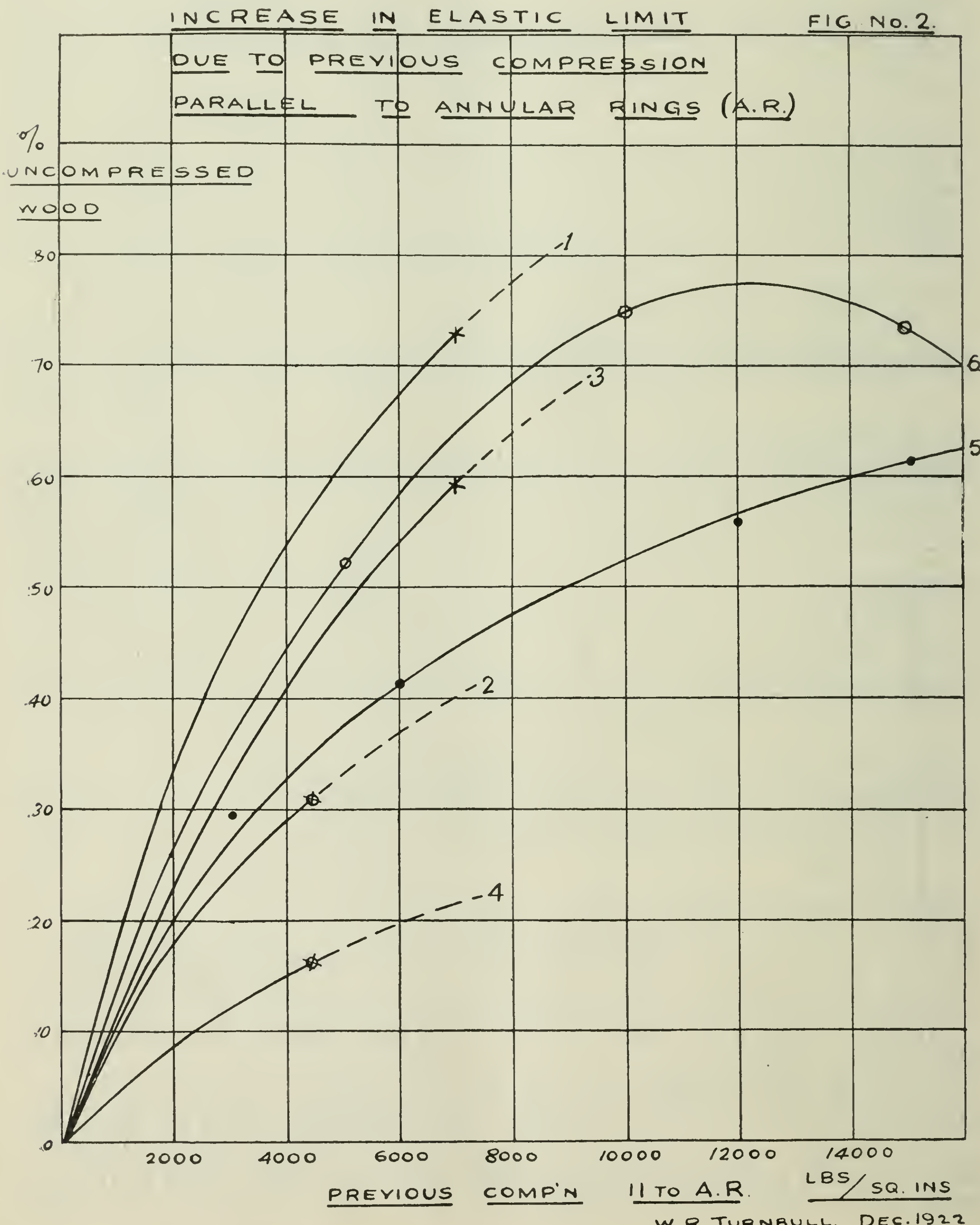
In these we have the results for four woods that have been consolidated and later tested as follows :—

(1) Compression across the grain at right angles to the annular rings, (2) compression on the ends of the fibre "end compression," and (3) bending.

The number of the curves are tabulated as to wood and method of test as follows; and the same numbers apply to the same woods, and method of testing, in each of the Figs. Nos. 2, 3 and 4.

Curves No. 1.—Honduras mahogany, tested in compression across the grain and at right angles to the annular rings.
,, ,, 2.—American white oak, tested as above.
,, ,, 3.—Honduras mahogany, tested in end compression.
,, ,, 4.—American white oak, tested in end compression.
,, ,, 5.—Yellow birch, tested in bending.
,, ,, 6.—American hickory, tested in bending.

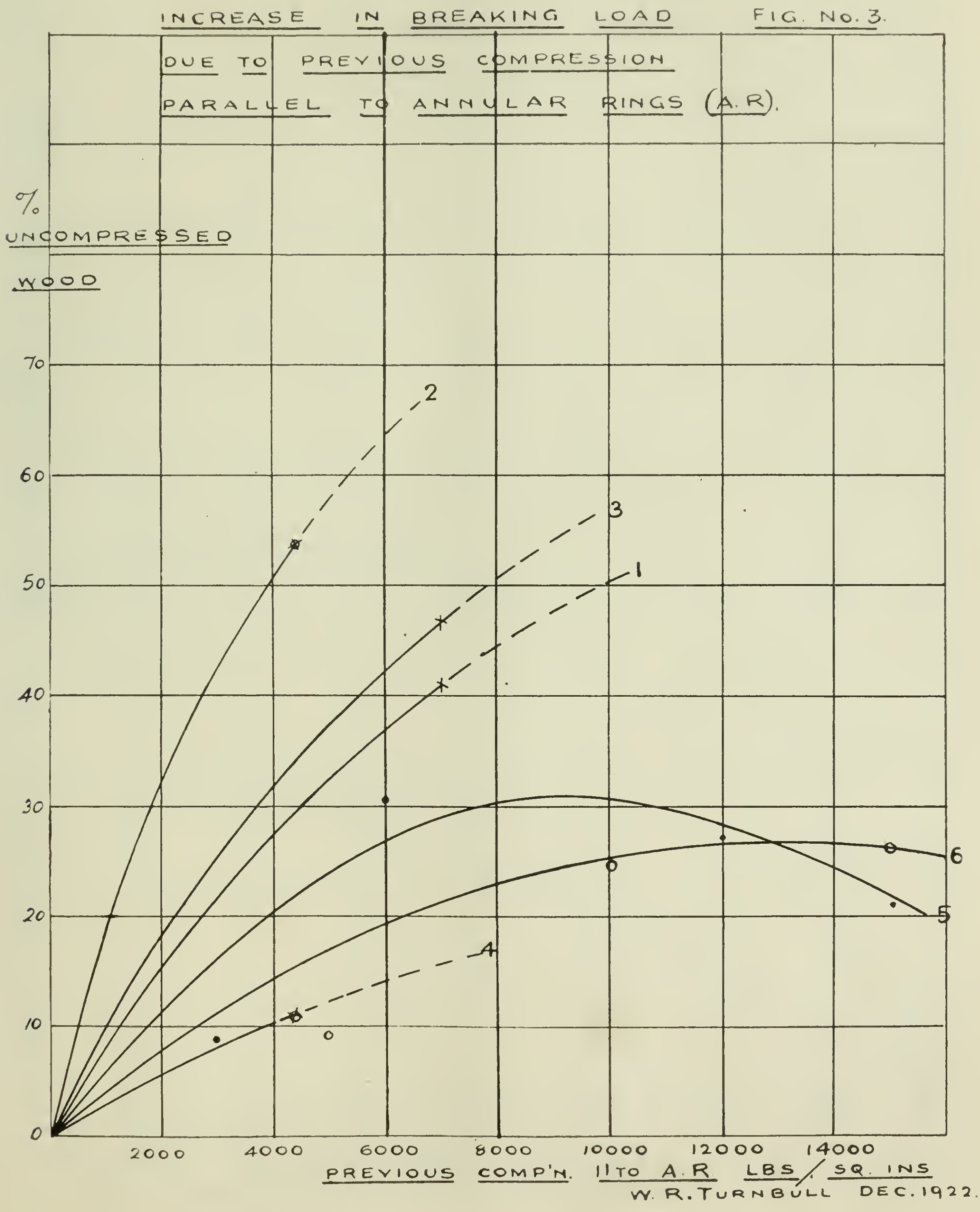
(When the samples were first consolidated, the sides were not confined for Nos. 1 to 4, but in the case of 5 and 6 the sides of the samples were confined by an iron " mould " to prevent side-bulging.)



Referring to Fig. 2, it will be noted that the elastic limit is increased by previous "consolidation" from 22 to over 80 per cent., depending on the wood and the manner of testing; and in the case of hickory, under bending test (Curve No. 6) a well-defined maximum occurs at about 77½ per cent. increase and 12,000 lbs./sq. ins., and in comparing this with Fig. No. 4 we note that a maximum in density (lbs./cubic ft.) also occurs at about this point of consolidation.

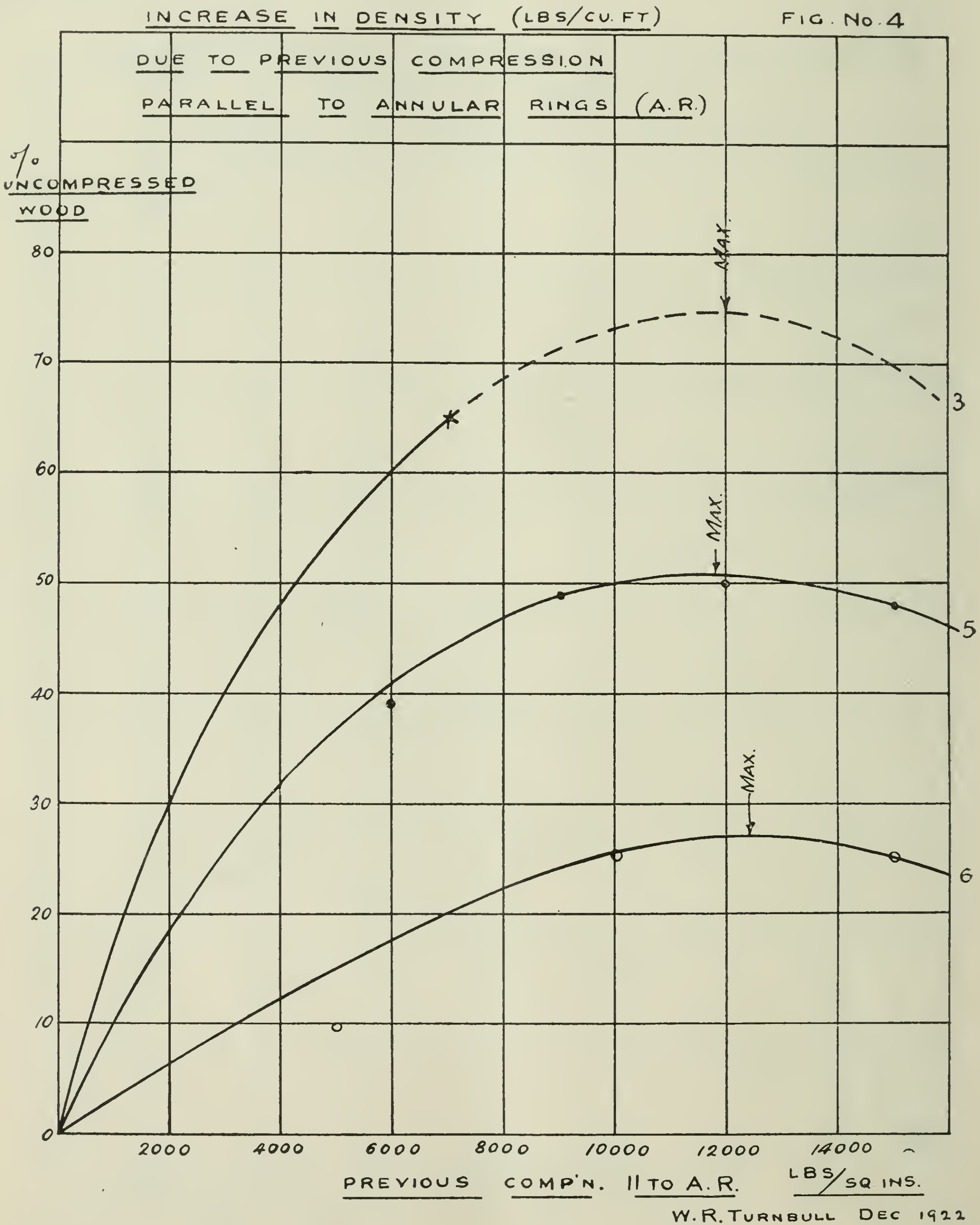
In fact, it seems probable that no useful purpose will be served by compressing the (deciduous?) woods beyond this consolidating load of 6 tons per square inch.

Referring to Fig. 3, we note that while there is a very material increase in



the breaking load, it is not so marked as in the cases for elastic limit; again, a maximum seems indicated at about 12,000 (10,000 for birch, 14,000 for hickory, both broken in bending tests).

The data on density increase (Fig. No. 4) is not so complete as would be desirable, but maxima are indicated, in the three cases given, in the vicinity of a consolidation pressure of 12,000 lbs./sq. in.; comparing these Curves 3, 5 and 6 with those of Fig. No. 2 (and corresponding numbers), we see that density increase is not so great as strength increase.



For Honduras mahogany it is apparently about the same, but for the other two we have :—

Birch maximum increase in density	= 51%.	Do. elastic limit = 62 +.
Hickory ,, ,, ,,	= 27%.	,, ,, ,, = 77·5%.

Note on "Recovery."—In some samples of Honduras mahogany that had been consolidated (7,000 lbs./sq. in.) over *four years* previously, the recovery (towards original thickness) was only from 6 to 7½ per cent.

Note on "Effects of Heat."—Samples of Honduras mahogany that had been consolidated (7,000 lbs./sq. in.) were heated, after six months, to a temperature of 200 deg. F. for 1½ hours, and shrank about 2½ per cent.; they then stood for three years and had recovered 1½ per cent.; they were again heated (120deg. F. for 21 hours) and shrank about 1 per cent., so that the dimension changes, due to extreme temperatures (for wood), are of the order of 1 to 2 per cent. only.

It is obvious, from the general results here given, that we have to deal with a rather remarkable phenomenon when we compress woods across the grain and parallel to the annular rings, and it is a phenomenon that should have some very useful applications in the arts and industries.

Such a common wood as birch can be given the strength and density of such a rare wood as English boxwood by consolidating it with a load of 6 tons per square inch, and many uses should be found (where wearing qualities, strength and resistance to shrinkage by heat are of importance) for such an artificial wood, or similar ones, in many industries.

Consolidated wood should be of great use for aeroplane parts and air propeller parts, for farm implement parts, spokes of wheels, shuttles for mill work, etc.; in short, wherever a strong, dense wood is required for special purposes.

I regret placing before the Royal Aeronautical Society a paper that is somewhat "sketchy," but the stress of other work has prevented me going into this matter more fully, and the results here presented represent a good deal more work than is apparent on the surface.

It is to be hoped that some of our younger engineers who have a testing machine, and more time, available will be able to take up this interesting research and give us much more extended results on the subject.

TWO-DIMENSIONAL AEROFOIL THEORY

BY MURIEL GLAUERT, B.SC.

1 *Introduction*

The object of the present paper is to give some account of certain methods developed by Joukowsky, Mises and others for calculating the forces experienced by a body of aerofoil shape placed in a stream moving in a fixed direction with uniform speed. The span of the aerofoil is supposed infinite and the cross section uniform and the motion thus becomes two-dimensional.

The classical methods of calculating the forces on a body placed in an inviscid fluid moving with uniform velocity give no resultant force on the body. But if on this motion we super-impose a circulation round the body we obtain a resultant force at right angles to the direction of motion, *i.e.*, a lift, but no drag. The lift will depend on the magnitude of the circulation imposed. It is, however, difficult to see how such a circulation could be set up in an inviscid fluid.

Now although the viscosity in a fluid, such as air or water, is small, it appears that its effect on the motion is appreciable. Let us therefore examine the various ways in which viscosity might be expected to affect the motion.

The force of viscosity is proportional to the velocity gradient perpendicular to the stream at any point, and hence it may be expected to affect the flow appreciably only in the regions where the velocity is changing rapidly. This will be the case near the surface of the body, since at the surface the velocity is zero, and also in the region of the trailing edge where, owing to the rapid change of curvature, the velocity tends to become very great.

The effect of viscosity in the layer of fluid close to the surface of the aerofoil will be to produce a retarding force or drag. We here have an indication of the reason why in the non-viscous solution the drag force does not appear. Neglecting viscosity in this region may be considered equivalent to neglecting the drag of the aerofoil. As, however, the characteristic property of an aerofoil is its large lift compared with its drag, a solution assuming slip at the surface of the body might be expected to give a reasonable approximation to the motion.

There remains the high velocity in the region of the trailing edge. Joukowsky conceived the idea of a circulation round the body set up by the viscous forces to counteract excessive velocity at the trailing edge. The magnitude of this circulation is in general indeterminate, but Joukowsky, in his theory, uses aerofoils with a sharp trailing edge and it is then possible to find a value for the circulation such that the flow at this point is finite.

Thus we are led to the consideration of the flow round an aerofoil placed in an inviscid fluid moving with uniform velocity and having a circulation imposed on it.

Now the equations of motion for a circle placed in such a stream are known and by means of a fundamental theorem by Bieberbach we can use these equations to obtain the flow past a body of aerofoil shape.

2 *General Theory*

Bieberbach's theorem is as follows: Given a closed curve without double points, then there is one and only one transformation of the type

$$\zeta = z + b_1/z + b_2/z^2 + \dots$$

where $\zeta = \xi + i\eta$, $z = x + iy$, and b_1, b_2, . . . are in general complex quantities of the type $a + ib$, such that the space external to the curve in the z plane is

transformed into the space external to a circle in the ζ plane in such a manner that the region at infinity remains unchanged. The transformation is analytic in the region external to the curve or circle, *i.e.*, there is a one to one correspondence between points in the z plane external to the curve and points in the ζ plane external to the circle. The position and magnitude of the circle will be determined by the values of b_1, b_2. . . .

Conversely there is a conformal transformation of the type

$$z = \zeta + a_1/\zeta + a_2/\zeta^2 + \ldots \qquad (1)$$

whereby a circle in the ζ plane is transformed into a contour similar to that of any given contour without double points.

Assuming the right hand side of (1) to contain $(n + 1)$ terms we have

$$z = \zeta + a_1/\zeta + a_2/\zeta^2 + \ldots a_n/\zeta^n \qquad (2)$$

Then

$$dz/d\zeta = 1 - a_1/\zeta^2 - 2a_2/\zeta^3 - \ldots - na_n/\zeta^{n+1} \qquad (3)$$
$$= (1 - v_1/\zeta)(1 - v_2/\zeta) \ldots (1 - v_{n+1}/\zeta^{n+1})$$

where $\Sigma v_1 = 0$, $\Sigma v_1 v_2 = a_1$, etc., and $v_1, v_2, \ldots v_{n+1}$ are the zeros of $dz/d\zeta$.

The transformation (1) is conformal, *i.e.*, elementary arcs retain their shape on transformation, their lengths being increased by a factor $|dz/d\zeta|$, while each element is turned through an angle arg $dz/d\zeta$.* This process, however, breaks down at points for which $dz/d\zeta$ is zero or infinite, and by taking one of the zeros of $dz/d\zeta$ on the circumference of the circle it is possible to obtain from the smooth contour of the circle a contour with a sharp edge at one point. As this may be taken as a first approximation to an aerofoil shape having the sharp point for its trailing edge, we consider a transformation of the type (1) applied to a circle having one of the zeros of $dz/d\zeta$ on the circumference and enclosing the remaining zeros. From the form of (3) we see that the origin has been taken at the C.G. of the zeros of $dz/d\zeta$.

Now consider a circle with centre M and radius a in the ζ plane (Fig. 1),

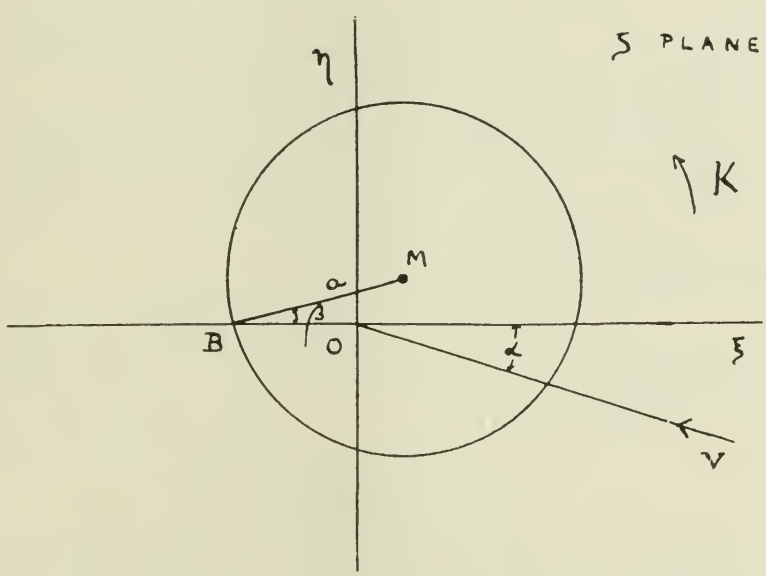


FIG. 1.

and let B be a point on the circumference at which $dz/d\zeta = 0$. Take as origin O of co-ordinates the centroid of the zeros of $dz/d\zeta$ and take BO for ξ axis. We shall suppose the cylinder having this circle for its cross section to be placed in a uniform stream V, the direction of the stream making an angle α with the negative axis of ξ, and there being a circulation round the cylinder equal to K.

* See Appendix I

For the motion let $w = \phi + i\psi$ where ϕ is the velocity potential and ψ the stream function. Then the magnitude of the velocity q at any point in the z plane is given by $|dw/dz|$.

But

$$dw/dz = (dw/d\zeta)/(dz/d\zeta)$$

and at the point B $dz/d\zeta = 0$. Hence, if the velocity at the trailing edge of the aerofoil in the z plane is to remain finite $dw/d\zeta$ must be equal to zero at B, *i.e.*, B is a point on the circumference of the circle at which the velocity vanishes.

To find this stagnation point consider the circle referred to axes $M\xi_1$, $M\eta_1$, through the centre, the direction of motion being that of the negative axis of ξ_1. Then from symmetry there will be two stagnation points A and B say, such that MA, MB make angles ϵ with the ξ_1 axis as shown in Fig. 2.

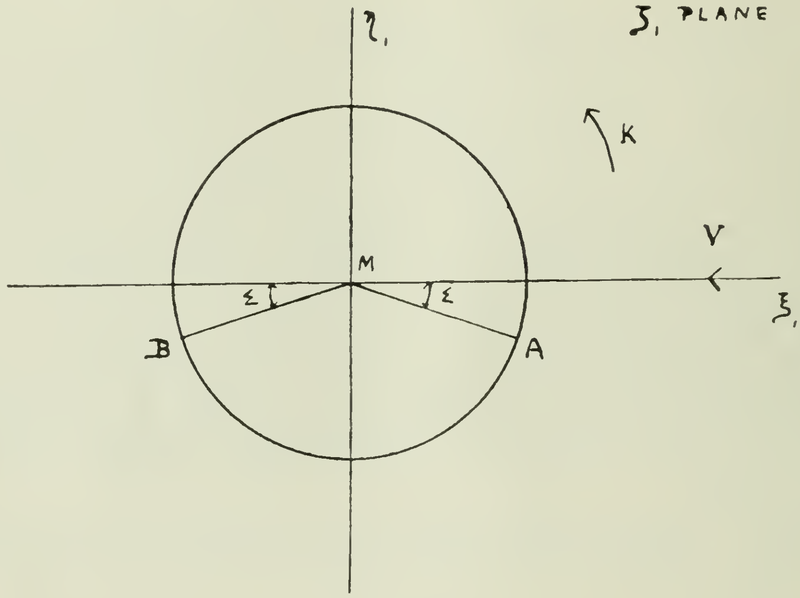


FIG. 2.

Then the motion of the fluid is given by*

$$w = \phi + i\psi = V\{\zeta_1 + a^2/\zeta_1\} + (iK/2\pi)\log\zeta_1 \quad . \quad . \quad (4)$$

where

$$\zeta_1 = \xi_1 + i\eta_1 = re^{i\theta}$$

Equating the real parts of (4) we have

$$\phi = V(r + a^2/r)\cos\theta - K\theta/2\pi$$

and if v be the component of velocity perpendicular to the radius vector at any point ζ_1, $v = -(1/r)\,\delta\phi/\delta\theta$.

At A and B $r = a$ and $v = 0$ whence

$$K = -4\pi aV\sin\theta$$
$$= 4\pi aV\sin\epsilon$$

Returning to Fig. 1 we have, if B is to be a stagnation point, $\epsilon = \alpha + \beta$ giving

$$K = 4\pi aV\sin(\alpha + \beta) \quad . \quad . \quad . \quad . \quad (5)$$

In order therefore that the velocity at the trailing edge should be finite, the circulation which must be imposed on the uniform stream V is determined by equation (5) and is seen to depend on α, the angle which the undisturbed stream makes with OB.

* See Lamb's "Hydrodynamics," Sections 68 and 69.

Lamb (*loc. cit.*) gives the following formula for the resultant pressure on the circular cylinder per unit length

$$L_1 = \rho V K$$

where ρ is the density, and this pressure is at right angles to the direction of the undisturbed stream.

Now since the motion is irrotational the circulation round any closed path enclosing the cylinder is constant. But the transformation (1) leaves the region at infinity unchanged and hence the circulation round the contour in the z plane is also K; the resultant pressure, or lift, is therefore given by

$$L = \rho V K \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$= 4\pi a \rho V^2 \sin(\alpha + \beta) \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

If M_1 is the moment of this lift about the centre M of the circle of transformation it is shown in Appendix I. that

$$M_1 = 2\pi c_1^2 \rho V^2 \sin 2(\alpha + \gamma) \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where $a_1 = c_1^2 e^{2i\gamma}$

Thus equations (7) and (8) give the resultant force on the aerofoil and its moment about M.

Putting $\alpha + \beta = \alpha_1$ we have for (7)

$$L = 4\pi a \rho V^2 \sin \alpha_1$$

where α_1 is the angle of incidence measured from the no lift axis, *i.e.*, BM (Fig. 3).

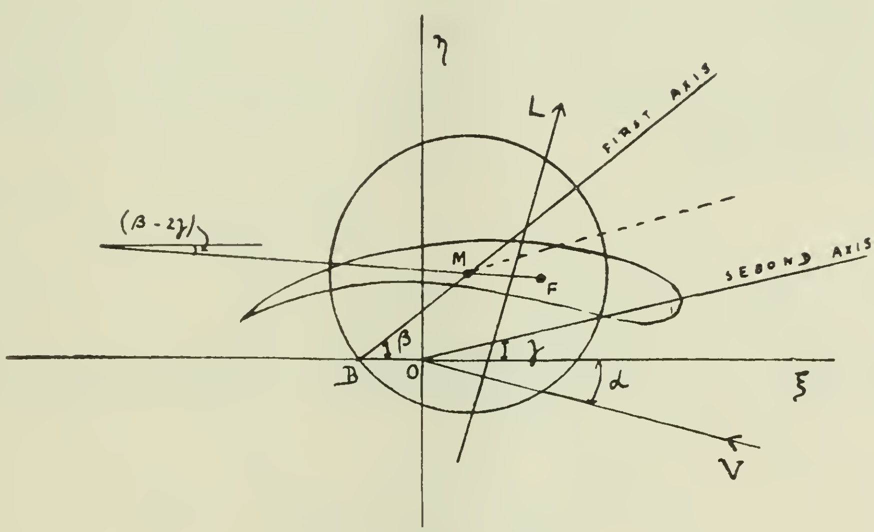


FIG. 3.

We shall call BM the first axis.

Again putting $\alpha + \gamma = \alpha_2$ we have for (8)

$$M_1 = 2\pi c_1^2 \rho V^2 \sin 2\alpha_2$$

and the moment of the lift about M vanishes when $\alpha_2 = 0$, *i.e.*, when $\alpha = -\gamma$.

We shall call a line through O making an angle γ with BO the second axis.

We notice

(1) That the lift depends only on the radius a of the circle and the position of the first axis.

(2) That the moment about M depends only on the constant c_1 and the direction of the second axis.

Two more interesting properties can be established.

(3) There is a point F about which the moment of the lift is independent of α. Vectorially

$$MF = (c_1^2/a)\, e^{i\,(2\gamma - \beta)} = (a_1/a)\, e^{-i\beta}$$

and the moment of L about this point $= -2\pi c_1^2 \rho V^2 \sin 2\,(\beta - \gamma)$.

(4) The lines of action of the lift as α varies envelop a parabola having BM as directrix and F as focus.

Proof of (3) and (4).

From M (Fig. 3) mark off a distance $MF = c_1^2/a$ in a direction making an angle $2\,(\beta - \gamma)$ with the first axis, the angle being measured in a clockwise direction. Then the second axis bisects the angle between MF and the first axis. (In the figure β is taken greater than 2γ.)

Let M be the moment of L about F. Then

$$\begin{aligned} M &= M_1 - L\,(c_1^2/a)\cos(\alpha - \beta + 2\gamma) \\ &= 2\pi c_1^2 \rho V^2 \{ \sin 2\,(\alpha + \gamma) - 2\sin(\alpha + \beta)\cos(\alpha - \beta + 2\gamma) \} \\ &= -2\pi c_1^2 \rho V^2 \sin 2\,(\beta - \gamma) \qquad . \quad . \quad . \quad . \quad . \qquad (9) \end{aligned}$$

i.e., M is independent of α.

Again, if $2h_0$ is the perpendicular from F on the first axis

$$h_0 = (c_1^2/2a)\sin 2\,(\beta - \gamma) \qquad . \quad . \quad . \quad . \qquad (10)$$

and if h is the perpendicular from F on the line of action of the lift

$$\begin{aligned} h = M/L &= -(c_1^2/2a)\sin 2\,(\beta - \gamma)/\sin(\alpha + \beta) \\ &= -h_0/\sin(\alpha + \beta) \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (11) \end{aligned}$$

From the form of h we see that the foot of the perpendicular from F on the line of action of L lies on a line parallel to the first axis at a distance h_0 away from it, *i.e.*, lies on the tangent at the vertex of a parabola having F as focus and first axis as directrix (Fig. 4). It follows that the line of action of L is a tangent to this parabola.

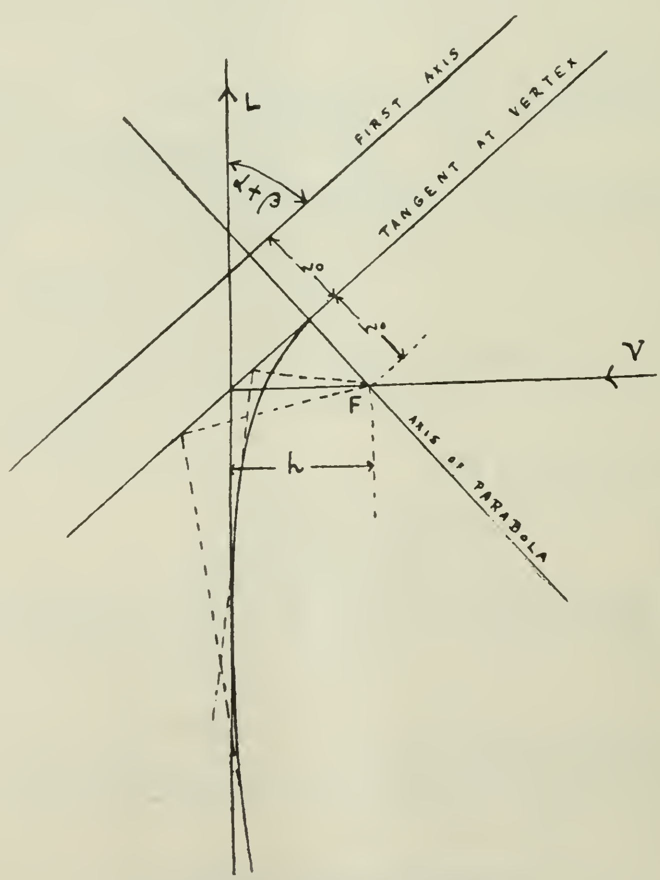


FIG. 4.—*Lift Parabola.*

An interesting case arises when $h_0 = 0$, *i.e.*, when $\beta = \gamma$ and the first and second axes are parallel. The lines of action of the lift now all pass through F and the aerofoil has a fixed centre of pressure.

From the above analysis we see that, given the transformation which will transform the circle into the given aerofoil, the characteristics of the aerofoil can then be found. The problem, however, of finding the necessary transformation is one of great difficulty and the method employed by Joukowsky and Mises has been to search for functions which would transform a circle into a contour approximating to an aerofoil shape.

3 *The Transformation* $z = \zeta + c^2/\zeta$.

Let us begin by considering a transformation containing the first two terms only of the series (1). We then have

$$z = \zeta + a_1/\zeta \qquad . \quad . \quad . \quad . \quad . \qquad (12)$$

and

$$dz/d\zeta = 1 - a_1/\zeta^2$$

The zeros of $dz/d\zeta$ are at the points $\pm\sqrt{a_1}$ and the origin is at the centroid of these zeros. If now we choose the ξ axis to pass through the two zeros, a_1 will be real, and writing $a_1 = c^2$, the zeros of $dz/d\zeta$ are the points B, A, given by $\zeta = -c$ and $\zeta = +c$. According to the theory developed above one of the zeros, $\zeta = -c$ say, must lie on the boundary of the circle of transformation. Then B is the point on the circle which will transform into B', the trailing edge of the aerofoil (Fig. 5).

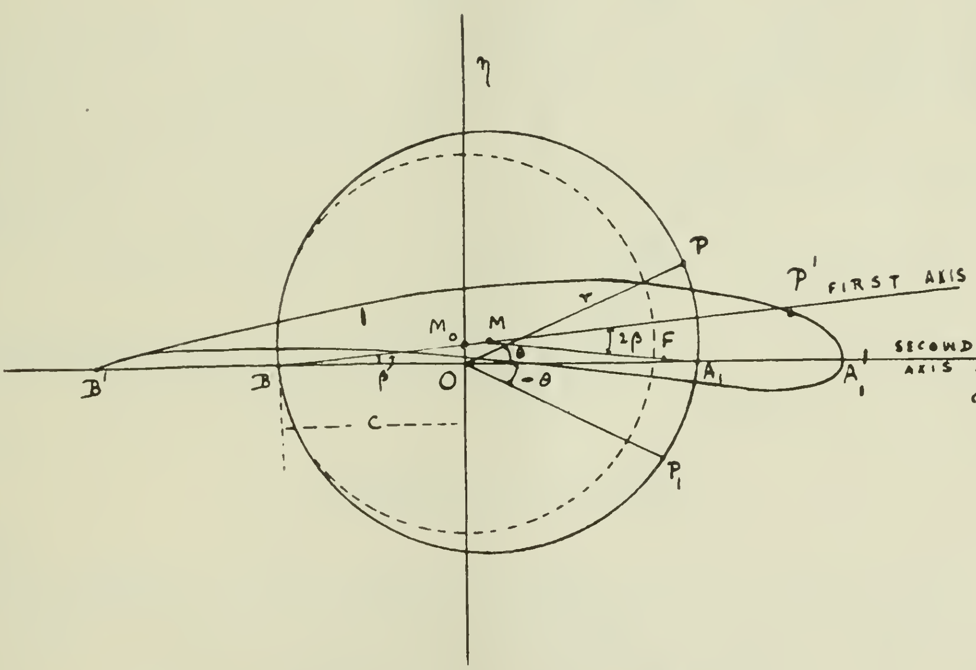


FIG. 5.—*Joukówsky Aerofoil.*

The other zero $\zeta = +c$ must lie within the circle. Let the ξ axis cut the circle again in A_1. As before M is the centre and a the radius of the circle and angle $MBO = \beta$. Then BM is the first axis.

Now $a_1 = c_1{}^2 e^{i\gamma} = c^2$ for the present case. Hence $c_1 = c$ and $\gamma = 0$ and the axis of ξ becomes the second axis. The line joining B and M will always cut the η axis in M_0 say, and the thickness and camber of the aerofoils obtained from the circle by means of the transformation (12) will depend on the quantities a and β. The focus is a point F in MA_1 such that $MF = c^2/a$.

The type of aerofoil obtained by the transformation (12) was first studied by Joukowsky and the aerofoils are known as Joukowsky aerofoils. Before proceeding to discuss the properties of these aerofoils and the method of constructing them we shall first consider the case when M lies on the η axis.

4 *The Circular Arc*

The contour obtained when M lies on the η axis does not belong to the class of aerofoils under consideration for now we have two zeros of $dz/d\zeta$ lying on the boundary of the circle, *i.e.* at A and B, instead of one (Fig. 6). Hence at the

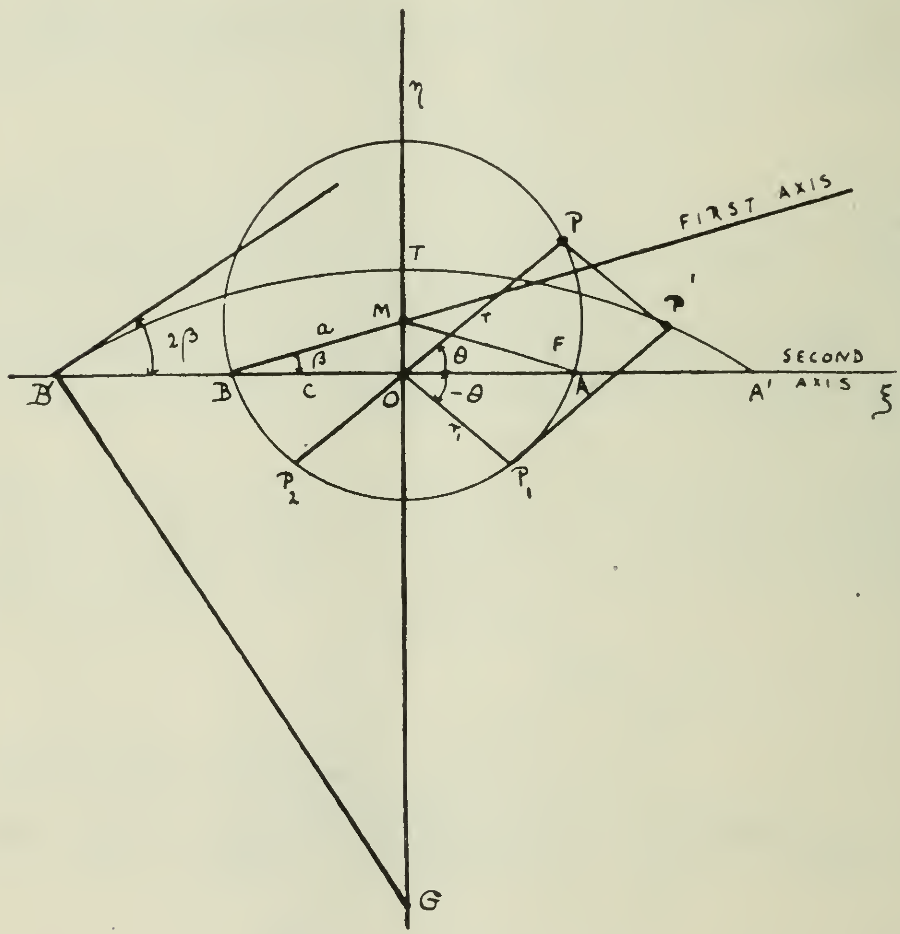


Fig. 6.—*Circular Arc.*

point A' on the contour corresponding to A on the circle the velocity will become infinite. As, however, the most suitable aerofoil shapes are obtained when M_0M and β are small, the limiting case when $M_0M = 0$ is of interest. The circle now transforms into a circular arc. For if $\zeta = re^{i\theta}$ be the point P on the circle, the corresponding point P' on the contour in the z plane is given by

$$z = \zeta + c^2/\zeta \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$x + iy = re^{i\theta} + (c^2/r)\,e^{-i\theta}$$

whence equating real and imaginary parts

$$x = \cos\theta\,(r + c^2/r)$$

$$y = \sin\theta\,(r - c^2/r)$$

Eliminating r we have

$$x^2\sin^2\theta - y^2\cos^2\theta = 4c^2\sin^2\theta\cos^2\theta \quad . \quad . \quad . \quad (14)$$

Since $PM = a = c \sec\beta$, and $OM = c\tan\beta$ we have from the triangle POM

$$c^2 \sec^2\beta = r^2 + c^2\tan^2\beta - 2rc\tan\beta\sin\theta$$

i.e.,

$$(r^2 - c^2)/2rc = \tan\beta\sin\theta$$

But

$$y = \sin\theta\,(r^2 - c^2)/r$$
$$= 2c\tan\beta\sin^2\theta$$

Substituting for θ in (14) we obtain

$$x^2 + \{\, y + 2c\cot 2\beta \,\}^2 = (2c \operatorname{cosec} 2\beta)^2$$

showing that the locus of P′ is an arc of a circle of radius $2c \operatorname{cosec} 2\beta$ and centre the point $(0, -2c\cot 2\beta)$. The point B will transform into the point B', $z = -2c$, and the point A into the point A', $z = +2c$.

It is easily seen that the tangent to the arc at B' makes an angle 2β with the x axis.

The chord $B'A' = 4c$, while if the arc $B'A'$ cuts the η axis in T, then $OT = 2OM$, giving

$$\text{camber} = 2OM/4c = \tfrac{1}{2}c\tan\beta.$$

From the fact that equation (14) remains unchanged when $-\theta$ is substituted for θ it follows that as P describes the circle, P' describes the arc $B'TA'$ twice. The above results are illustrated geometrically in Appendix II.

5 *Joukowsky Aerofoils*

We return to the case when M does not lie on the η axis. Let $MM_0 = mc$ (Fig. 5) and assume m and β small. Then if z be a point P' on the aerofoil corresponding to a point P, $\zeta = re^{i\theta}$ on the circle we can find approximate expressions for the co-ordinates of P'.

Then to the first order $a = c\,(1 + m)$ and the co-ordinates of centre M are $\xi = mc$, $\eta = \beta c$.

Since P is on the circle centre M and radius a we have

$$(r\sin\theta - mc\sin\beta - c\tan\beta)^2 + (r\cos\theta - mc\cos\beta)^2$$
$$= (c\sec\beta + mc)^2$$

Retaining only terms of the first order we have

$$r^2 - 2rc\,(\beta\sin\theta + m\cos\theta) - c^2\,(1 + 2m) = 0$$

Then

$$r/c = 1 + m\,(1 + \cos\theta) + \beta\sin\theta$$
$$c/r = 1 - m\,(1 + \cos\theta) - \beta\sin\theta$$

Now

$$z = \zeta + c^2/\zeta$$
$$x = (r + c^2/r)\cos\theta$$
$$y = (r - c^2/r)\sin\theta$$

whence

$$x/c = 2\cos\theta$$
$$y/c = 2\sin\theta\,\{\, m\,(1 + \cos\theta) + \beta\sin\theta \,\}$$

When $\theta = 0$, $x = 2c$, and when $\theta = \pi$, $x = -2c$, hence the chord $A'B'$ (Fig. 5) of the aerofoil is $4c$ approximately.

If P and P_1 are points on the circle such that OP and OP_1 make angles θ and $-\theta$ with $O\xi$ and y and y_1 are the ordinates of the corresponding points on the aerofoil we have, if t is the thickness of the aerofoil

$$t = y - y_1$$
$$= 4mc\sin\theta\,(1 + \cos\theta)$$

This will be a maximum when $dt/d\theta = 0$, giving $\cos\theta = \frac{1}{2}$, *i.e.*, thickness is a maximum when $x = c$, *i.e.*, at a distance from leading edge equal to about $\frac{1}{4}$ chord. At this point $t = 3\sqrt{3}mc$.

Again

$$\tfrac{1}{2}(y + y_1) = 2c\beta \sin^2\theta$$

which is a maximum when $\theta = \pi/2$ and hence mean camber $= 2\beta c/4c = \beta/2$.

Again, since the circle centre M would touch a circle centre M_0 and radius BM_0 (the dotted circle in Fig. 5) at B, it follows that the aerofoil and circular arc into which these two circles are transformed by means of (13) will have the same tangent at B'. Hence the Joukowsky aerofoil has a cusp at the trailing edge, the cuspidal tangent making an angle 2β with the chord.

In Appendix II. a method of constructing a Joukowsky aerofoil is given and some geometrical properties of the aerofoil are proved. Also in Fig. 16 the flow round the aerofoil is illustrated.

Finally

$$k_L = L/b\rho V^2 = (4a/b)\,\pi \sin(\alpha + \beta)$$

where b is the chord of the aerofoil.

Since $a = (1 + m)\,c$, and $b = 4c$ approximately for Joukowsky aerofoils

$$k_L = (1 + m)\,\pi \sin(\alpha + \beta)$$

For wing sections m lies between 0.1 and 0.05, hence to a first approximation

$$dk_L/d\alpha = \pi$$

i.e., the slope of the lift curve is π per radian, or 0.055 per degree for aerofoils of infinite aspect ratio. This value corresponds to a slope of 0.041 per degree for aerofoils of aspect ratio 6.

The slopes obtained from wind channel tests on models of some typical wing sections of aspect ratio 6 are given below. The average slope is 0.041, thus giving good agreement with the above theory.

R. & M.	WING.	SLOPE.	R. & M.	WING.	SLOPE.
323	R.A.F. 14	0.0413	377	Bristol F2B	0.0412
774	R.A.F. 15	0.0391	375	Albatross	0.0408
415	R.A.F. 18	0.0406	481	Curtiss	0.0425

Strut Shape.—In the case when M lies on the ξ axis $\beta = \gamma = 0$ and the first and second axes coincide. Then the circle transforms into a symmetrical strut-shaped section (Fig. 7) having a fixed centre of pressure, *i.e.*, the lines of action of the lift all pass through a point F in BM such that $MF = c^2/a$.

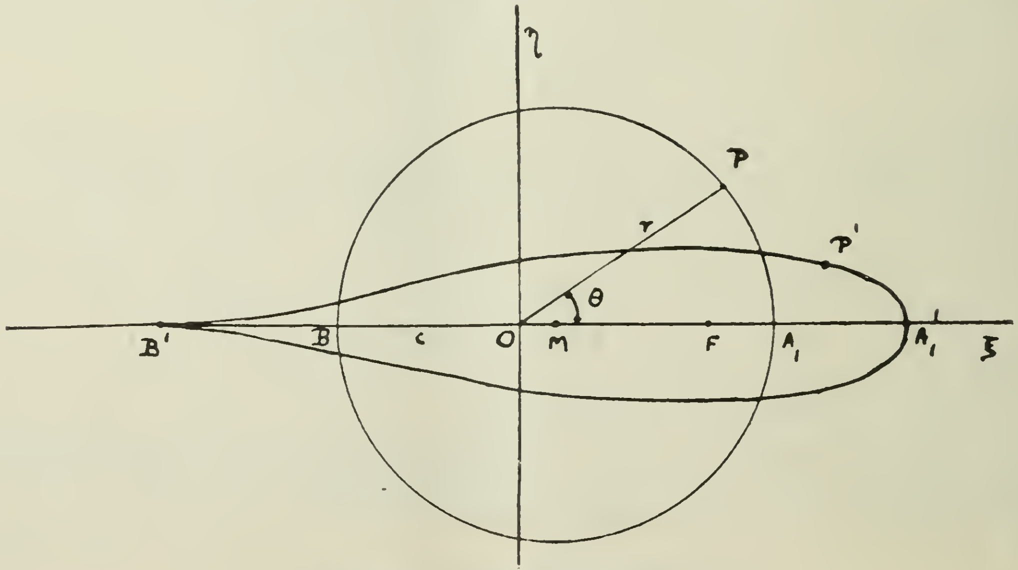


FIG. 7.—*Joukowsky Strut.*

The co-ordinates of a point P' on the strut corresponding to a point P, $\zeta = re^{i\theta}$ on the circle are now given by

$$x/c = 2\cos\theta,\ \ y/c = 2m\sin\theta\,(1 + \cos\theta)$$

The maximum thickness is $3\sqrt{3}mc$ and occurs when $x = c$.

6 *Summary of Results for Joukowsky Aerofoils*

Given a circle

$$|\zeta - \zeta_0| = a$$

in the ζ plane where the radius a and the centre M, $\zeta = \zeta_0$, are determined by means of the quantities $BO = c$, $MBO = \beta$, $MM_0 = cm$ (Fig. 5), then by means of the transformation

$$z = \zeta + c^2/\zeta$$

the circle is transformed into an aerofoil having a cusp at the trailing edge and a chord of length $4c$ approximately. The cuspidal tangent makes an angle 2β with the chord.

The lift is given by

$$L = 4\pi a\rho V^2 \sin(\alpha + \beta)$$

and the moment of the lift about the centre M by

$$M_1 = 2\pi c^2 \rho V^2 \sin 2\alpha$$

The slope of the lift curve is approximately π per radian.

If F is the point such that vectorially $MF = (c^2/a)\,e^{-i\beta}$ the lines of action of the lift all touch a parabola whose focus is F and directrix BM.

The moment of the lift about F is given by

$$M = -2\pi c^2 \rho V^2 \sin 2\beta$$

and is seen to be independent of α. Further, the perpendicular h from F on the line of action of the lift is given by

$$h = (c^2 \sin 2\beta)/2a \sin(\alpha + \beta)$$

The mean camber is $\beta/2$ approximately, hence when $\beta = 0$, we obtain a strut-shaped section, and since in this case $h = 0$, the strut will have a fixed centre of pressure.

The maximum thickness is $3\sqrt{3}\,mc$ approximately. Then as $m \to 0$, the thickness $\to 0$ and in the limit we have a circular arc of chord $4c$ and height $2c\tan\beta$.

To a first approximation $a = (1 + m)\,c$. Thus from the form of h it appears that the movement of the centre of pressure increases with increase of camber and decreases with increase of thickness.

7 *The Transformation* $(z - nc)/(z + nc) = (\zeta - c)^n/(\zeta + c)^n$.

The transformation $z = \zeta + c^2/\zeta$ may be written

$$(z - 2c)/(z + 2c) = (\zeta - c)^2/(\zeta + c)^2 \quad . \quad . \quad . \quad (15)$$

This is a more particular case of the more general transformation

$$(z - nc)/(z + nc) = (\zeta - c)^n/(\zeta + c)^n \quad . \quad . \quad . \quad (16)$$

Then the points $\pm c$ in the ζ plane transform into the points $\pm nc$ in the z plane.

We may write for equation (16)

$$z - nc = (\zeta - c)^n \phi_1(\zeta) \quad . \quad . \quad . \quad . \quad (16a)$$

where $\phi_1(\zeta)$ and $d\phi_1/d\zeta$ are finite, continuous and different from zero in the neighbourhood of $\zeta = c$.

Similarly we may write equation (16) in the form

$$z + nc = (\zeta + c)^n \phi_2 (\zeta) \quad . \quad . \quad . \quad . \quad (16b)$$

where $\phi_2 (\zeta)$ and $d\phi_2/d\zeta$ are finite, continuous and different from zero in the neighbourhood of $\zeta = -c$. Then it follows that $dz/d\zeta$ has zeros of order $(n - 1)$ at the points $\zeta = \pm c$.

Returning to equation (16)

$$(z - nc)/(z + nc) = (1 - c/\zeta)^n/(1 + c/\zeta)^n$$

Expanding in descending powers of $1/\zeta$ we have

$$z = \zeta + \{ (n^2 - 1)/3 \} (c^2/\zeta) + \dots \quad . \quad . \quad . \quad . \quad (16c)$$

Thus in this case $c_1 = (n^2 - 1) c^2/3$, and $\gamma = 0$.

The transformation (16) is applied as before to a circle with centre M and radius a, the point $\zeta = -c$ being a point B on the circumference and the point $\zeta = c$ lying within the circle (Fig. 8). $MBO = \beta$, and BM cuts the

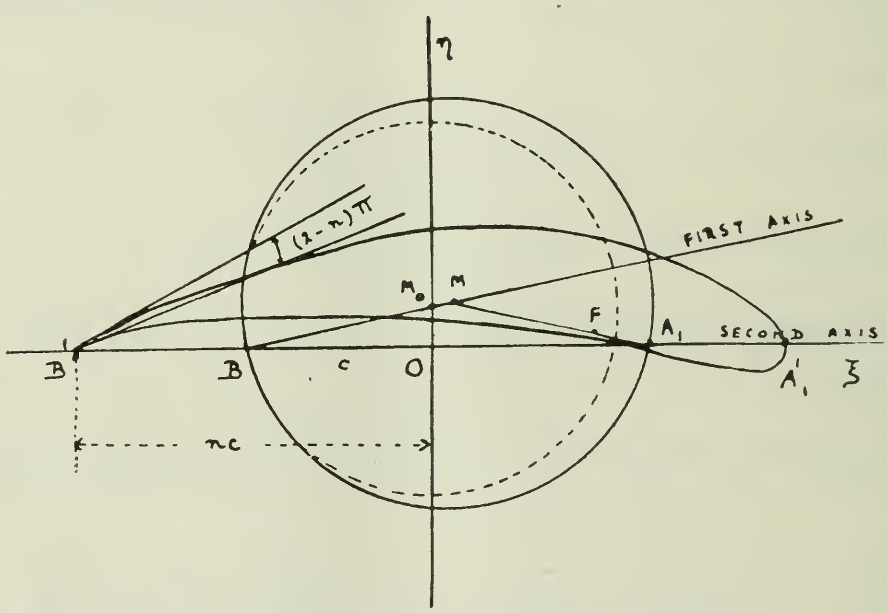


FIG. 8.—*Generalised Joukowsky Aerofoil.*

η axis in M_0. Then BM is the first axis, BO the second axis and the point B transforms into the point B', $z = -nc$. Applying the general theory we have

$$L = 4\pi a\rho V^2 \sin (\alpha + \beta)$$

The moment of L about centre $M = \frac{2}{3}c^2 (n^2 - 1) \rho V^2 \sin 2\alpha$. The focus F is a point in MA_1 such that $MF = (n^2 - 1)c^2/3a$. The moment of L about $F = -\frac{2}{3}\pi c^2 (n^2 - 1) V^2 \sin 2\beta$.

Again the most suitable types of aerofoils are obtained by taking MM_0 and β small.

As before we first consider the limiting case when $MM_0 = 0$, although the contours so obtained will not belong to the class of aerofoils under consideration as now there are zeros of $dz/d\zeta$ at two points on the circumference of the circle, *i.e.*, at $\zeta = \pm c$. The equations of motion would then give infinite velocity at the point on the contour corresponding to the point $\zeta = + c$ on the circle.

8 *The Double Circular Arc*

When M lies on the η axis the circle passes through the points $\zeta = \pm c$ and these points transform into the points $z = \pm nc$ (Fig. 9).

The transformation (16) may be written vectorially as

$$P'A'/P'B' = (PA/PB)^n \quad . \quad . \quad . \quad . \quad (17)$$

Denoting PA and PB by r_1 and r_2 and the angles they make with the ξ axis by θ_1 and θ_2 and adopting a similar notation with dashed letters for corresponding lines and angles in the z plane, we may write for equation (17)

$$r'_1 e^{i\theta'_1} / r'_2 e^{i\theta'_2} = \{ r_1 e^{i\theta_1} / r_2 e^{i\theta_2} \}^n$$

i.e.,

$$(r'_1/r'_2)\, e^{i(\theta'_1 - \theta'_2)} = (r_1/r_2)^n\, e^{in(\theta_1 - \theta_2)}$$

whence

$$\theta'_1 - \theta'_2 = n(\theta_1 - \theta_2)$$

i.e.,

$$A'P'B' = nAPB = n\psi \text{ say.}$$

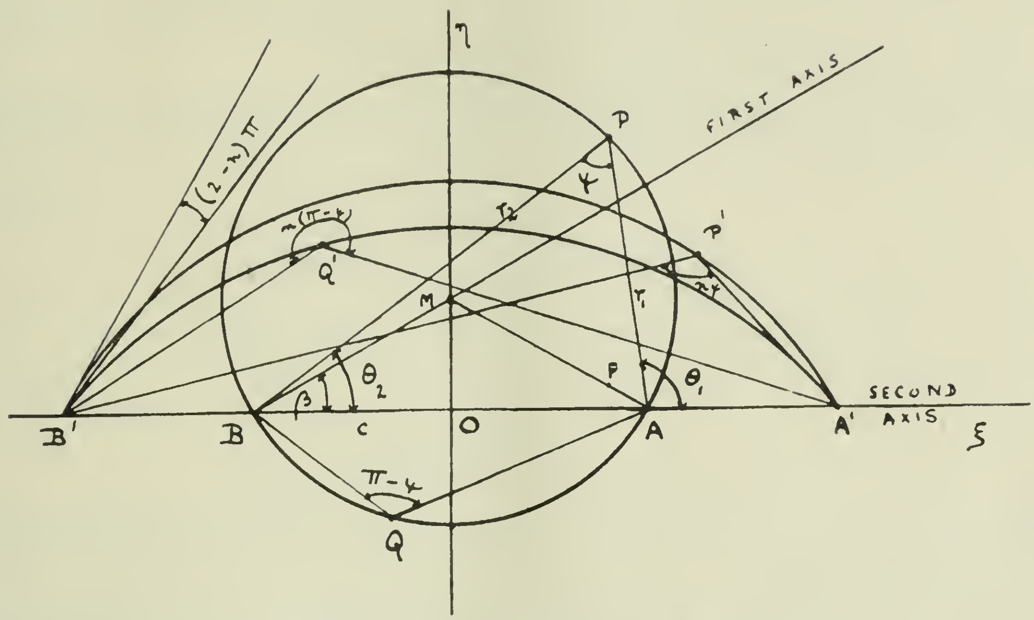


FIG. 9.—*Double Circular Arc.*

Now as P moves round the major arc APB of the circle ψ is constant, therefore as P' moves from A' to B' the angle $A'P'B'$ remains constant, *i.e.*, P' describes a circular arc.

Again, if Q is a point on the minor arc of the circle and Q' the corresponding point in the z plane, then reflex angle

$$A'Q'B' = nAQB = n(\pi - \psi)$$

$$\therefore\ A'Q'B' = 2\pi - (n\pi - \psi) = (2 - n)\pi + n\psi$$

But as Q describes the arc BQA, angle BQA remains constant and therefore as Q' moves from B' to A' angle $B'Q'A'$ remains constant, *i.e.*, Q' describes another circular arc.

Now it can easily be shown that two arcs standing on a common chord subtending angles α and β at the circumferences cut at an angle $\beta - \alpha$. Hence the arcs $A'P'B'$, $A'Q'B'$ cut at an angle τ where

$$\tau = (2 - n)\pi + n\psi - n\psi = (2 - n)\pi$$

For a small angle at the trailing edge we must take n slightly less than 2.

The circle will transform into a sickel providing

$$nAQB > \pi$$

i.e.,

$$n(\pi - \psi) > \pi$$

But $\psi = \pi/2 - \beta$, therefore we must have $\beta > \{ 1/n - 1/2 \} \pi$.

·9 The Generalised Joukowsky Aerofoil

To return to the case when the point $\zeta = c$ lies within the circle. Then the aerofoils obtained by means of the transformation (16) will conform to the general theory and the flow round them will be streamline.

Since in practice n is taken slightly less than 2 the new series of aerofoils are of the same type as the Joukowsky aerofoils, but with a small angle at the trailing edge. In Fig. 8 n has been taken equal to 1.95, giving an angle of 9° at the trailing edge.

Since (Fig. 8) the circle with centre M touches the circle with centre M_0 and radius BM_0 at B, it follows that the aerofoil and double circular arc into which these circles are transformed by means of (16) will have the same tangents at B', hence the aerofoil has an angle $(2 - n)\pi$ at the trailing edge.

It can be shown that, assuming MM_0 and β to be small, the length of the chord $A'B'$ is approximately $4nc$.

Strut Shape.—In the case when M lies on the ξ axis, $\beta = \gamma = 0$, and we obtain a strut-shaped section (Fig. 10), having an angle $(2 - n)\pi$ at the trailing edge.

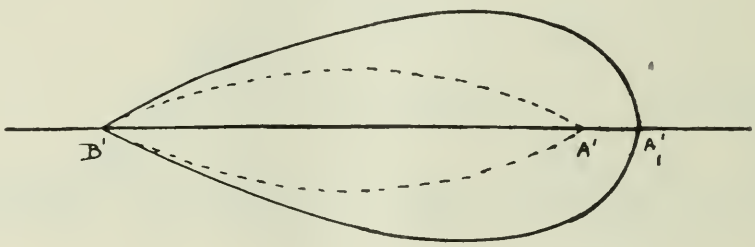


FIG. 10.—*Generalised Joukowsky Strut.*

Since $\beta = \gamma = 0$, the first and second axes are parallel and the strut has a fixed centre of pressure.

It may be noted that in both the Joukowsky type of aerofoils, and in the generalised type, the symmetrical case is the only one which gives a section having a fixed centre of pressure. As it would obviously be of interest to obtain non-symmetrical shapes with a fixed centre of pressure, transformations have been sought for which would produce the required result.

Mises by using three terms of the transformation (2), and by a suitable choice of the point B and the coefficients a_1 and a_2 obtains an aerofoil section (Fig. 11) having a fixed centre of pressure. For more detailed information about this aspect of the subject which provides considerable scope for further investigation the reader is referred to Professor Mises' article in the "Zeitschrift für Flugtechnik und Motorluftschiffahrt," Vol. II. (1920), page 86.

APPENDIX I.

Proof of the Moment Formula

1. *Complex Quantities.*

The complex quantity $z = x + iy$, where $i = \sqrt{-1}$ may be represented in the z plane by the point P whose co-ordinates referred to rectangular axes are (x, y).

If r, θ are the polar co-ordinates of P we have

$$\begin{aligned} z &= x + iy \\ &= r(\cos\theta + i\sin\theta) \\ &= re^{i\theta} \end{aligned}$$

r is always positive and is called the modulus of the complex quantity z and is denoted by $|z|$. θ is called the argument of z and is denoted by $\arg z$.

2. *Stream and Potential Functions.*

If we consider the irrotational motion of an inviscid fluid in two dimensions it can be shown that a potential function $\phi(x, y)$ and a stream function $\psi(x, y)$ exist such that the lines $\phi(x, y) = \text{constant}$ give the lines of equi-velocity-potential, and the lines $\psi(x, y) = \text{constant}$ give the stream lines.

The above is equivalent to the statement that we can find $\phi(x, y)$ and $\psi(x, y)$ such that

$$w = \phi + i\psi = f(x + iy) = f(z)$$

Then ϕ and ψ satisfy the relations

$$\delta^2\phi/\delta x^2 + \delta^2\phi/\delta y^2 = 0, \quad \delta^2\psi/\delta x^2 + \delta^2\psi/\delta y^2 = 0,$$

and if u, v are components of the velocity q at any point in the plane parallel to the co-ordinate axes

$$u = -\delta\phi/\delta x = -\delta\psi/\delta y$$
$$v = -\delta\phi/\delta y = \delta\psi/\delta x$$

The form of ϕ and ψ will depend on the conditions at the boundary of the fluid.

Taking

$$\begin{aligned} w &= \phi + i\psi \\ dw/dz &= \delta w/\delta x = \delta\phi/\delta x + i\delta\psi/\delta x \\ &= -u + iv \\ &= -q(\cos\delta - i\sin\delta) \\ &= -qe^{-i\delta} \end{aligned}$$

where δ is the angle made by the resultant velocity with the x axis.

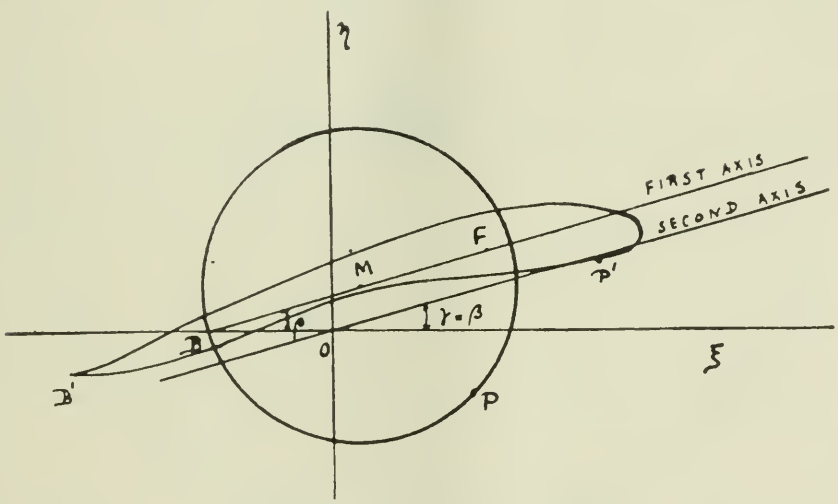


FIG. 11.—*Aerofoil with Fixed Centre of Pressure.*

Moment Formula

Referring to Fig. 2 and equation (4) of the present paper, we find that the motion in the ζ_1 plane is given by

$$w = \phi + i\psi = +V(\zeta_1 + a^2/\zeta_1) + (iK/2\pi)\log\zeta_1$$

If now we transfer the origin to the point o (Fig. 1) and take axes through this point inclined at an angle α to the axes in the ζ_1 plane, we have, taking $\zeta = \zeta_0$ to be the point M in the ζ plane,

$$\zeta_1 = (\zeta - \zeta_0)e^{i\alpha}$$

$$\therefore\ w = V\{(\zeta - \zeta_0)e^{i\alpha} + a^2e^{-i\alpha}/(\zeta - \zeta_0)\} + (iK/2\pi)\{\log(\zeta - \zeta_0) + i\alpha\}$$

$$\therefore\ dw/d\zeta = V\{e^{i\alpha} - a^2e^{-i\alpha}/(\zeta - \zeta_0)^2\} + (iK/2\pi)/(\zeta - \zeta_0)$$
$$= Ve^{i\alpha} + (iK/2\pi)/\zeta + (-Va^2e^{-i\alpha} + iK\zeta_0/2\pi)/\zeta^2 + \ldots$$

Now

$$z = \zeta + a_1/\zeta + a_2/\zeta^2 + \ldots$$

$$dz/d\zeta = 1 - a_1/\zeta^2 - 2a_2/\zeta^3 - \ldots$$

$$\therefore\ dw/dz = (dw/d\zeta)/(dz/d\zeta)$$

$$= (dw/d\zeta) \times (1 + a_1/\zeta^2 + \ldots)$$

$$= Ve^{ia} + (iK/2\pi)/\zeta + (Va_1e^{ia} - Va^2e^{-ia} + iK\zeta_0/2\pi)/\zeta^2 + \ldots$$

$$\therefore\ (dw/dz)^2 = V^2c^{2ia} + (iKVe^{ia}/\pi)/\zeta$$

$$+ (-K^2/4\pi^2 + 2V^2a_1c^{2ia} - 2V^2a^2 + iKV\zeta_0c^{ia}/\pi)/\zeta^2 + \quad . \quad . \qquad \text{(i.)}$$

Now in the z plane, *i.e.*, the plane of the aerofoil, consider a region of fluid bounded by the aerofoil and by a large circle with centre o and radius R (Fig. 12).

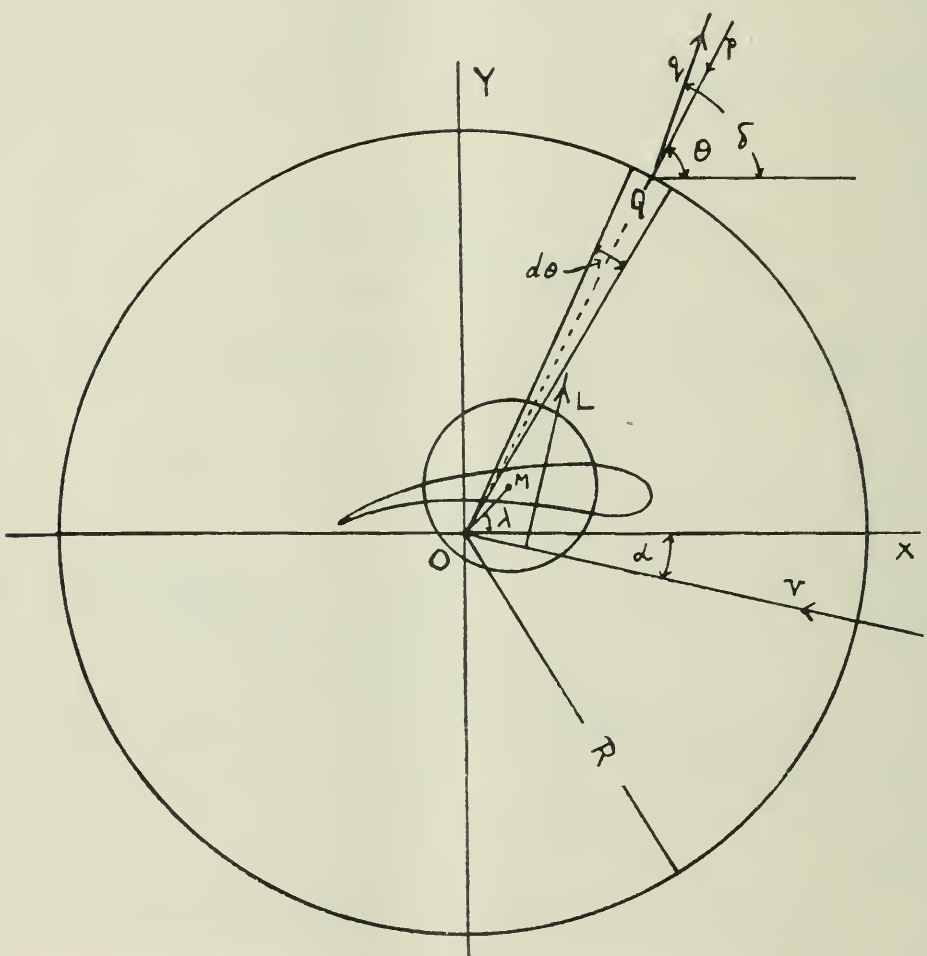


FIG. 12.

Then the excess of the momentum of the fluid leaving this region over that entering it in unit time is equal to the sum of the external forces, namely, the reaction $-L$ of the aerofoil and the pressures on the boundary of the circle. Again the moment of this excess of momentum about any point is equal to the sum of the moments of the external forces about that point. Since the pressure on the boundary of the circle is everywhere normal to it, the moment of the pressure about o vanishes.

Let ds be an element of arc at a point Q on the boundary of the circle and let OQ make an angle θ with Ox. At Q the velocity q makes an angle δ with ox.

Then $\rho q \cos(\delta - \theta)\, ds$ represents the quantity of fluid flowing across the boundary ds in unit time, ρ being the density of the fluid. Since the velocity

perpendicular to OQ is $q \sin(\delta - \theta)$ the moment of momentum of this fluid about o is $\rho R q^2 \cos(\delta - \theta) \sin(\delta - \theta)\, ds$.

Equating the integral of this quantity taken round the circle to the moment of the external forces about o we have, if M is the moment of the lift L about o,

$$- M = \rho \int_0^{2\pi} R^2 q^2 \cos(\delta - \theta) \sin(\delta - \theta)\, d\theta$$

$$= \tfrac{1}{2}\rho R^2 \int_0^{2\pi} (dw/dz)^2\, e^{2i\delta} \sin 2(\delta - \theta)\, d\theta$$

Now $-\sin 2(\delta - \theta) =$ imaginary part of $e^{-2i(\delta - \theta)}$

$$= Ie^{-2i(\delta - \theta)} \text{ say.}$$

$$\therefore\ M = \tfrac{1}{2}\rho R^2 \left\{ I \int_0^{2\pi} (dw/dz)^2\, e^{2i\theta}\, d\theta \right\}$$

Now substituting for $(dw/dz)^2$ the expansion in terms of $1/\zeta$ obtained from (i.), and putting $\zeta = Re^{i\theta}$ we have a series of terms of the type

$$\int_0^{2\pi} e^{in\theta}\, d\theta$$

where $n = 2, 1, 0, -1, \ldots$

But

$$\int_0^{2\pi} e^{in\theta}\, d\theta = 0 \text{ if } n \text{ does not } = 0$$
$$= 2\pi \text{ if } n = 0$$

Hence

$$M = \tfrac{1}{2}\rho I\left(2V^2 a_1 e^{2i\alpha} + iKV\zeta_0 e^{i\alpha}/\pi\right) 2\pi$$
$$= \rho I\left(2\pi V^2 c_1{}^2 e^{2i(\alpha + \gamma)} + iKVr_0 e^{i(\alpha + \lambda)}\right)$$

where

$$a_1 = c_1{}^2 e^{2i\gamma} \text{ and } \zeta_0 = r_0 e^{i\lambda}$$
$$\therefore\ M = 2\pi c_1{}^2 \rho V^2 \sin 2(\alpha + \gamma) + K\rho V r_0 \cos(\alpha + \lambda)$$

whence if M_1 be the moment of L about the centre M

$$M_1 = 2\pi c_1{}^2 \rho V^2 \sin 2(\alpha + \gamma)$$

The above proof is given in an article by Mises,* who has also given a proof of the formula $L = \rho VK$ along the same lines.

Appendix II.

1 *The Circular Arc Treated Geometrically*

In paragraph 4 of the present paper it was shown that when the centre M of the circle of transformation (Fig. 6) lay on the η axis, the circle transformed into a circular arc.

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* "Zeitschrift für Flugtechnik und Motorluftschiffahrt," Vol. 8 (1917), page 161.

P is the point $\zeta = re^{i\theta}$ on the circle.

P_1 is the point $\zeta = r_1 e^{-i\theta}$.

Now if PO is produced to meet the circle again in P_2 we have

$$OP \times OP_2 = c^2$$
$$\therefore\ OP_1 = OP_2 = c^2/r$$

hence P_1 is the point $(c^2/r)\,e^{-i\theta}$.

But the point P' on the arc corresponding to the point P on the circle is given by

$$z = \zeta + c^2/\zeta$$
$$= re^{i\theta} + (c^2/r)\,e^{-i\theta}$$

i.e., is found by adding the vectors OP and OP_1. Hence to obtain the point P' we complete the parallelogram POP_1P'.

It follows from the above that P and P_1 each transform into the point P' and hence P' describes the arc $B'TA'$ twice as P describes the circle.

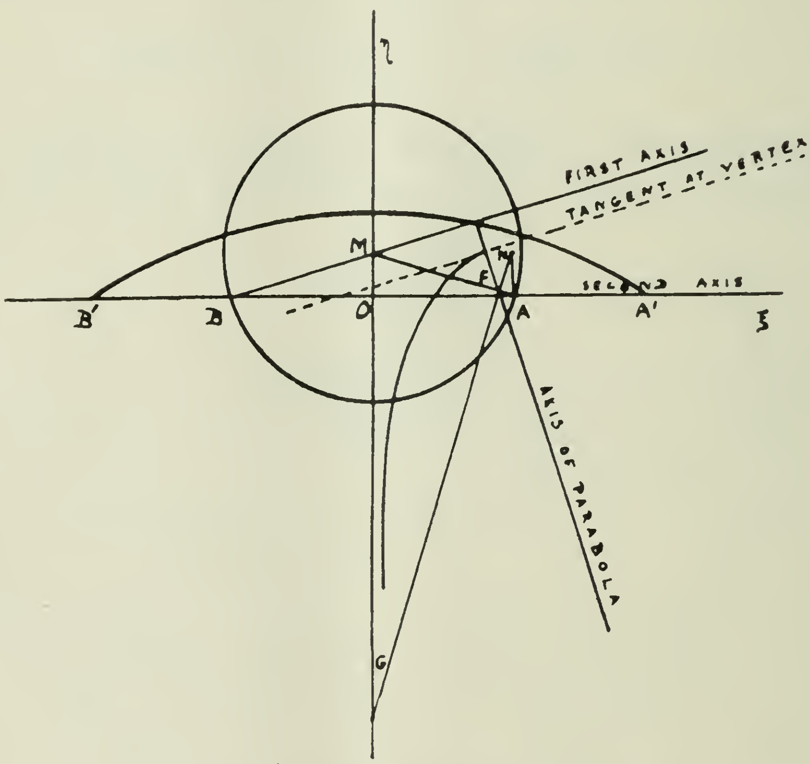

FIG. 13.—*Lift Parabola for Circular Arc.*

The focus F is a point in MA (Fig. 13) such that $MF = c^2/a$. The point is found geometrically by erecting a perpendicular AN to AB at A of length equal to OM, and drawing NF perpendicular to MA. It will be found that NF produced passes through the centre G of the arc $B'A'$.

The axis of the lift parabola is the line through F perpendicular to BM, the directrix.

2 *Construction of a Joukowsky Aerofoil*

We obtain a Joukowsky aerofoil when the centre M of the circle of transformation is taken so that the line joining B and M cuts the η axis (Fig. 14).

Let P be the point $\zeta = re^{i\theta}$ on the circle K with centre M and radius BM.

Then the corresponding point P' on the aerofoil is given by

$$z = \zeta + c^2/\zeta$$
$$= \zeta + \zeta_1 \qquad (18)$$

where

$$\zeta_1 = (c^2/r)\,e^{-i\theta} = r_1 e^{-i\theta}$$

Now the point P_1 given by $\zeta = \zeta_1$ is such that OP_1 is the inverse of OP with respect to a circle centre o and radius c, while OP_1 makes an angle $-\theta$ with the ξ axis.

But it is well known that the inverse of a circle is a second circle. Hence as P moves round the circle K, P_1 will move round a second circle K_1. Moreover, a circle K_0 with centre M_0 and radius M_0B will touch circle K at B. Again the circle K_0 inverted with respect to a circle centre o and radius c and then reflected traces out the same circle. Now since on inversion the angle at which two curves cut remains unchanged, it follows that circle K_0 will touch circle K_1 at B. Therefore the centre M_1 of circle K_1 lies on BM. But the circle K when inverted becomes a circle with its centre in MO produced. Hence, when this latter circle is reflected about the ξ axis its centre M_1 will lie on a line such that the angle $M_1OM_0 =$ the angle M_0OM. Thus the position of M_1 in the line BM is determined, and since the circle K_1 passes through B it can now be drawn.

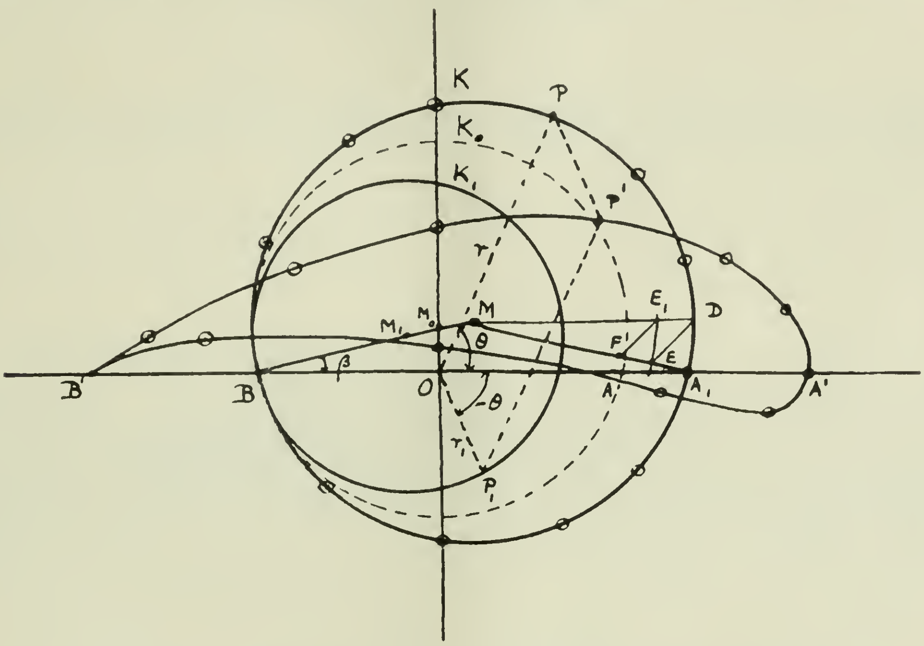

FIG. 14.—*Construction of a Joukowsky Aerofoil.*

The point P_1 on this circle K_1 is such that OP_1 makes an angle $-\theta$ with the ξ axis.

Then from equation (18) we see that the point P' is found by compounding the vectors OP and OP_1 by the parallelogram law.

Thus, by taking positions of P for which $\theta = 10°$, $20°$, $30°$, etc., a series of corresponding points on the aerofoil can be found.

As $\gamma = 0$, the focus F is a point in the line MA_1 such that $MF = c^2/a$.

To find F by a geometrical construction draw through M a line parallel to the ξ axis to meet circle K in D. With centre M and radius c draw an arc to cut MA_1 in E and MD in E_1. Draw E_1F parallel to DE to meet MA_1 in F.

Then F is the focus of the lift parabola and BM the directrix. The line of action of the lift for any given angle of incidence is then easily determined (Fig. 4).

In a previous paper* I have obtained the streamlines round the aerofoil from those round the given circle by a method similar to that used in constructing the

* R. & M., 788. Stream-lines round a Joukowsky Aerofoil.

aerofoil. The flow past the circle and the corresponding flow past the aerofoil are illustrated in Figs. 15 and 16.

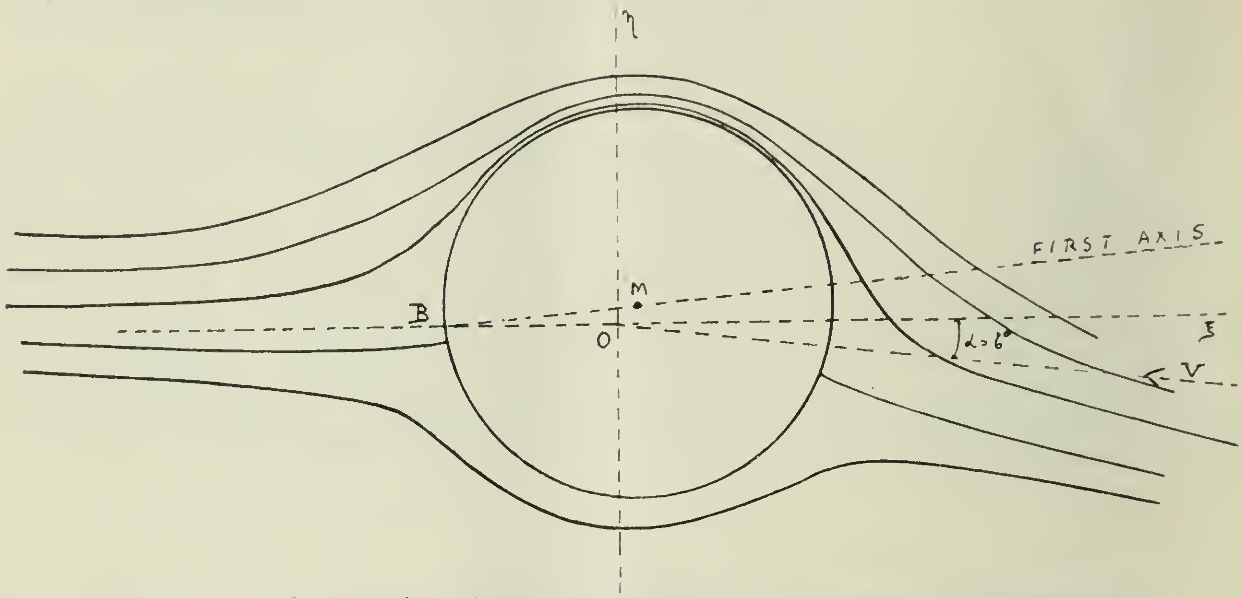

FIG. 15.—*Streamlines Round a Circular Cylinder with Circulation.*

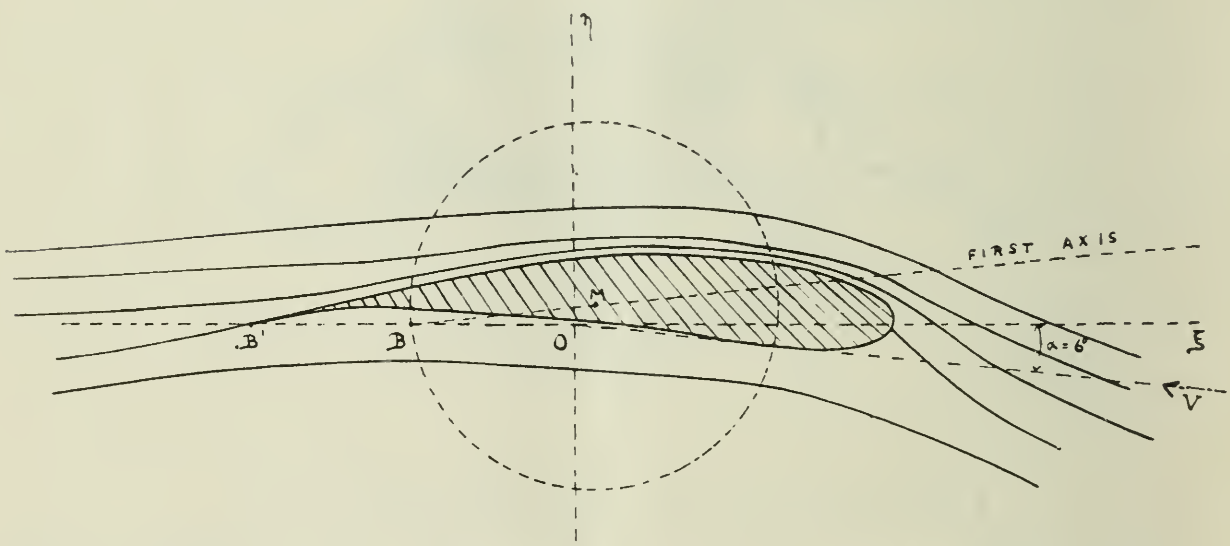

FIG. 16.—*Streamlines Round a Joukowsky Aerofoil.*

REVIEWS

Sur la Theorie des Surfaces Portantes

M. Roy. Gauthier-Villars et Cie, 1922.

Prandtl's aerofoil theory is receiving considerable attention at the present time, and M. Roy has now given a short account of the basis and development of the theory. Starting with a short summary of the fundamental equations of hydrodynamics, the author passes on to discuss the idea of circulation and the method in which it arises. After this point the lines of Prandtl's original papers are followed very closely, and the problems of the monoplane, biplane and wind channel interference are discussed in turn. The author concludes with a short account of the minimum energy losses of an airscrew as developed by Betz.

The book contains little that is new to the student of modern aerofoil theory, but it undoubtedly gives a good account of Prandtl's work, which will be of value to those who cannot obtain access to Prandtl's original papers. The author has also provided an interesting introduction to the fundamental ideas of the theory, but the student must be warned that the proof of the relationship connecting lift and circulation given on page 27 is quite unsound.

The Evolution of Climate

By C. E. P. Brooks, M.S.C., F.R.A.I., F.R.Met.Soc. With a Preface by G. C. Simpson, D.Sc., F.D., Director of the Meteorological Office.

In this book Mr. Brooks discusses the various theories to account for the past glaciation of the earth, and then gives his own conception of the cause. He gives also a more or less precise geological history of the various countries in which proofs of past glaciation have been found. He comments on the vast amount of literature on the subject and on the impossibility of any one man assimilating it all.

Mr. Brooks comes to the conclusion that the various hypotheses put forward to account for the past changes of climate are untenable. However, he fails to state the most conclusive argument against Croll's theory, namely, that whatever the combination of the eccentricity of the earth's orbit and the position of the apse may be, the total heat received in the year on each hemisphere remains invariable. If the total heat received is unaltered, it is difficult to believe that the mean annual temperature can be greatly altered by its different distribution. He also turns down the theory put forward by Tyndall and Arrhenius that the ice ages were due to changes in the composition of the atmosphere, probably correctly. But the question of the effect of radiation on atmospheric temperature is so complicated and difficult that it is hardly safe to dogmatise upon it, and it seems possible, though improbable, that changes in the amount of carbonic acid in the atmosphere might have some effect upon the temperature.

Mr. Brooks' own suggestion is that the changes in the distribution of land and sea may produce large changes of temperature. By tabulating the temperatures of various regions of the earth and correlating them with what he calls the " continentality " of the district, he has found a close agreement between the quantities. His conclusions are that in winter the effect of land to the West in temperate latitudes is always to lower temperature; that the effect of land to the East is almost negligible. In summer the general effect of land, whether to East or West, is to raise temperature, but the effect is not nearly so marked as the opposite effect in winter (see Quarterly Journal of R. Met. Soc., Vol. XLIII., 1917, p. 169, and Vol. XLIV., p. 253).

Mr. Brooks' conclusions are most interesting, and are well worth careful study. He considers also that a further cause of change of climate will be produced by a change of distribution of land and sea, because the anticyclonic areas would take up a different position and the usual track of cyclonic disturbances would change.

In our present state of knowledge, or want of knowledge, as to the cause of

cyclones and anticyclones, we may perhaps safely say that this would happen, but it is very difficult to give any detail. Would, for example, any three meteorologists agree as to the track that cyclones would follow if Scandinavia were glaciated? It must also be remembered that glaciation depends on snow-fall as well as on temperature. Raising the land to a sufficient height would produce glaciation, because it would lower the temperature and also cause copious snow-fall. But large parts of Canada and Siberia have mean annual temperatures far below the freezing point, as is evidenced by the permanently frozen ground, and yet no glaciation. Land to the westward in temperate latitudes cuts off winter rain, as well as lowers the temperature.

The chief part of the book gives some account of the various ice-ages in different countries. Perhaps the statements are given without sufficient hint that they are not generally recognised facts, but are facts mixed with more or less conjecture. But the book is very interesting and suggestive, and can be confidently recommended.

A Treatise on Engine Balance Using Exponentials

By P. Cormac.

Mr. P. Cormac's new book on Engine Balance, which forms a welcome addition to the literature of the subject, will be of special interest to aeronautical engineers in that it treats somewhat fully of aero engines, including radial and rotary engines. It is essentially a book for a student of considerable mathematical ability. Instead of the usual elementary graphical methods of balancing a number of rotating forces, or the alternative of expressing the resolved components in two directions at right angles by the circular functions, the author has obtained his results by the use of the exponential method of vector representation, in which a rotating force is denoted by $Z_0 e^{i\theta}$ and a force in one direction varying harmonically $Z_0 \cos\theta$ becomes $\frac{1}{2} Z_0 (e^{i\theta} + e^{-i\theta})$. The addition of a number of forces from a corresponding number of cylinders is much easier to evaluate than a series of circular functions, and the results are thus obtained with great directness. The proofs are of the simplest to any student of higher mathematics, but it is probable that the great majority of engineers will prefer the more cumbrous methods. The author has, however, provided a useful first chapter on the complex function, which should be sufficient for those unfamiliar with its use.

As already mentioned, in addition to the multi-cylinder-in-line engine, considerable space is devoted to radial and rotary engines, and it is in these cases that the greatest simplification occurs. The effect of off-setting is dealt with, and also certain special forms of engine such as the Leitlin rotary engine and oscillating cylinder engines, but it is remarkable that the book throughout is confined to engines in which the motion and forces in connection with each cylinder are identical. In view of the preponderance of makes of radial, rotary and Vee engines with master connecting rod assembly, it is regretted that some investigation into the considerable differences thus introduced was not attempted.

In addition to the study of the balance of the whole engine, the author has applied his method to an examination of the torque curve of multi-cylinder engines. As it is first necessary to obtain the torque curve for a pair of cylinders from the indicator diagram, and then to analyse it into the sum of a series of harmonic functions, it is difficult to see that the resultant torque will be obtained with any greater ease or accuracy than by the method of arithmetical addition. The concluding chapters are devoted to the reverse problem of the design of crank systems with specified balance.

Although it is considered that the book could be considerably improved, as indicated by a fuller treatment of the more common types of aero engines, even at the expense of other less usual types, yet it contains much which is not to be found in present text books on the subject, and indicates a method of approach to many similar problems often left unsolved because of the cumbrous analysis involved.

The Journal

OF THE

Royal Aeronautical Society

(Founded 1897 in succession to the Annual Reports)

Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

No. 152 AUGUST, 1923 Vol. XXVII

NOTICES

Associate Fellowship Examination

The second examination for Associate Fellowship will take place in the Library on Monday, September 24th (Part I.), and Tuesday, September 25th (Part II.). Intending candidates should forward entry forms (accompanied by the prescribed fee) on or before Monday, August 27th, stating the subjects in which they desire to be examined.

Lectures

The following programme of lectures has been arranged for the 59th Session commencing in October:—

Date.	*Author.*	*Lecture.*
1923.		
October 4th ...	Chairman's Inaugural Lecture	(Subject to be announced later.)
,, 18th ...	Sqn. Ldr. Hill	"The Manœuvres of Inverted Flight."
November 1st	Major Wimperis	"Present Developments in Aircraft Instruments."
,, 15th	Mr. H. R. Ricardo	"The Thermodynamics of Aircraft Engines."
,, 29th	Sqn. Ldr. Maycock	"Airmanship at Sea."
December 13th	Colonel Edmonds	"Air Strategy."
1924.		
January 10th...	Dr. Aitchison and Mr. North	"Materials from the Aeronautical Point of View."
,, 24th...	Dr. Ramsbottom	"Fabric and Dopes."
February 7th...	A Representative of the University of Tokyo... ...	(Subject to be announced later.)
,, 21st...	Mr. H. Hamshaw Thomas ...	"Aerial Photography and Survey."
March 6th ...	Major Tucker	"Sound Detection."
,, 20th ...	Captain W. S. Farren ...	"The Report of the Aeronautical Research Committee's Panel on Scale Effect."
April 3rd ...	Colonel the Master of Sempill	"The British Aviation Mission to the Imperial Japanese Navy."

Library

The following books have been received and placed in the Library:—Southport Auxiliary Observatory, Annual Report, 1921, Air Ministry; Report of Government Financial Assistance to C.A.T. Companies, Air Ministry; Rigging Notes, Avro Biplane, Type 504K, Air Ministry; Universal Camera Mounting, Air Ministry; Napier Lion (Series II.) Aero Engine, Air Ministry; Notes on Giant Aeroplanes, by J. Weiss and A. Keith; Fatigue of Metals, C. E. Stromeyer; Design of Commercial Airplanes, E. P. Warner; Flying Fishes and Soaring Flight, E. H. Hankin; Commercial Aeronautics in China, H. Chatley; Radiators for Aircraft Engines, S. R. Parsons and D. R. Harper; Malaises des Aviateurs, de Brichambaut and Behague; Atoms, J. Perrin; Scientific Papers of John Aitken, Cambridge University Press; L'Aptitude au voi en Avion, P. Perrin de Brichambaut; Gliding and Soaring Flight, J. B. Weiss; Dictionary of Applied Physics, Volume IV., Light, Sound, Radiology, edited by Sir Richard Glazebrook.

W. Lockwood Marsh, *Secretary*.

*SOME NOTES ON SIR GEORGE CAYLEY AS A PIONEER OF AERONAUTICS

BY J. E. HODGSON.

I propose to deal with the subject of this paper under three headings: First, a brief summary of Cayley's life and character; second, an account of his work on navigable balloons; and third, a like account of his work on mechanical flight. The latter sections will be prefaced with a short survey of the state of those two branches of aeronautical science at the period of Cayley's youth. It should, perhaps, be added by way of introduction, that Cayley's place as a pioneer in aeronautics has not yet been fully assessed. His name is barely mentioned in that greatly over-rated, ill-arranged and cumbrous compilation, "Astra Castra," published by Hatton Turnor in 1865, and it is entirely omitted from the "Dictionary of National Biography." In English aeronautical histories—for the most part regrettably unscholarly and inadequate—the comprehensive character of Cayley's work is usually overlooked, and though he is more frequently mentioned in terms of respect by French writers, their accounts are inevitably incomplete. For with exception of the excellent reprint of the essay on "Aerial Navigation"—dealing mainly with mechanical flight—published in 1910 by the Royal Aeronautical Society as the first of a series of "Aeronautical Classics," Cayley's writings—notably his investigations into the problems underlying navigable balloons—are not readily accessible.

Notes on Cayley's Life

Sir George Cayley—he was the sixth baronet since the creation of the title in 1661—was born at his ancestral home Brompton Hall, in the North Riding of Yorkshire, on December 27th, 1773. The bent of his mind towards scientific pursuits became apparent in early years, and it is said that his interest in aerial navigation—"a noble art" as he subsequently termed it—was inspired when he was not more than 10 or 12 years old by the Montgolfiers' discovery of the balloon. As he himself relates his first experiment in aeronautics was made with a Chinese "flying top" in the year 1796, and it is significant of his steadfast attachment to the subject, that as late as 1854, he sent a description of an improved type of this aerial toy to the distinguished French aeronaut and writer, Dupuis-Delcourt. In 1795 he married Sarah Walker (the only daughter of Rev. George Walker, F.R.S., and President of the Literary and Philosophical Society of Manchester), by whom he had a large family, the members of which in later years were taken for extended tours to France and Italy. As a young man he was influenced—like Wordsworth and many more of his contemporaries—by the outburst of the French Revolution, and he took an active part on the Whig side in county politics, acting for many years as President of the York Whig Club. But though inspired with warm feelings in the cause of liberty, he was not drawn into sympathy with any unconstitutional proceedings, and in the spirit of a true Englishman, he took his part as the commander of a corps of volunteers when Napoleon threatened this country with invasion. His first published contributions on "Aerial Navigation" (mechanical flight) appeared in Nicholson's "Journal of Philosophy" in 1809-10; in 1816-17 he expounded in the pages of Tilloch's "Philosophical Magazine" his ideas on navigable balloons, a subject to which he returned in letters published in the "Mechanics' Magazine" in 1837.

* A Paper read before the Newcomen Society and published by arrangement with that body.

Further contributions (suggested by the discussion on Henson's "Ariel Carriage") to the latter periodical in 1843, with two articles in the Bulletin of the "Société Aérostatique et Météorologique de France" in 1853, complete the record of his extant writings. It can hardly be doubted that these cover but a small part of all the investigations, calculations and experiments made on aeronautical matters over many years. Of his life and scientific labours apart from aeronautics little is known. Politics and the affairs of the family estates doubtless occupied much of his time, but it is clear that his chief concerns were science and engineering in general, and aeronautics in particular. He built a shed or outhouse at Brompton in which he conducted a large number of experiments on the application of electricity as a motive power, and on the development of the gas engine. In the latter direction he contributed to Nicholson's Journal in 1807 a "Description of an engine for affording mechanical power from air expanded by heat," but though constructed at Newcastle on what Cayley termed "a considerable scale," it proved unsuccessful.

Later in life he accepted the congenial office of chairman of the old Polytechnic Institution, while in 1852 he stood successfully as Parliamentary candidate for the borough of Scarborough. But at his advanced age the duties and responsibilities of a Member of Parliament must have proved arduous, and it has been suggested that he only entered Parliament in order to draw public attention to a subject which, rightly and with remarkable foresight, he realised to be of the greatest importance to this country and to civilisation at large. He is not known, however, indeed he is not likely to have found opportunities of impressing his views on the House. In any case he retired after two years and died at Brompton Hall on December 15th, 1857.

Of Cayley's character it is difficult, from the meagre knowledge available, to form a true conception. I regret it is not possible to do so, for as one whose studies have mainly concerned men of letters and men of action, I know the value of a just and lively estimate of a man's character, as a help to understanding his work and his aims. Engineers and inventors are just as human as other men, and their characters are surely worthy of, and should receive, a due measure of attention. From the little we do know of Cayley, I imagine that his temperament and environment tended to make him a genial country gentleman—one blessed with ample means, with a family to rear and educate, an estate to manage, and local affairs to engage some measure of his time—rather than to develop an engineer or a scientist, whose whole life was destined to be sternly devoted to one or other of those fields of endeavour. At the same time I feel that the late Sir Walter Raleigh, in the necessarily brief account of Cayley contained in the introductory chapter to his "History of the War in the Air," was a little depreciatory in suggesting that Cayley merely "amused his leisure with science," as also in his remarks that Cayley "put nothing on the market," and was "content to enunciate a truth and to call it probable." For Cayley's work, as we shall see, was seriously undertaken, though I gather he had no ambition to earn rewards, either material or otherwise. He himself disavowed any desire "to scramble"—as he called it—for any share in the credit, at that time too often mistakenly attached to the invention of aerostatic machines, "save only," he significantly added, "that of braving the risible muscles of my friends by substituting *acres* for *yards* of cloth in their structure." Indeed the moderation he exercised in setting forth his ideas—indicative of a certain measure of greatness—affords a marked contrast to the assurance frequently displayed in contemporary ballooning projects. Nevertheless, he sought to encourage the work of others—his criticism of Henson's "Ariel Carriage," for instance, was not less restrained that it was sound—and in his eagerness to acquire information he carried on a considerable correspondence with scientists and inventors of his day, both at home and abroad. I have read that it was due to Cayley's generous

financial help that his friend, Baron de Ferussac, was enabled to start the publication of the "Bulletin Universelle." One can only add that the amplitude of Cayley's views on aerial navigation suggests a mind of great breadth of view, endowed with remarkable imaginative foresight. For as early as 1809 he expressed complete confidence in the practicability and security of mechanical flight as a method of transport, at a speed exceeding that of the railway train, and by 1816-17 he was advocating the navigable balloon with even stronger conviction, as offering "a direct, swift and easy floatage from any one point to every other on the face of the globe." His large conception of air travel cannot be more forcibly expressed than in his own words. "An uninterrupted navigable ocean," he said, "that comes to the threshold of every man's door, ought not to be neglected as a source of human gratification and advantage."

State of Contemporary Aerostatic Science

I pass to outline, as briefly as possible, the state of the science of aerostation, particularly with regard to the direction or control of balloons, at the time when Cayley first turned his attention to the subject. It is well known that the balloon was invented by the brothers Montgolfier in 1783, and that in October of that year Pilâtre de Rozier first ascended into the air in a balloon of the Montgolfière type—that is a balloon inflated with hot-air rarefied by means of a fire burning in a brazier. In the following December this method of inflation was improved upon by J.-A.-C. Charles, who introduced the use of hydrogen. After hundreds of years of aspiration and fruitless endeavour, these achievements were naturally regarded with wonderment and gave rise to extravagant hopes of utility in the service of man. But it was realised almost at the outset, that the use of the balloon as a method of aerial travel would be greatly limited, unless it were possible to devise a method of control. The case was aptly put by a contemporary versifier in four lines:—

"To Montgolfier the invention's due
Unfinished as it lies,
But his will be the glory
Who direction's art supplies."

Hence arose countless schemes, mostly emanating from France, conceived to accomplish that obviously desirable object. By a natural but erroneous process of inventive thought, the false analogy of the boat sailing on the water and steered by a rudder, was followed in the earliest projects. One of the first designs embodying the sails and rudder principle was that of Thomas Martyn, a natural history draftsman, who published in London an engraving of his "Aerostatic Globe," which he claimed to have originally designed in November, 1783. Such suggestions can have served no other purpose than that of making clear the apparent lack of what was then commonly termed a "point d'appui," and, as a corollary, the need of applying a propelling force. Oars or wings, consisting of a light framework covered with silk or other material, were the first forms of mechanism designed to afford manual propulsive power, J. P. Blanchard, in March, 1784, being the first to experience—though not to admit—their inefficiency. Other projects tried or suggested were forms of jet propulsion (by means of hot-air, on the principle of the æolipyle of the ancients), or by the reaction of gunpowder exploded in the form of rockets. Yet another method—invented by David Bourgeois in 1784, which also engaged the attention of Montgolfier in France, and (at a slightly later date) of R. L. Edgeworth and Cayley himself in Great Britain—involved the use of an adjustable plane surface fitted beneath the balloon, whereby to obtain some measure of control from the pressure of air on such plane during the rise or fall of the balloon. Of the great majority of these early schemes it need only be said that they were faulty in theory or futile in application. An important exception must be made, however,

in the case of the theoretical examination of the principles involved, undertaken by J.-B.-M. Meusnier, equally distinguished in military, scientific and mechanical achievements. As early as December, 1783, he prepared a memoir in which he suggested the need of making the form ellipsoidal, and introduced the use of the air ballonet. It is probable, however—I am indebted on this point to my friend Colonel Lockwood Marsh, Secretary of the Royal Aeronautical Society—that Meusnier erroneously conceived the ballonet as a means of height control rather than for the purpose it more truly serves in modern practice—namely, to preserve the shape of airships of the non-rigid type. But Meusnier's work and the drawings and plans which accompanied it, are very remarkable, as well in respect of form and the ballonet, as in his ideas of diagonal rope suspension of the car and in the application of airscrews—worked, of course, by manual power—for propulsion. The airship—it deserves to be so termed, if only for the fact that it was designed to be 260 feet in length—was never constructed, and the only contemporary test of the ballonet principle was in its profitless application to the elongated free balloon of the brothers Robert, in which they made ascents during July and September, 1784. An air ballonet was, however, incorporated in the design of a "fish-formed" balloon invented in 1789 by Baron Scott, a French officer, who may possibly have taken his idea of shape from the so-called "Flying Fish" aerostatic machine exhibited in Cornhill in 1785, which there is reason to believe was the work of John Hoole, son of Samuel Hoole, watchmaker and mechanician, and (more notably) the friend of Dr. Johnson. Incidentally, it may be added that Johnson not only gave expression, at an early date, to doubts on the utility of the balloon—"a species of amusement," as he characterised the invention in a letter to Dr. Brocklesby, "for I do not find that its course can be directed, so as that it should serve any purpose of communication"—but agreed that wings would not assist as a means of direction. Scott's ideas more probably inspired S. J. Pauly, a Genevan gunsmith, who actually constructed a "fish-formed" dirigible which he tried with some measure of success at Sceaux in 1802, deriving an inadequate propelling power from the use of wings or revolving oars. In 1815 he came to London, and with the financial help of Durs Egg, doubtless known to many of you as a noted London gunsmith, the "Dolphin Balloon" was commenced at Kensington. The project has an indirect connection with Cayley—whose published writings on dirigible balloons it preceded—inasmuch as on hearing of it, and realising the great expense of such ventures, he reaffirmed the need of conducting experiments by means of public subscriptions. The envelope—about 90 feet long—was made of layers of gold-beaters' skin, and it was to contain an air ballonet of 21 feet in diameter, horizontal stability being maintained by means of an adjustable weight suspended between the tail of the balloon and the car. A more important feature was the intention to obtain "the propelling impetus" from "a kind of atmospheric steam engine, invented by Mr. Collier." But financial and other difficulties proved insuperable, and "Egg's folly," as it was called by the cynics, was never completed. It represents, however, in the main, the stage the navigable balloon had reached in Cayley's day, the chief features being the shape—tending towards streamline—the air ballonet to preserve that form, the suspension of a gondola or car beneath the envelope, and the suggested use of steam as a prime mover.

It will of course be appreciated that the foregoing remarks are but the merest outline, and that there were many aspects of the problem which, in those early days, had received but small consideration at the hand of the pioneers. These men aimed primarily at using the principle, the "floatage" as Cayley called it, of the balloon, as an aeronautical device, and by a modification of the original shape and the application of motive power, rendering it capable of direction and control, thus converting it into a machine fitted to the purpose of aerial navigation. The highly complex and multifarious questions in physics, mechanics, metallurgy, and so forth, with which the airship designer and constructor of

to-day has to deal, were, perhaps fortunately, beyond the ken of the pioneers. They strove with such elementary knowledge as they possessed to do no more, but no less, than drive and steer a balloon through the air—an achievement which was not actually accomplished until 1852, when Henri Giffard, in an elongated balloon fitted with a steam engine of three horse-power, obtained an independent speed of six miles per hour.

Cayley's Work on Navigable Balloons

Coming to Cayley's own work on dirigible balloons, it has been already mentioned that his first published essays on the subject appeared in Tilloch's "Philosophical Magazine" during 1816-17. It is true that his earliest writings on aeronautics dealt with mechanical flight, but I take his contributions to "lighter-than-air" theory first, because his ultimate faith in the success of aerial navigation over the world's surface was based on the possibilities of the navigable balloon. He made this clear beyond dispute in one of his last letters on the subject. He pointed out that it had been proved—to use his own words—on "tolerably well-ascertained data," that elongated balloons of a large size were capable of being driven through calm air at a speed approaching that of the railway train, and could carry a considerable cargo by reason of their buoyancy. From these premises he argued that "on a great scale, balloon floatage offers the most ready, efficient and safe means of aerial navigation." "Elongated balloons of large dimensions," he wrote in the same paper, "offer greater facilities for transporting men and goods through the air, than mechanical means alone, inasmuch as the whole weight is suspended in the air without effort . . . and when the invention is realised, it will abundantly supply the increasing locomotive wants of mankind." Moreover in the sentence immediately following he gave prophetic utterance, with remarkable foresight, to the view which, after much ebbing and flowing, has in quite recent years received the support of distinguished aeronautical experts—the view that the relation of airships to aeroplanes is complementary and not competitive. "Mechanical flight," Cayley wrote in 1843, "seems more adapted for use on a much smaller scale, and for less remote distances; serving, perhaps, the same purpose that a boat does to a ship, each being essential to the other."*

Broadly speaking, it was Cayley's ability to grasp the basic scientific or mechanical principles underlying the theory of navigable balloons, rather than his skill as an inventor or designer, wherein lies his true greatness as a pioneer of the airship. He was, for instance, one of the first to realise fully the practical significance of one of the main factors on which airship theory rests—namely, the physical law that "the surfaces (and hence the resistance) increase as the squares of the diameter of the balloon, whereas the capacity to contain gas (and hence the supporting power) increases as the cubes of the diameter." If it were not for that principle he knew quite well that the difficulties would be far greater; or, as he himself put it, "we must be contented to give up balloons for purposes of locomotion altogether, or to attempt them on that scale of magnitude which a well-grounded calculation of their power proves to be necessary." As to the factors involved in the question of resistance, he pointed out (as indeed others had already done), that obviously the spherical form of the ordinary free balloon should be lengthened horizontally, thus diminishing the cross-section for the same volume, and he realised the desirability of dividing the gas into several compartments—as he said, "like the stomach of a leech." With more originality he

* The importance Cayley attached to the advantages of the airship is revealed in the fact that while he first stated them in the "Mechanics' Magazine," March, 1837, he not only reprinted them (as an interpolation), but enforced them in the final paragraph of his essay on mechanical flight, contributed to the same magazine in April, 1843.

even suggested that dirigibles, "when used as permanent vehicles and on the true scale of magnitude, will probably be made of thin metallic sheets kept firm by condensation [that is pressure] with separate light bags of gas within." For a first experiment on these lines he suggested that Charles Green's large balloon —presumably the famous "Nassau Balloon" of 1836—should be requisitioned, and that two other balloons of smaller size should be "packed at opposite sides of this larger one," a suggestion which, however, was not tried. Of greater interest is his early anticipation of some degree of rigidity, revealed in the proposal to guard against the then unknown laws of resistance offered by fluids to solid bodies, by means of "light poles and internal cross bracings of wire or cord," designed to preserve the shape of his elongated spheroidal balloon. Moreover Cayley carefully considered the problems affecting the transmission of power from an engine suspended in a car beneath the envelope to the balloon itself—a point of particular moment in his own design owing to the need of keeping the boiler and furnace of the steam engine which he proposed to fit as far as possible below the envelope. As to his "prime mover," he had perforce to adopt the steam engine, but it is, I think, quite clear that he realised it was by no means an ideal form of power for the purpose, and there is reason to believe that his experiments, both with gas engines and electricity, were largely inspired by the idea of turning those sources of energy to account in aeronautics. With regard to the fabric of the envelope, which necessitated, as Cayley laid down, a material "perfectly air-tight, light and strong," he suggested that the great expense of silk (covered with india-rubber varnish) would be prohibitive, and proposed as an alternative "double-cotton Indian-rubber cloth," as invented in 1823 by Charles Macintosh for waterproof garments and air-tight cushions. In this connection it is of interest to note that Cayley's work was not wholly theoretical, for in 1816 he refers to a cloth weighing ½lb. per square foot as used in "my experiments."

It is not possible within the limits of this paper to enter on any full account of Cayley's designs for a navigable balloon. It must suffice to give some indication of his ideas in general, and the calculations (in all cases quoted from his own writings) on which they were based. In his first design, as communicated (with accompanying plans) to Tilloch's Philosophical Magazine in 1816-17, he provided for a Montgolfière or hot-air balloon 300ft. long, 45ft. in elevation, and 90ft. wide, made of "woollen cloth." In form it was an elongated spheroid, with a conical head and a slight tapering towards the stern—on the axiom common among sailors, that a ship to sail well should have a "cod's head and a mackerel's tail"—and it was to be kept to its shape by the light poles and cross bracing before mentioned. Professor Raleigh has remarked on the soundness of Cayley's ideas on what is now known as the doctrine of streamline, but he points out that though Cayley realised that the shape of the hinder part of a solid body travelling through the air is of as much importance as the shape of the fore part, he does not seem to have known that it is actually of more importance. In the first design the impelling power was to be derived from the deviation obtained from the pressure of air on a passive plane surface, a wholly inadequate method, the idea of which, as Cayley admits, was derived from the plan described by John Evans a year earlier. In his next essay, Cayley goes far beyond his original ideas, and offers calculations based on the possibility of propelling an elongated balloon by wing waftage—that is, wings fitted to the sides of the car and actuated by mechanical power. He expressed a preference for this (mistaken) form of propulsion as against "rotary wafts" or airscrews, mainly owing to the difficulty of "giving firm support and communicating motion to the latter," though admitting the advantage they afforded of uniform action. This improved design was to be 144ft. long, with a lift of 163,000 lbs., which reduced by the weight of the materials (1,700 lbs.) and of the engine, boiler, fuel, etc. (15,210 lbs.), would leave about 34 tons. Stated in terms of performance, Cayley estimated that this would allow of the transportation of 50 men for 48 hours, or a voyage of 960 miles

in calm air. Further consideration of this "balloon of 50 tons" led him to comment on the difficulties involved in the "stupendous bulk"—difficulties arising from expenses of construction and inflation (he estimated hydrogen to cost £300 a ton, though looking to a reduction by the application of new methods of production) and difficulties in "disposing of them when not employed." He does not appear to have contemplated the construction of immense airship sheds, such as have been erected in recent years, for he made calculations on the "horizontal drag" of large balloons at anchor, from which he deduced that this drag diminishes with size and oblong structure. Indeed, at a later date he laid down that "permanently-filled balloons would ride out storms when properly secured, without the danger of being driven to the earth or damaged."

In 1837 he revised the foregoing speculations in a lengthy communication to the "Mechanics' Magazine," wherein he sought to present considerations which would be "most conducive at present towards a final accomplishment of the aerial object in view." His elaboration of the "inclined plane" method of propulsion —of which he offered what he called "a rough and hasty sketch" by way of a plan—involved the combination of a large Montgolfière below and a smaller hydrogen balloon above, separated by the plane. But apart from the danger of such a combination—a danger experienced with fatal results in 1785 by Pilâtre de Rozier —the method was not worth even the brief consideration Cayley gave to it. Moreover, the twenty years or so which separated his essays had resulted in improvements in the steam engine, which by 1837 gave promise of greater power for weight. In this connection Cayley referred to the steam carriage of his friend Sir Goldsworthy Gurney (he wrote of it in 1837 as "recently completed"), the engine of which he calculated would give one horse-power for 200 lbs. weight. But though he recognised that "lighter first movers than steam engines may be discovered, and made applicable to propelling balloons," he proceeded to take the case as he found it. With a hydrogen balloon of a similar shape to his original design, 90ft. in diameter and 315ft. long, he estimated there would be available for engine power, crew and cargo, a lift of about 29¼ tons. He further estimated that a balloon of this size, bearing a strong resemblance, as he put it, "to a hundred-gun ship," would require an engine of 60 horse-power.

As to the application of propelling power, he still adhered to the idea of wings arranged in two tiers, but he also reverted to the idea of airscrews— "oblique vanes," as he said, "reversing the action of the sails of a windmill." As to the latter, he instanced the results of experiments made by the French Academy, which went to show that "a proper fulcrum or resistance for the engine power to work upon can be had at a velocity of 25 feet per second." Finally, reckoning the engine, with fuel and water for four hours, at 510 lbs. per horse-power, and deducting also the weight of the "machinery for waftage," Cayley arrived at the conclusion that travelling at the designed speed of 14 miles per hour his balloon would have a useful load of about 9 tons.

Cayley never attempted the construction of a large navigable balloon, but we may well believe he was only deterred by reason of the great expense. This difficulty led him in 1816 to suggest experiments by public subscription, an appeal he renewed in 1837 in the form of a proposal to organise a "Society for Promoting Aerial Navigation." It was doubtless the indifference shown towards his proposals that led him to write in 1843—in words which, with some modification, are even to-day not wholly inapplicable to the prevailing attitude towards the airship—"I think it a national disgrace in these enlightened locomotive times not to realise by public subscription the proper scientific experiments, necessarily too expensive for any private purse, which would secure to this country the glory of being the first to establish the dry navigation of the universal ocean of the terrestrial atmosphere."

Contemporary Ideas on Mechanical Flight

Turning to the general conceptions of mechanical flight in Cayley's time, and the ideas which had led up to them, it may be said at once that there were only two points of view—the one from which flight was regarded as a matter of flapping wings, and the other as a matter of impossibility any way. The old, the very old idea of wings, was doubtless inspired by the natural, not to say obvious, analogy of the flight of birds, a sense in which the practicability of the thing was first expressed by Roger Bacon in the middle of the 13th century, while early in the 16th century the idea occupied the great mechanical genius of Leonardo da Vinci. Indeed, broadly speaking, it is true to say that flight was invariably regarded in the light of the imaginary achievement credited by Francis Bacon to the inhabitants of his "New Atlantis," 1627. "We imitate also the flight of birds," says the sage, in a marvellously prophetic recital of countless mechanical devices and scientific notions practised in that Utopian community. Those words held good for something like the next two hundred years, not only, indeed, as an expression of aims, but as a baneful conception which blocked other fields of speculation. For that reason it is unnecessary to enter into any detail, or to recall the endeavours—more numerous, probably, than is commonly realised—made during the 18th and 19th centuries to achieve flight by means of wings. Even in the years immediately preceding the discovery of the balloon, J.-B. Blanchard was engaged in futile and fruitless attempts to construct a "flying vessell." Not that the study of the principles involved in bird flight was in itself profitless; on the contrary, Lilienthal himself regarded it as "the basis of aviation." But up to Cayley's day, and long after, those principles were not understood; they gave rise to the most varied and impossible speculations, and the blind attempts made to imitate their apparent characteristics diverted attention from the more strictly mechanical aspects of flight. It is because Cayley was amongst the first to approach the problems of flight from the mechanical and not, so to speak, from the ornithological point of view, and because he first conducted "gliding" experiments on a considerable scale, that he deserves to stand among the great pioneers of aviation in the direct line between such names as Leonardo da Vinci in the 16th, and John Stringfellow in the 19th century.

Cayley's Work on Mechanical Flight

It has been seen that Cayley's first experiment in such matters was made as early as 1796 with a Chinese or aerial top (identical with the device exhibited before the French Academy of Sciences in 1784 by Launoy and Bienvenu), which served at once to illustrate the principle of the helicopter and the airscrew. Though but a toy of a few inches in length, its capacity to demonstrate certain elementary but important principles in aeronautics, made a lasting impression on Cayley's mind, and (as already mentioned) only three years before his death he sent to Dupuis-Delcourt a drawing of one which he had made—the best, he said, that he had ever seen, capable of rising 90 feet in the air. Having collected a body of "facts and practical observations in the course of much attention to the subject"—to use Cayley's own words—he published his first essays "On Aerial Navigation," dealing with the subject wholly from the point of view of mechanical flight, in the pages of Nicholson's Journal during 1809-10. The character of his observations and experiments is at once shown in his ability to grasp essential principles. His reflections on bird flight led him to the belief—confirmed by so great an observer as Charles Darwin nearly 25 years later—that flying required less exertion than was then commonly supposed. But he categorically denounced the idea of flight by means of wings (worked by muscular effort) as ridiculous. In his clearly expressed conviction that mechanical flight was possible, and in his enunciation of the "whole problem" as contained in the simple but comprehensive formula, "To make a surface support a given weight by the application of power

to the resistance of air," he is revealed as the earliest true pioneer of the aeroplane. He had perfect confidence in the practicability of transporting passengers and goods—the latter word is expressive of his large ideas—"more securely by air than by water, and with a velocity of from 20 to 100 miles per hour."

His earlier experiments in aerodynamics had revealed (the figures quoted are Cayley's calculations) that a surface of one square foot moving at a velocity of 11.538 feet per second generated a resistance equivalent to 4 ozs., or at 17.16 it gave 8 ozs. With some such data—obtained by an early form of "whirling table"—he proceeded to make "gliding" experiments, carried out over a number of years, on what he called "a considerable scale of magnitude"—apparently with a machine having a surface of 300 square feet—and his enthusiastic description of one of the trial flights (made from the high ground behind Brompton Hall) may be quoted as the first of its kind (and therefore historic) and as in itself of great interest. "It was beautiful," he wrote in November, 1809, "to see the noble white *bird* sail majestically from the top of a hill to any given point of the plain below it, according to the set of the rudder, merely by its own weight, descending in an angle of about 18 per cent. with the horizon." Cayley states that the upward lift of this "gliding" machine was at times so strong that anyone running forward in it against a light breeze would be raised from the ground for "several yards together."

But even at this date Cayley's thoughts were also engaged on the question of propulsion, or the necessity of a "first mover" as he termed it. Realising that a steam engine of the type invented by Boulton and Watt would be inadequate, he looked more hopefully to the development of some such engine as was reported to have been designed by William Chapman of Newcastle—an early form of internal combustion engine, with oil of tar as fuel. Evidently he intended to make some trial of a "propelling apparatus" with his "glider," for he refers to the fact that an accident prevented his doing so. He continued, however, to consider many other factors involved in mechanical flight—problems dealing with questions of initial velocity, the leverage on the wings, and the need for lightness in construction combined with strength. The latter he suggested for the first time might be achieved by designing superposed surfaces, as now usual in the biplane. The wings he conceived should be supported by "diagonal bracing"—which he termed "the great principle for producing strength without accumulating weight"—while he foreshadowed the necessity for streamline design in the maxim, that "in the art of aerial navigation every pound in direct resistance that is done away with will support 30 lbs. of additional weight without any additional power."

On the publication in 1843 of particulars of Henson's "steam carriage," he returned to the subject of mechanical flight, and further explained his ideas in two letters to the "Mechanics' Magazine" in April, 1843. His criticism of Henson's scheme, though accompanied at the outset with an expression of encouragement, clearly indicated his reasoned doubts as to its success. In the first place "the magnitude of the proposed vehicle," involving a terrific stress on the necessarily light structure of the main supporting surfaces or wings—150ft. span and 30ft. chord, as designed—afforded ground for serious misgivings. The stress or "leverage" on the wings Cayley again suggested might be overcome by securing the required surface not "in one plane, but in parallel planes one above the other," and he went so far as to propose a tri-plane, or "three-decker" as he termed it. Moreover, while expressing his conviction "that the inclined plane, with a horizontal propelling apparatus, is the true principle of aerial navigation by mechanical means," Cayley doubted Henson's ability to provide the "very great engine power—the *sine qua non* of the case"—which the design required.

As in the case of navigable balloons, Cayley's design for an "aerial

carriage" (which he described in the same paper) is hardly as interesting, and certainly not as sound, as his examination and discussion of theoretical principles. His plan shows that instead of obtaining the required supporting surface by means of rigid horizontal wings, he proposed two sets of superposed circular planes (designed when in motion to act as helicopters) set at an obtuse angle, and revolving in contrary directions. These circular planes he termed "elevating fliers" to distinguish them from two smaller horizontal airscrews for propelling the machine. The "framing" or fuselage was to be covered with canvas at once to increase the surface and afford protection to the engine, while a "broad horizontal rudder or tail" was designed for use in ascent or descent, and to act as a stabilising "elevator" in flight, with a small vertical rudder for lateral guidance.

Conclusion

Just a word or two in conclusion. Though it be true that Cayley, unlike other pioneers of mechanical inventions, "put nothing on the market," I do not think we should allow this fact to detract from his merits. After all, to Shakespeare at least, the conception and cultivation of an idea was of far greater significance than any deeds which might spring therefrom. Moreover, though I find it difficult to assess the influence of Cayley's experiments and theoretical writings, it may surely be allowed that his ideas were in the direct line of true progress, and led to ultimate accomplishment. It has been said by an authoritative French writer, Alphonse Berget, that Cayley's name deserves to be recorded "in letters of gold at the beginning of the history of the aeroplane." Whether it might not fitly be so written at the beginning of the modern history of aeronautics is a matter of opinion. In any case it will, I think, be generally agreed that it deserves to be writ large and in imperishable ink on the roll not only of England's, but of the world's great aeronautical pioneers.

Cayley's aeronautical writings, as referred to in the foregoing "Notes," are:—

1. On Aerial Navigation (Mechanical Flight), contributed to Nicholson's "Journal of Philosophy," Vol. XXIV., 1809, pp. 164-174; Vol. XXV., 1810, pp. 81-87 and p. 161, etc.
2. On Aerial Navigation (Dirigible Balloons), Tilloch's "Philosophical Magazine," Vol. XLVII., 1816, pp. 81-86 and 321-329; also Vol. L., 1817, pp. 27-35.
3. Practical Remarks on Aerial Navigation (Dirigible Balloons), "The Mechanics' Magazine," Vol. XXVI., 1837, pp. 418-428; also (in the same publication) Retrospect of the Progress of Aerial Navigation, pp. 263-265, and On the Principles of Aerial Navigation, pp. 273-278, both in Vol. XXXVIII., 1843, and both dealing with Mechanical Flight.
4. Mémoire sur le Vol Artificiel (Wing-Propelled Gliding), "The Bulletin of the Société Aérostatique et Météorologique de France," No. 4, 1853, pp. 147-151.

The three papers in Nicholson's Journal (No. 1) were reprinted in the Aeronautical Society's Annual Report for 1876, and again as Aeronautical Classics, No. 1, 1910; also in Means' Aeronautical Annual for 1895. The "Practical Remarks" (No. 3) were reprinted in "Aeronautics," Vol. II., 1909, p. 142, and Vol. III., 1910, p. 1.

Wilbur Wright Lecture, May 31st, 1923

RELATION BETWEEN AERONAUTIC RESEARCH AND AIRCRAFT DESIGN

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It is a great honour to be invited to give the Wilbur Wright Lecture on Aeronautics, especially so for a fellow citizen of the Wright brothers. I think that I appreciate the honour all the more because of personal relationships with Mr. Orville Wright and because, since the day of their first successful cross-country flight, I have had the opportunity of realising the truly unique qualities of these great men. The fact cannot be emphasised too often that, from the very beginning of their work, their point of view was that of the scientific investigator. Empirical methods, engineering development, did not satisfy them; they wished to know the underlying scientific facts, and to build on them. They had, in reality, the true concept of the purpose of the great aerodynamic laboratories of to-day.

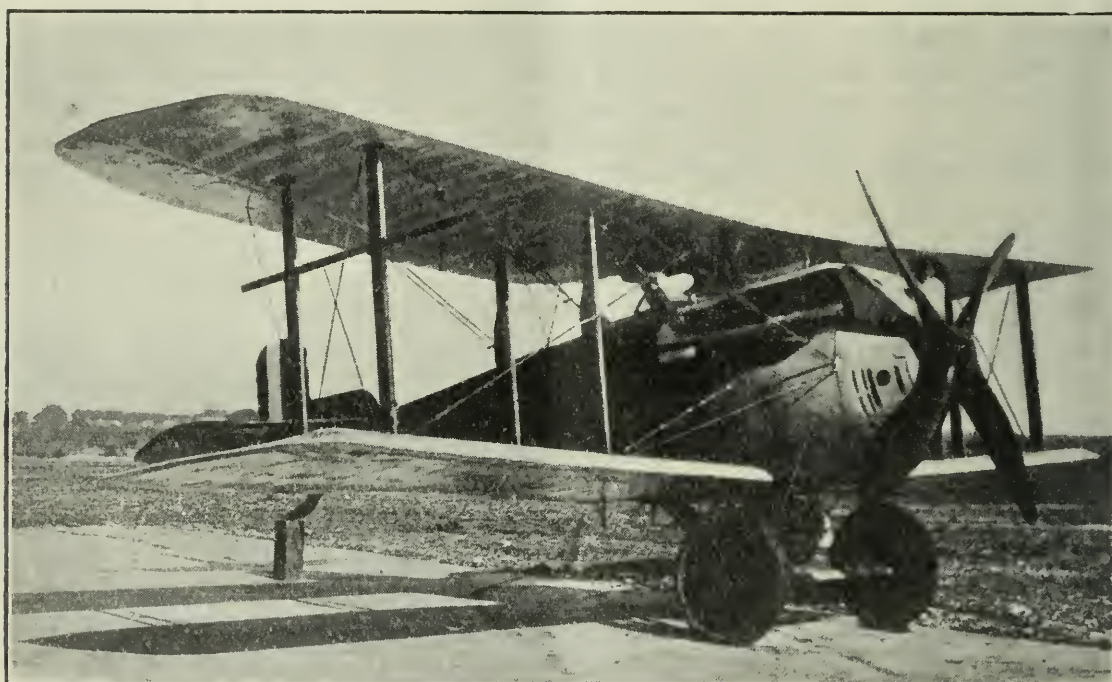
The selection of a subject for the Wilbur Wright Lecture is not an easy matter, especially when the selection must be made months in advance and when, as in this case, the request was made to send the title at once by cable. I confess my title is banal, but it was the best I could think of which would be sufficiently indefinite to allow me to include in the lecture the results of several investigations then in progress. For there is always a grave uncertainty in any physical investigation as to the day when the results obtained will have sufficient value to be reported.

The aerodynamic laboratory with which I am connected is the Langley Memorial Laboratory, not far from Old Point Comfort, Virginia, which has been developed since 1915 by the National Advisory Committee for Aeronautics of the United States. This Committee is an independent Government agency, not under any of the Departments, but reporting directly to the President. We have a laboratory for power plant investigations; a large wind tunnel of the type developed by the N.P.L.; another tunnel in which the air may be compressed to twenty atmospheres or more; excellent facilities for the design and construction of instruments; and a large fleet of aeroplanes equipped for scientific purposes. In addition, we are able to engage the services of competent mathematical physicists familiar with aerodynamics. What we would like to do would be to give free scope to these latter, and to conduct the laboratory tests under their direction, so that theory and knowledge of facts could make progress together. But this is not possible in an establishment whose primary purpose is to give advice to other Governmental services, especially advice concerning questions raised by these services. It is true that we can often inspire these questions, and we can always, in the process of obtaining the answers, learn more than is required for the specific purpose. It follows, that while we are conducting practical tests we are also doing fundamental scientific work continuously, exactly as a justice of a high court expresses his deepest thoughts as *obiter dicta*.

As it has happened, two problems of a general nature have come to us this year from both the Army and the Navy, which, while not new at all, have led to new methods and to new knowledge. Both have an immediate bearing upon the design of aircraft; and it was for these reasons that I selected my rather indefinite title for this lecture.

The first problem stated generally was to learn more about the distribution of forces on the parts of aircraft. It came to us in three questions:—(*a*) How is the distribution of load over a wing tip and aileron modified by changing the plan form of the wing of an aeroplane? (*b*) Why are high-speed pursuit aeroplanes subject to certain types of accident, such as the ripping off of the linen envelope of the wings? (*c*) What are the forces to which the fixed and movable surfaces and the envelope of an airship are subjected when it is making manœuvres?

The first of these led to an extensive investigation in the standard wind tunnel. One series of tests was on four model aerofoils without ailerons, having square, elliptical and positively and negatively raked tips; the second series was on wings having raked tips with ailerons adjusted to different settings. The models had a chord of six inches and a mean semi-span of 18 inches, and the method of images, recommended in one of the British R. and M. reports, was adopted in the investigation. A large number of series of openings were made

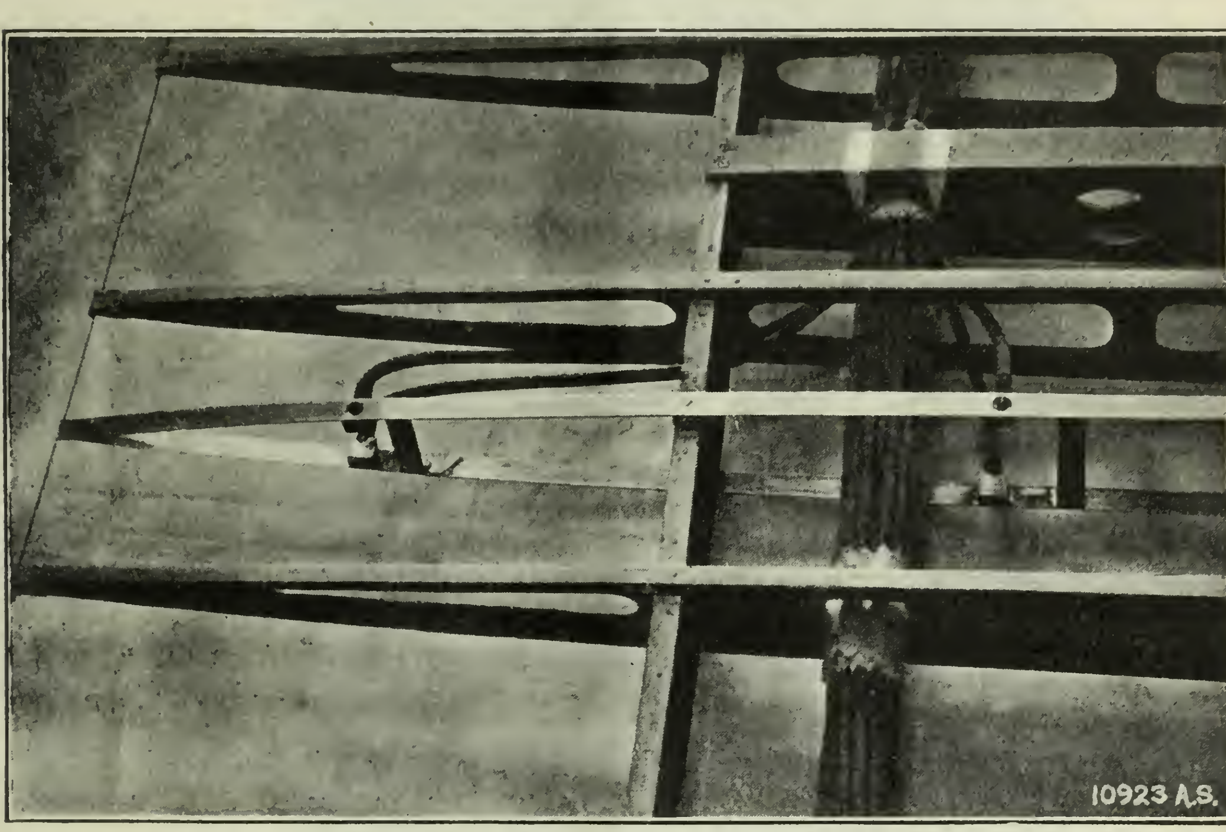


Thomas Morse MB-3 ready for wing and aileron pressure distribution tests.

in the surfaces of the aerofoil, and each was connected to a liquid manometer. The results give a great deal of what is apparently new information concerning the air flow near the tip of a wing. They will soon be published both in tabular and in graphical form, so that designers can calculate with ease the distribution of lift between the ends of the wing spars, the shears and bending moments, and the aileron efficiency. Further, with the knowledge obtained, proper distribution of load in sand testing is facilitated. The most important general conclusions are that tips with a positive rake give an erratic distribution of lift near the tip of the aileron and that this may be avoided by the use of a negative rake. Considerable new light is also thrown upon the question of aileron balance. (Several lantern slides were shown.)

In order to study the air-flow about a high-speed pursuit aeroplane, a Thomas-Morse MB-3 machine was rebuilt and suitably prepared for experimentation. This has a maximum air-speed of 145 m.p.h. A large number of holes were made in the two surfaces of both the upper and lower wings; these were connected by rubber tubes to recording multiple manometers mounted in the fuselage; so in this way sixty records could be made simultaneously.

The manometer, which has been described in published reports of the Committee, consists of a series of metal capsules across the middle of each of which is stretched a metal diaphragm. In most of the tests the two holes facing each other on opposite sides of the wing were connected to the opposite sides of the capsule; but in some cases only one hole was so connected, the other side of the capsule being joined to a reservoir in the cockpit communicating with a static tube whose opening was in the interior of the wing. Special attention was paid to the distribution of pressure in the slipstream and near the leading and trailing edges. Since there is such a great variation in pressure over a wing, each capsule was adjusted separately so as to have the proper sensibility corresponding to the opening with which it was connected. At the leading edge pressures as high as 200 lbs./sq. ft. had to be measured, while further back the pressure often did not exceed 30 lbs./sq. ft. An accelerometer, a recording air-



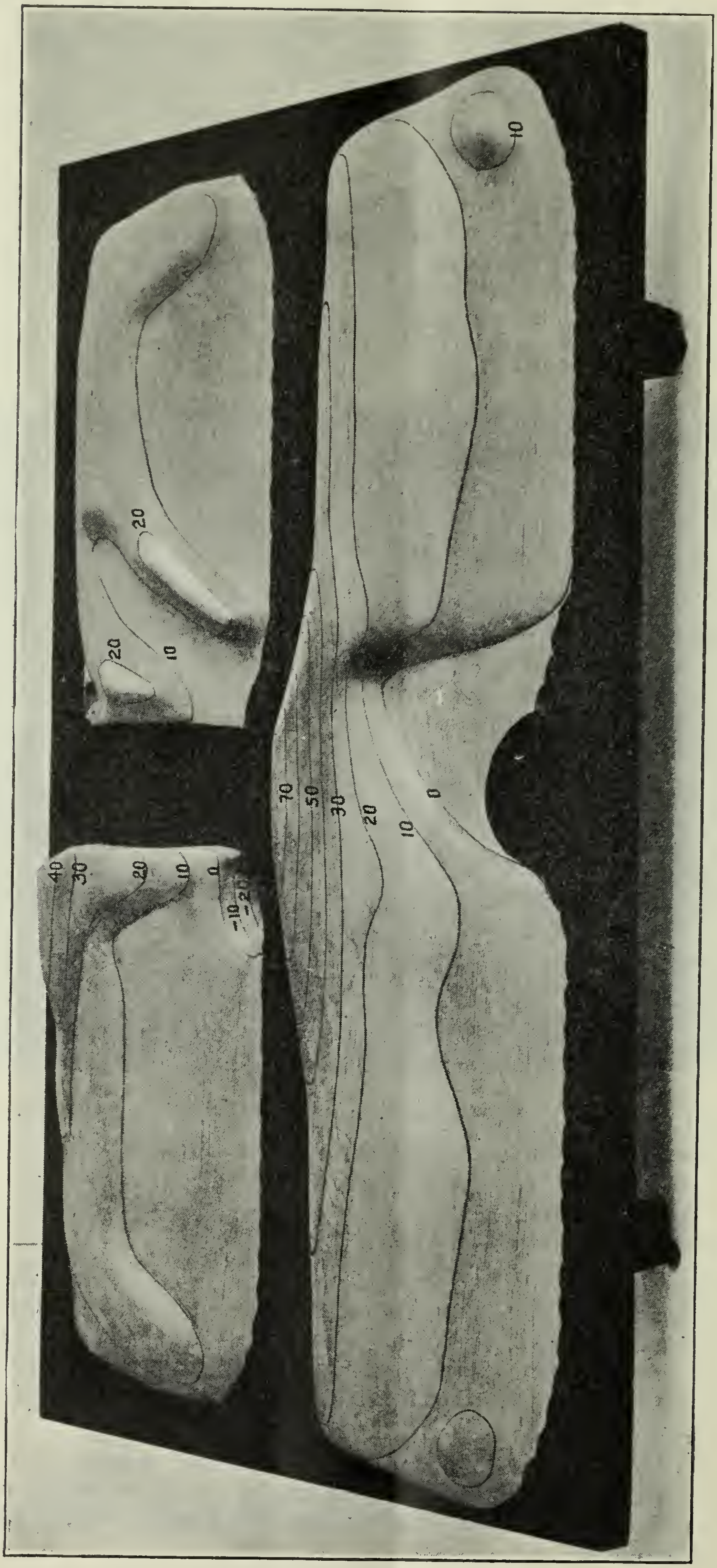
Enlarged view of portion of skeleton wing of MB-3, showing tubes and surface connections for pressure distribution tests.

speed meter, a control position recorder, and an electric chronometer were also installed in the aeroplane.

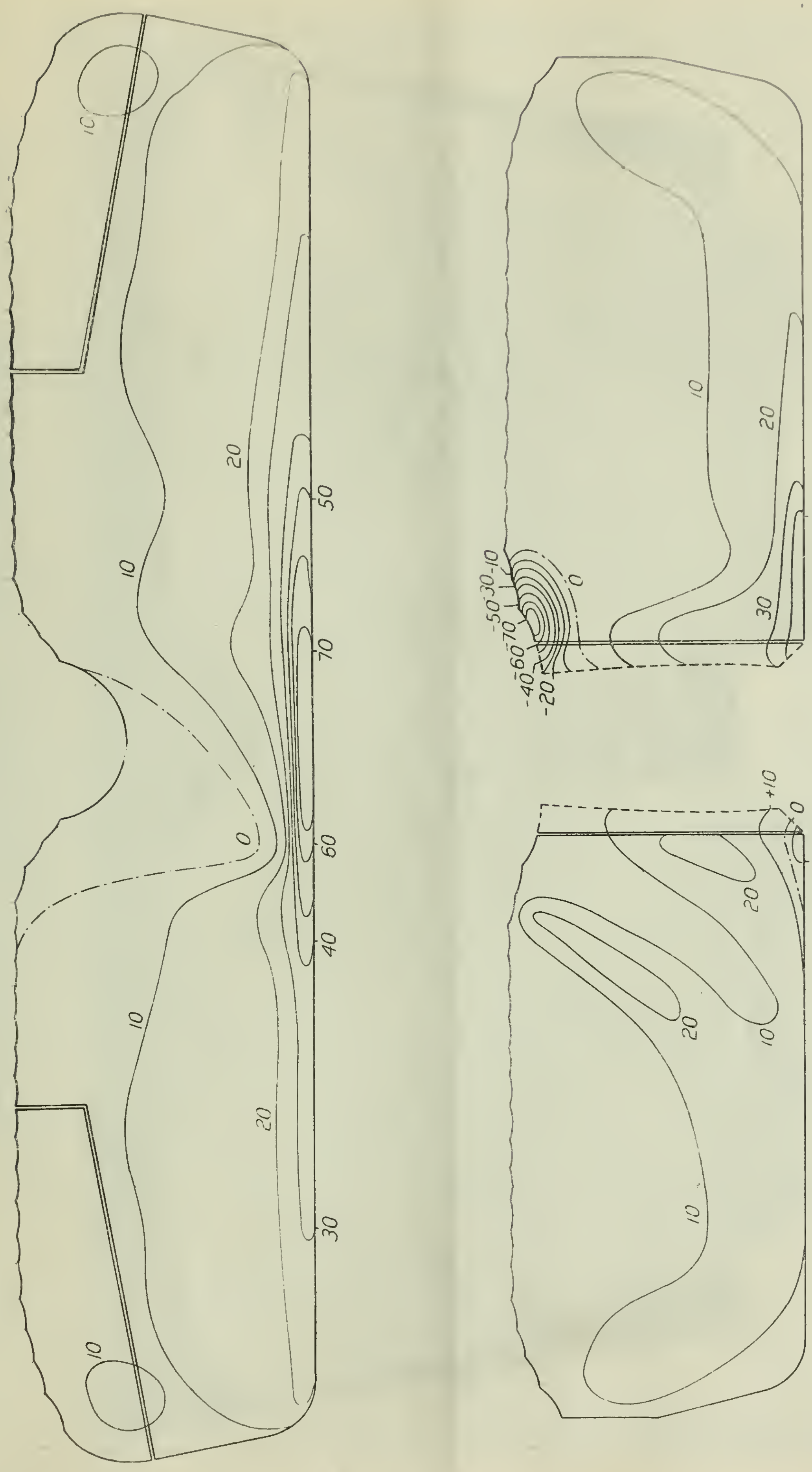
The information specially desired was the distribution of lift over the portions of wings in the slipstream during steady flight and that over the entire wings during violent manœuvres. Measurements were made at air-speeds of 70, 115 and 145 miles per hour at closed, medium and full throttle under conditions of steady flight, and also during three manœuvres, a roll, a flattening out of a dive and a vertical bank at 150 m.p.h.

The result can be understood most easily by the use of graphical methods. Contour lines of pressure may be drawn on a model of the wings; or, what is far more striking, three dimensional models may be constructed. Both these methods are illustrated.

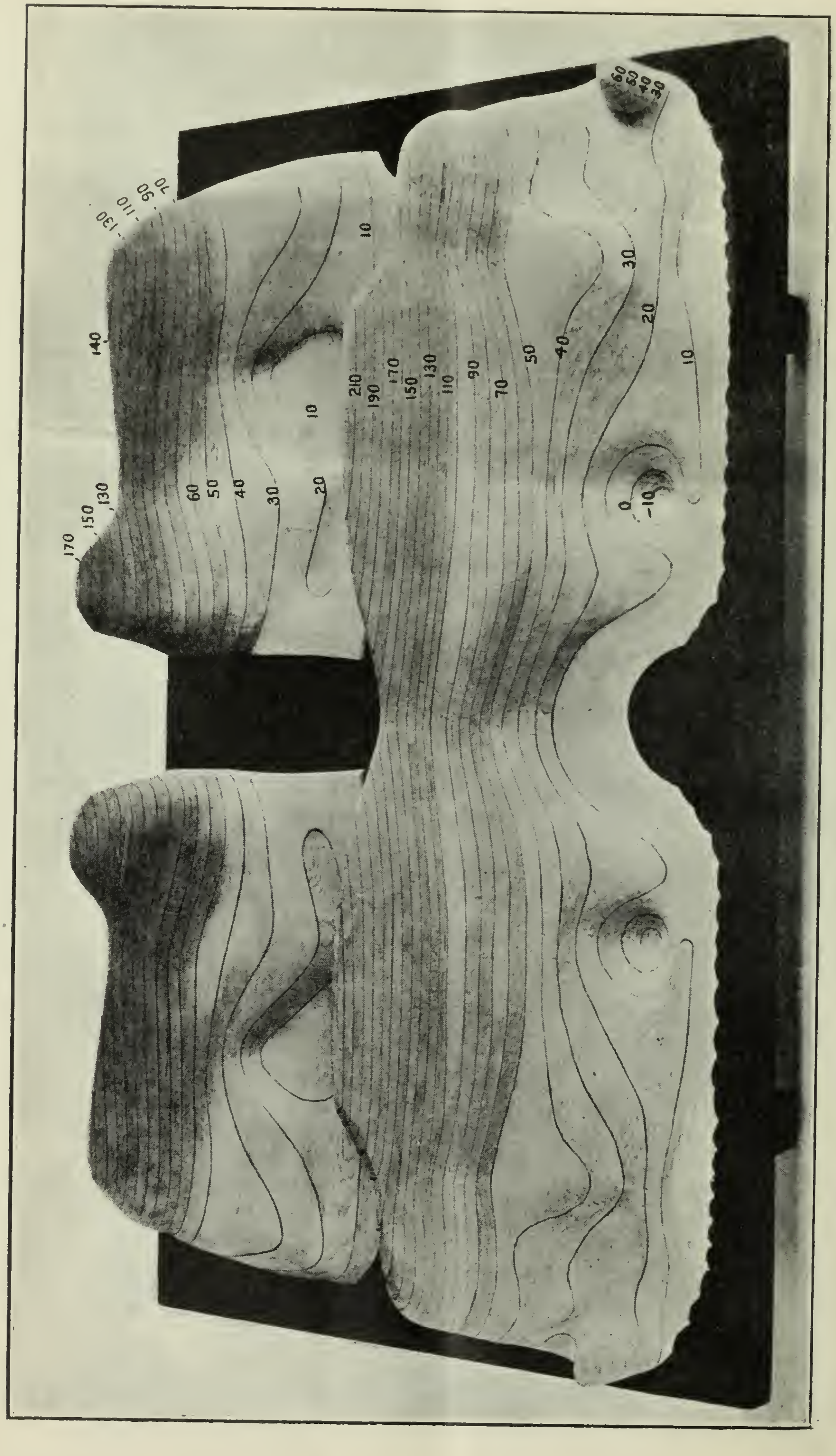
The numbers adjacent to any contour line indicate the total pressure upward



Model in relief, showing lift of MB-3 wings in steady flight at 70 m.p.h. and 1,600 r.p.m.



Lift of wings in steady flight at 70 m.p.h. and 1,600 r.p.m.



Model in relief, showing lift of MB-3 wings in a vertical bank at 150 m.p.h. and 1,900 r.p.m. Acceleration 4.2 g. Elevator pulled up 12°.

in lbs. per square foot, *i.e.*, the combination of the effects on the two sides of the wing. The relief maps also give the combined effects.

Some of the most striking facts observed are :—

1. The lift in the slipstream during steady flight is far from uniform on this aeroplane; at high air-speed and high engine-speed a lift of 100 lbs./sq. ft. was observed on the leading edge of the upper wing, while on the leading edge of the lower right wing there was an area of down pressure of 60 lbs./sq. ft.

2. At low air-speed and high engine-speed, that is while climbing, there was at the trailing edge of the lower left wing, near the fuselage, a down pressure of 70 lbs./sq. ft.

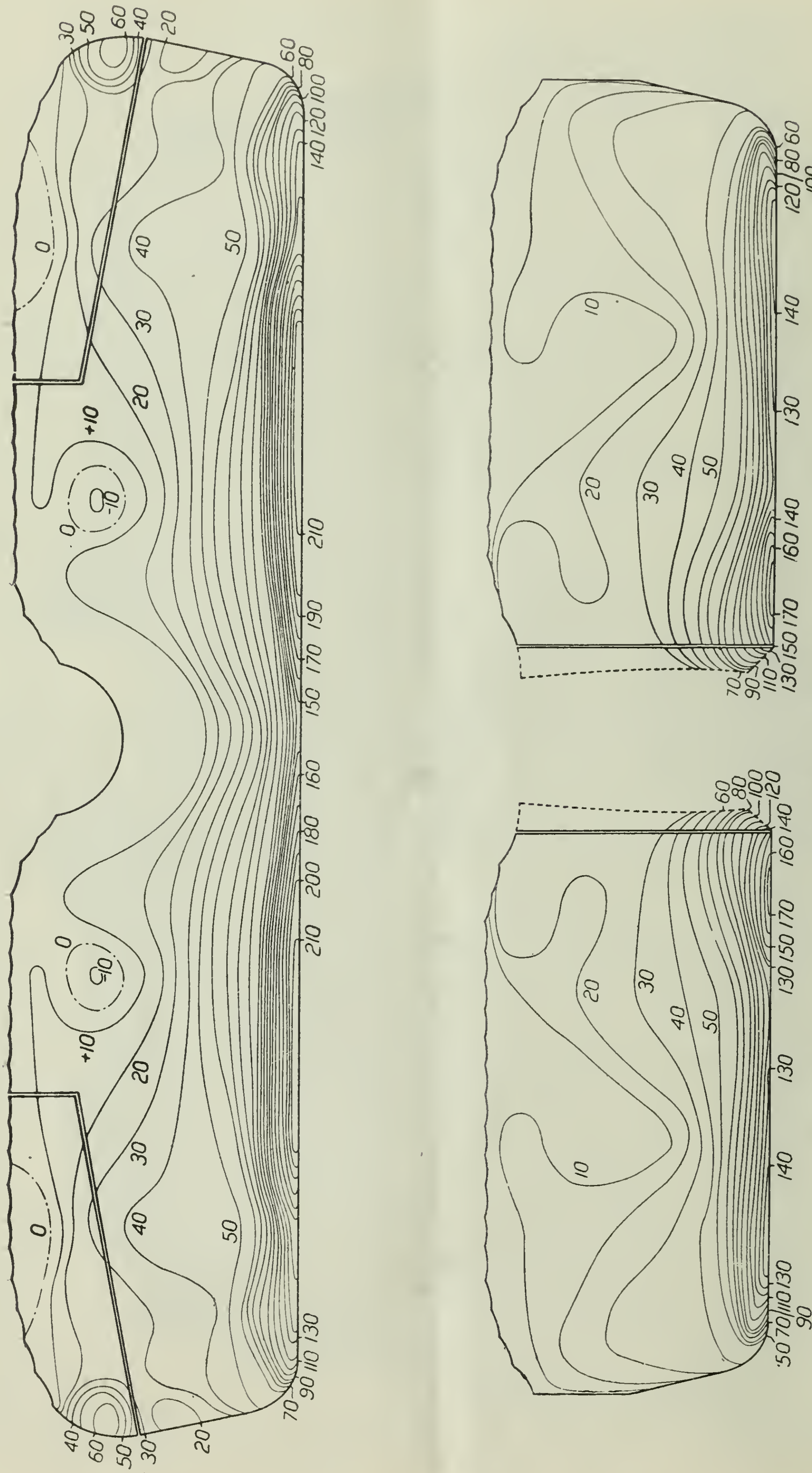
3. When the suction on the upper surface of a wing was measured with reference to the air inside the wing, it was found to amount to as much as 76 lbs./sq. ft. in steady flight, whereas in one isolated point an inward pressure of as much as 24 lbs./sq. ft. was observed.

4. In flattening out of a dive the wings support only 80 per cent. of the total load on the aeroplane, whereas in a vertically banked turn at 150 m.p.h., where the acceleration rose to 4.2 g. the wings carried 90 per cent. of the load, the remainder being borne by the fuselage and tail surfaces.

5. In steady flight at 145 m.p.h. the lift per sq. ft. of the upper wing is twice that of the lower, the total lift of both wings being about 400 lbs. greater than the weight of the aeroplane, balancing the down load on the fuselage and tail. This fact is, no doubt, due to the rigging of this particular aeroplane, *i.e.*, to the angular difference between the wings and to the lower wing being almost at zero lift.

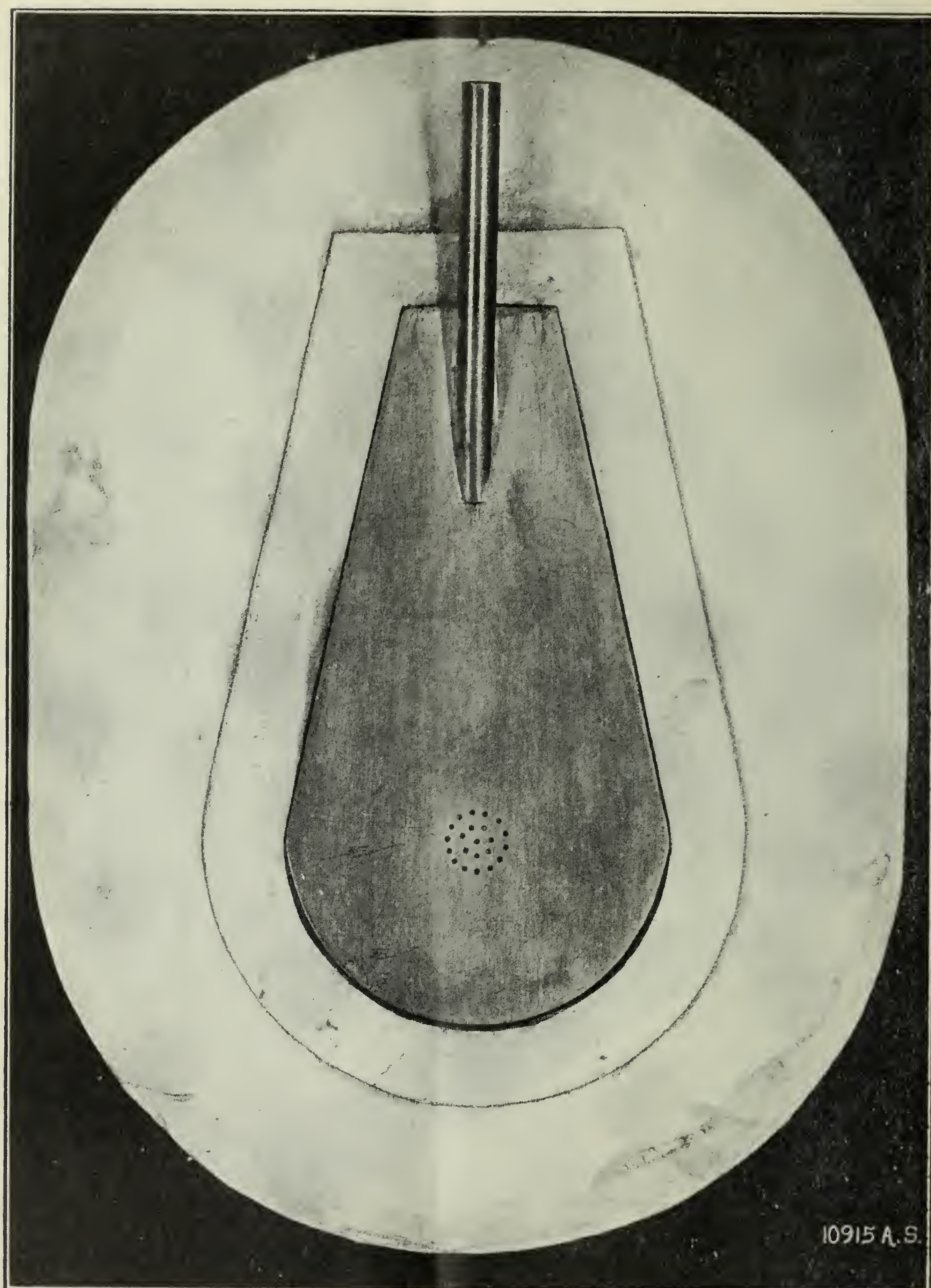
It is important to add that this MB-3 machine is a single-seater, so that the pilot has to control the machine and press the button which starts all the automatic recording devices. This investigation of the MB-3 proved so interesting and offered so many suggestions that further studies of pursuit aeroplanes have been called for; the plans are now perfected for similar investigations of the latest types of military fighting aeroplanes. One problem in this connection is to compare the inherent advantages and disadvantages of monoplane and biplane machines.

As is well known, the United States is interested in the construction of airships. The Navy has practically finished a large rigid, and the Army has well under way a semi-rigid. As is equally well known, the actual scientific knowledge of the aerodynamics of airships is not extensive. At the request first of the Navy and later of the Army, our National Advisory Committee undertook to study and report upon the airship designs made by these two services. In connection with this work one of the technical staff of the Committee, Dr. Munk, elaborated a certain theory of the airship which was distinctly novel but led to results at variance with accepted practice. It was evident that real knowledge could be obtained only by extensive experimentation on actual airships. What was needed primarily was a series of measurements of pressures over the envelope and surfaces of an airship when in steady flight and when making manœuvres. For this purpose a non-rigid airship, Navy type C, was placed at the disposal of the Committee. It is 200ft. long, 40ft. in diameter, and has 200,000 cubic feet capacity. Pads were specially designed for the measurement of pressure. These lie practically flush with the envelope of the airship, and each consists essentially of a metal box whose top and bottom surfaces are pear-shaped, roughly 2ins. by 4ins., and held a distance of one-hundredth of an inch apart by means of studs; in the top plate there are grouped in a comparatively small circle 22 holes each three-hundredths of an inch in diameter; a brass tube $\frac{1}{4}$in. in diameter serves as an outlet from the box. This is connected by rubber or aluminium tubing to a liquid manometer in the car of the airship.



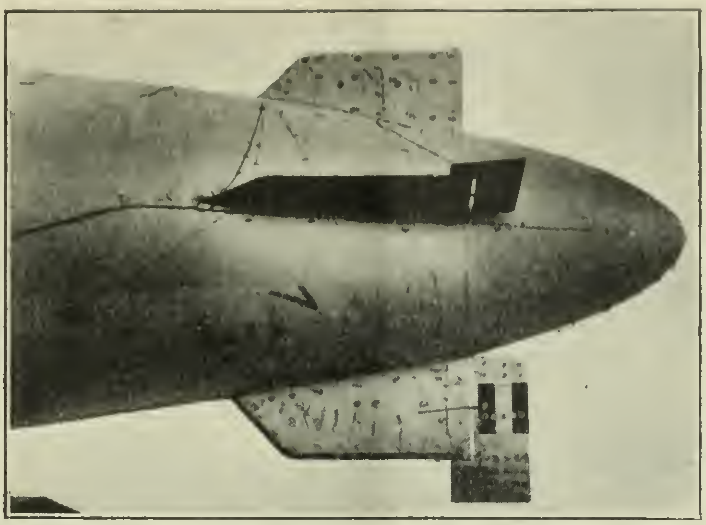
Lift of wings in a vertical bank at 150 m.p.h. and 1,900 r.p.m.
Acceleration 4.2 g. Elevator pulled up 12°.

Lift of MB-3 wings in a vertical bank at 150 m.p.h. and 1900 r.p.m. Acceleration 4.2 g. Elevator pulled up at 12°.



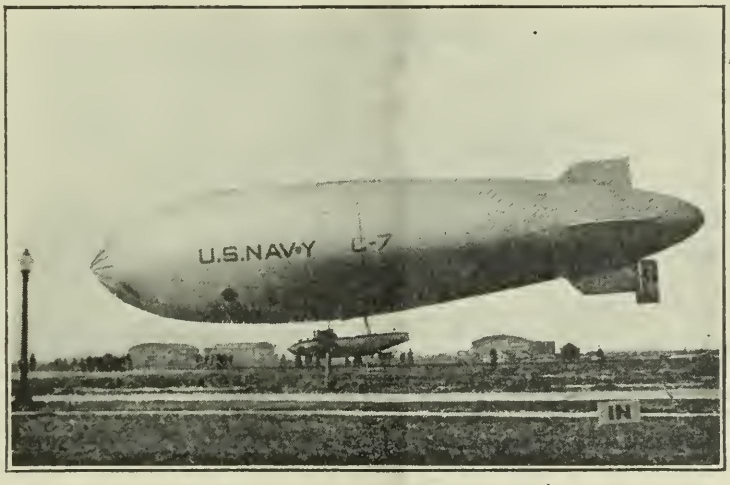
Pressure pad for use on airships.

There are about 400 of these pads on the envelope and surfaces of the airship, thirty-six being in the bottom fin and rudder. Simultaneous reading of 260 manometers may be made photographically.



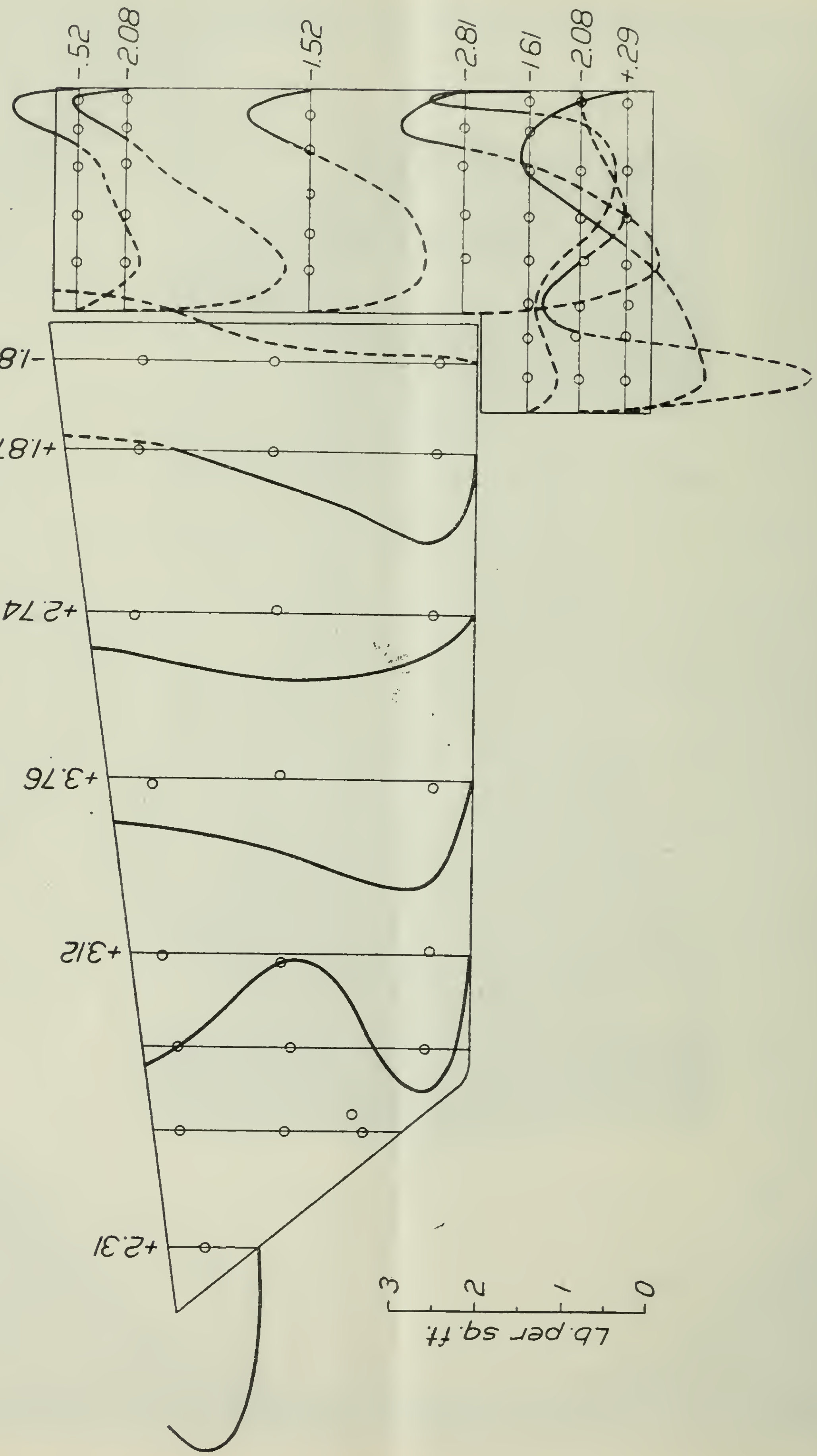
Control surfaces of airship, showing location of pressure pads.

This investigation of the aerodynamics of an airship is not yet completed, but I can show you certain observations which indicate the importance and novel character of the results being obtained. One illustration shows the pressure distribution over the bottom fin and rudder in circling flight, and the other when the airship while in steady flight has its helm put hard down.

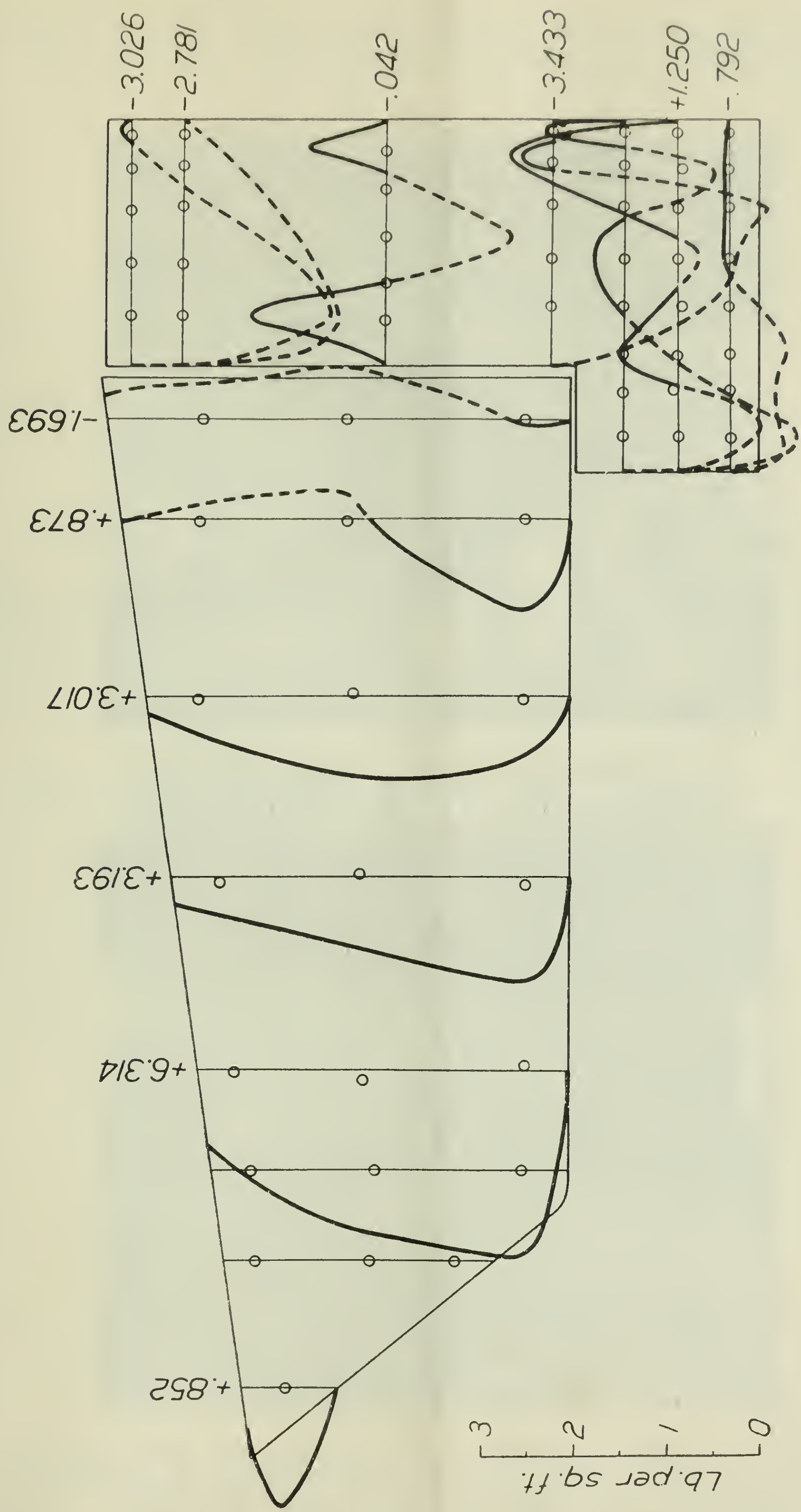


" C " class airship on which the pressure distribution work was done.

The drawings do not require much explanation, but emphasis may be placed upon the results shown when circling flight is begun. When the helm is suddenly applied, and before the airship attains an appreciable angular velocity, the angular acceleration creates such a large force on the vertical fins in the opposite direction to the force on the rudder that the net force on the stern of the airship is much smaller than has been supposed hitherto. It follows that the condition of the sudden application of the rudder is not a serious one from the point of view of the stresses in the hull of the airship. Presumably the reversal of the helm, when the airship is in a steady turn, does not cause a large increase of the bending moments beyond those already existing in that condition.

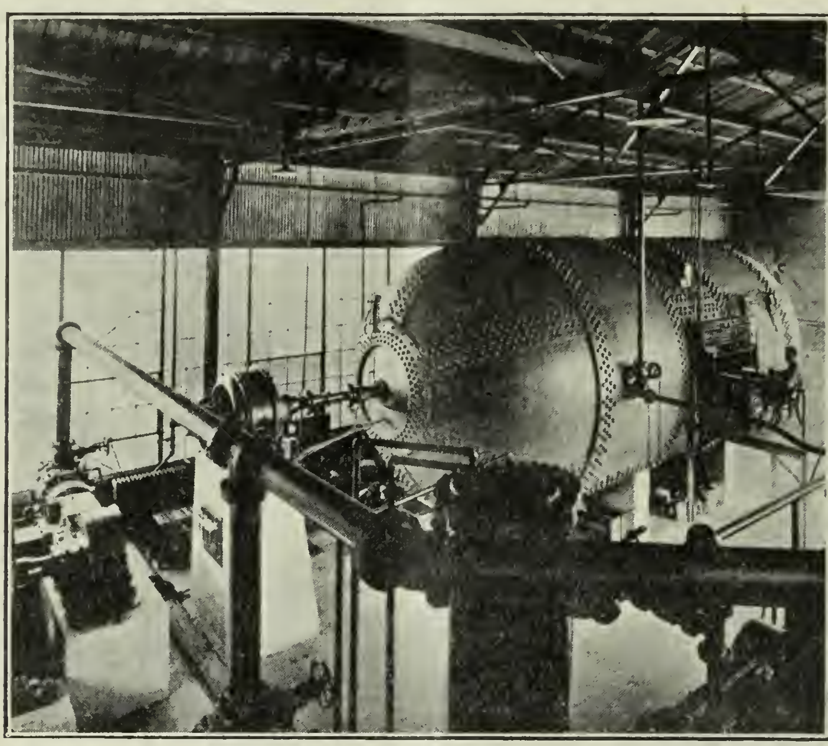


Pressure distribution over bottom fin and rudder. Circling flight.

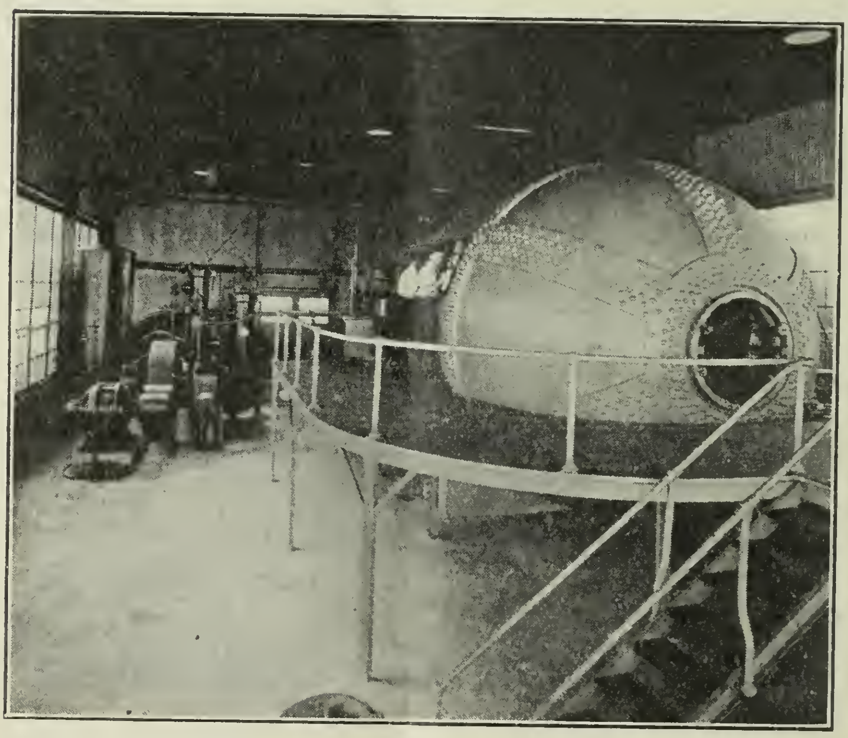


Pressure distribution over bottom fin and rudder.
Beginning of circling flight.

There are the three problems referred to at the beginning of this paper as requiring an elaboration of the methods for the study of pressure distribution; and no one can question the importance of the results obtained in the proper design of aircraft.



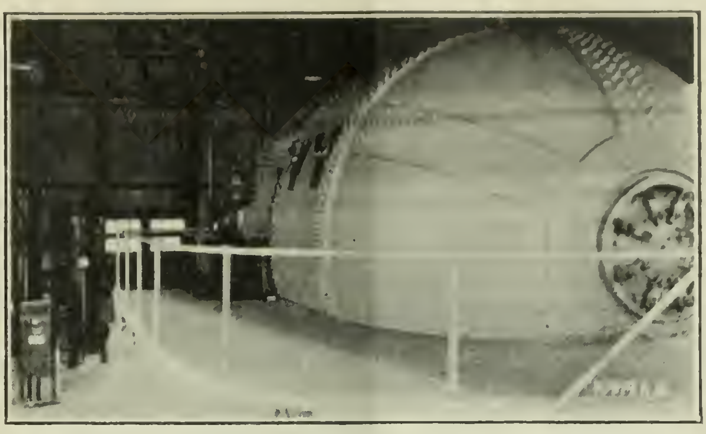
View of compressed air wind tunnel, showing fan motor and air pipes.



Compressed air wind tunnel with observation platform and compressor.

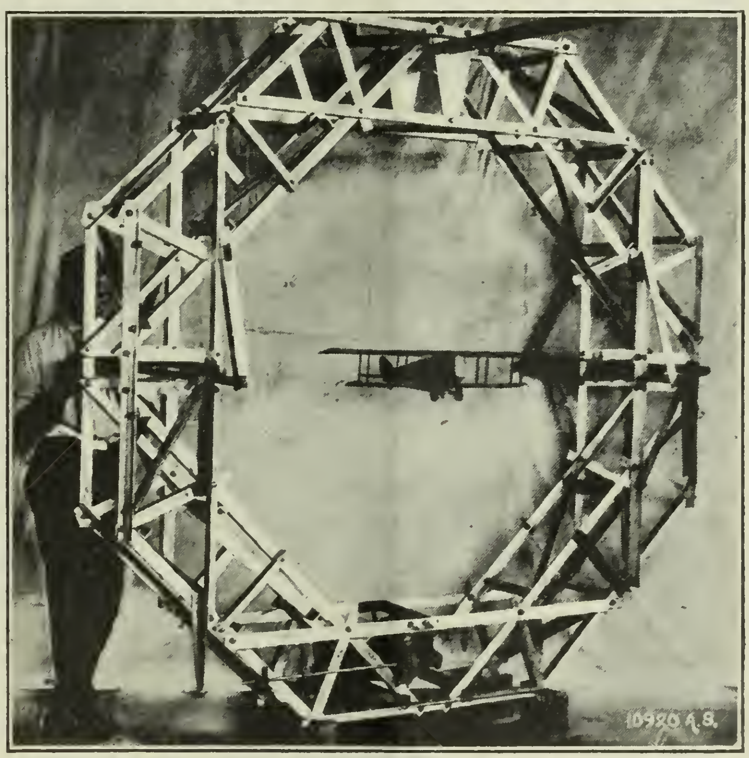
Quite a different set of questions has been asked our Committee, which lead in the end to an investigation of the so-called scale effect. Certain questions can,

of course, be answered on theoretical grounds, and answered definitely; but the great majority cannot. Any aircraft is a complicated mechanism made up of many parts; all of these have definite aerodynamical characteristics; but from a knowledge of these we cannot pass to that of the machine as a whole. The



Wind tunnel No. 3, showing observation platform and desk.

question as to the changes in forces and moments with scale, especially in manœuvres, is exceedingly difficult. The first investigation which should be made on scale effect is to determine which aerodynamic properties are most susceptible to the effect; after that, the number of problems to be undertaken is practically infinite.



Balance for wind tunnel No. 3.

At Langley Field our Committee has facilities for studying scale effect by four different methods, two of which are, I believe, unique. We have an ordinary wind tunnel, having a 5ft. throat and fitted with fans so that an air-speed of 100 m.p.h. (147 feet per second) may be used; this gives a certain Reynolds number, not very large. A larger number may be obtained by a free flight method

in which a large model is suspended below an aeroplane in steady flight; we have perfected methods for suspension and measurement, and the results are, on the whole, satisfactory. To secure a still larger Reynolds number, the Committee has had constructed during the past year a wind tunnel to operate with air compressed to 20 atmospheres or more. The tunnel proper is 5ft. in diameter at the experimental chamber, and is enclosed in a cylindrical tank with hemispherical ends. The walls of the tunnel are hollow, providing an annular dead air space in which the balance mechanism is installed. This may be controlled automatically, or settings may be made by small electric motors, operated from outside, which attach or release heavy balancing weights by means of cams, or shift lighter weights along balance arms. The model is attached to the balance by wires, there being three balance arms for measuring lift, drag and pitching moments. The tank is 35ft. long and 15ft. in diameter, and weighs 83 tons. It is mounted on a concrete foundation and is partially surrounded by a working platform. An observer on this makes settings and readings by looking into the tank through small glass windows. The density of the air in the tank is controlled by two compressors driven by electric motors. Continuous stages may be secured from one-tenth of an atmosphere to twenty atmospheres. Circulation of air is effected by a two-blade propeller of special design, 7ft. in diameter and driven at 900 r.p.m. by a 250 h.p. synchronous motor mounted on a separate foundation outside the tank. The drive shaft is made tight against air leakage where it passes through the head of the tank by a loosely packed gland, through which oil is circulated.

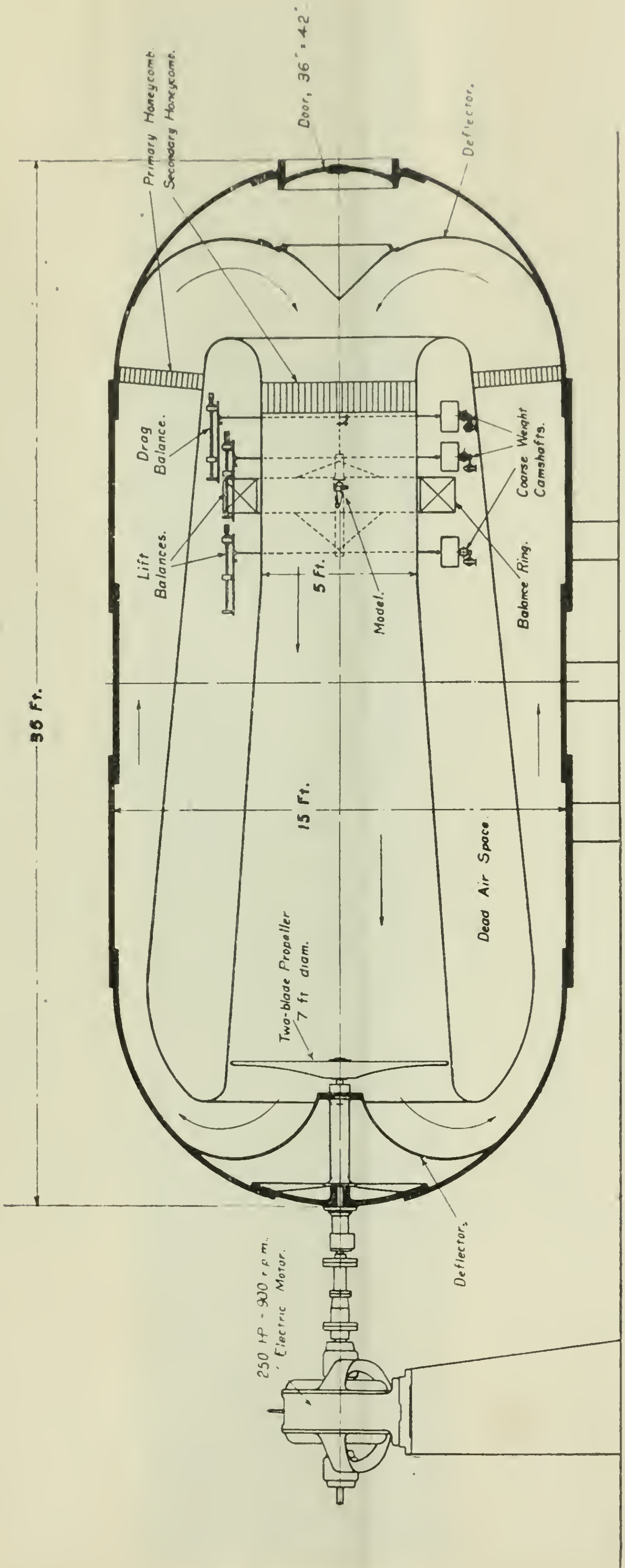
The concept of such a tunnel was originated by Dr. Max M. Munk, and this particular one was designed by him; and the mechanical equipment was designed and installed by Mr. D. L. Bacon, both members of the Staff of the Committee. The latter is in charge of the operation of this tunnel as well as of other tunnels in the Committee's laboratory.

It may be of interest to note that when the tunnel is operating at its greatest density, it is equivalent in scale to a tunnel 100ft. in diameter running at 60 miles per hour. It takes about an hour and a half to "inflate" the tank fully.

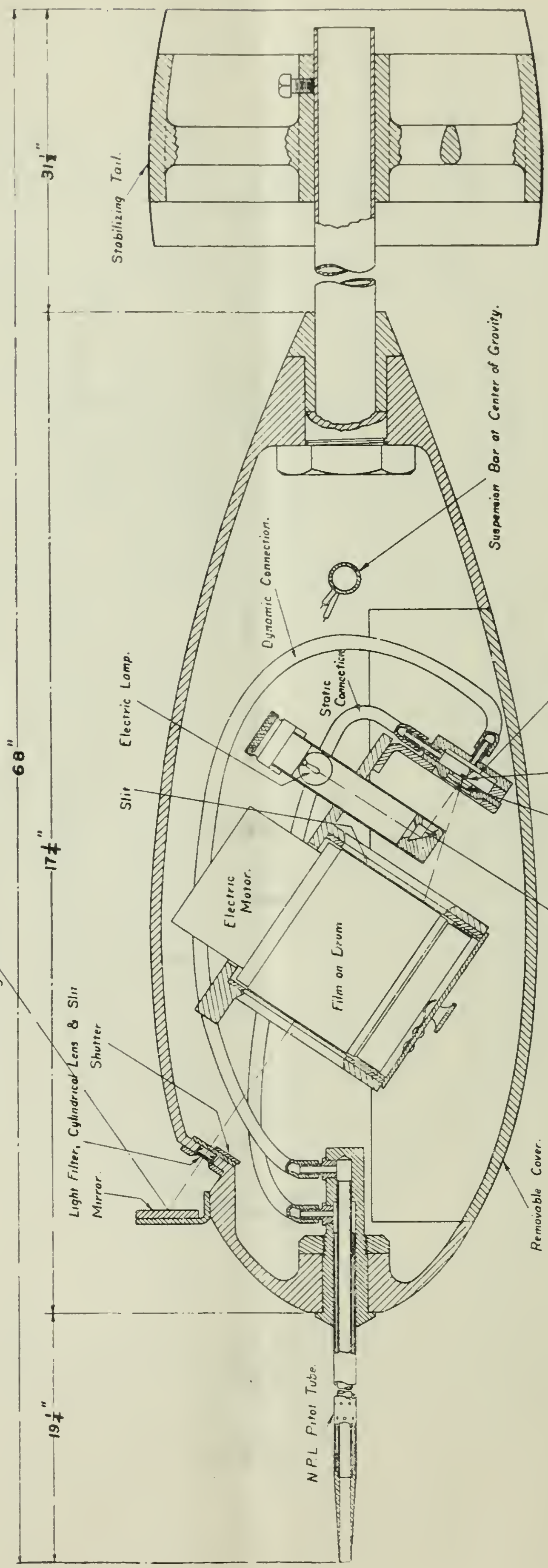
Another method for obtaining a large Reynolds number, which is used by the Committee, involves the accurate measurements of the motion of an actual aeroplane in flight. To this end the Staff of the Committee have perfected a large number of recording instruments. Among these may be mentioned a single-component accelerometer; a three-component accelerometer; a three-component angular velocity recorder; a control-position recorder; a control-force recorder; an air-speed meter; an angle of attack recorder, and an electric chronometer. The Committee owes the design of these instruments to the exceptional ability of two of its staff, Mr. F. H. Norton and Mr. H. J. E. Reid.

The latest instrument developed and one used in work about which I shall speak later is a form of kymograph.

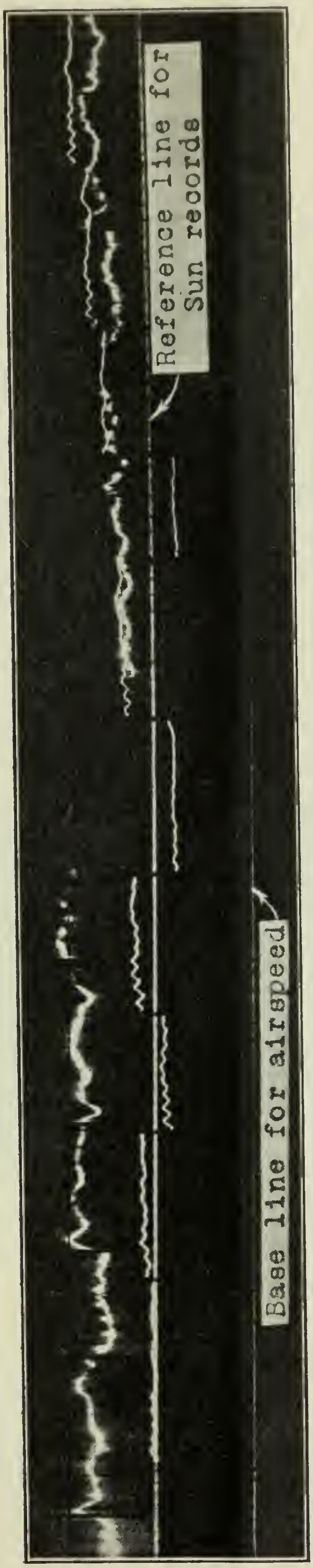
It consists of a streamlined body, shaped like a bomb, from the front end of which projects a N.P.L. pitot tube, and which has a tail appendage to render the whole directionally stable. There is a transverse shaft through the centre of mass, to the two ends of which are attached suspension wires leading to winches in the cockpit of the aeroplane, so that when the latter is in flight the kymograph may be lowered to a distance of 25 feet so as to be in undisturbed air. In the upper forward surface of the "bomb" there is an opening closed with a cylindrical lens, outside of which is a small vertical mirror, so that the rays of light from the sun may be reflected through the lens and then through two crossed slits on to a photographic film. The pitot tube is connected to a capsule manometer, whose motions are recorded on the same film. This is wound on a drum, inside of which is a constant speed electric motor driven by a current led in through the suspension wires of the instrument. An actual photograph of the records on the drum is given.



N.A.C.A. variable density wind tunnel.

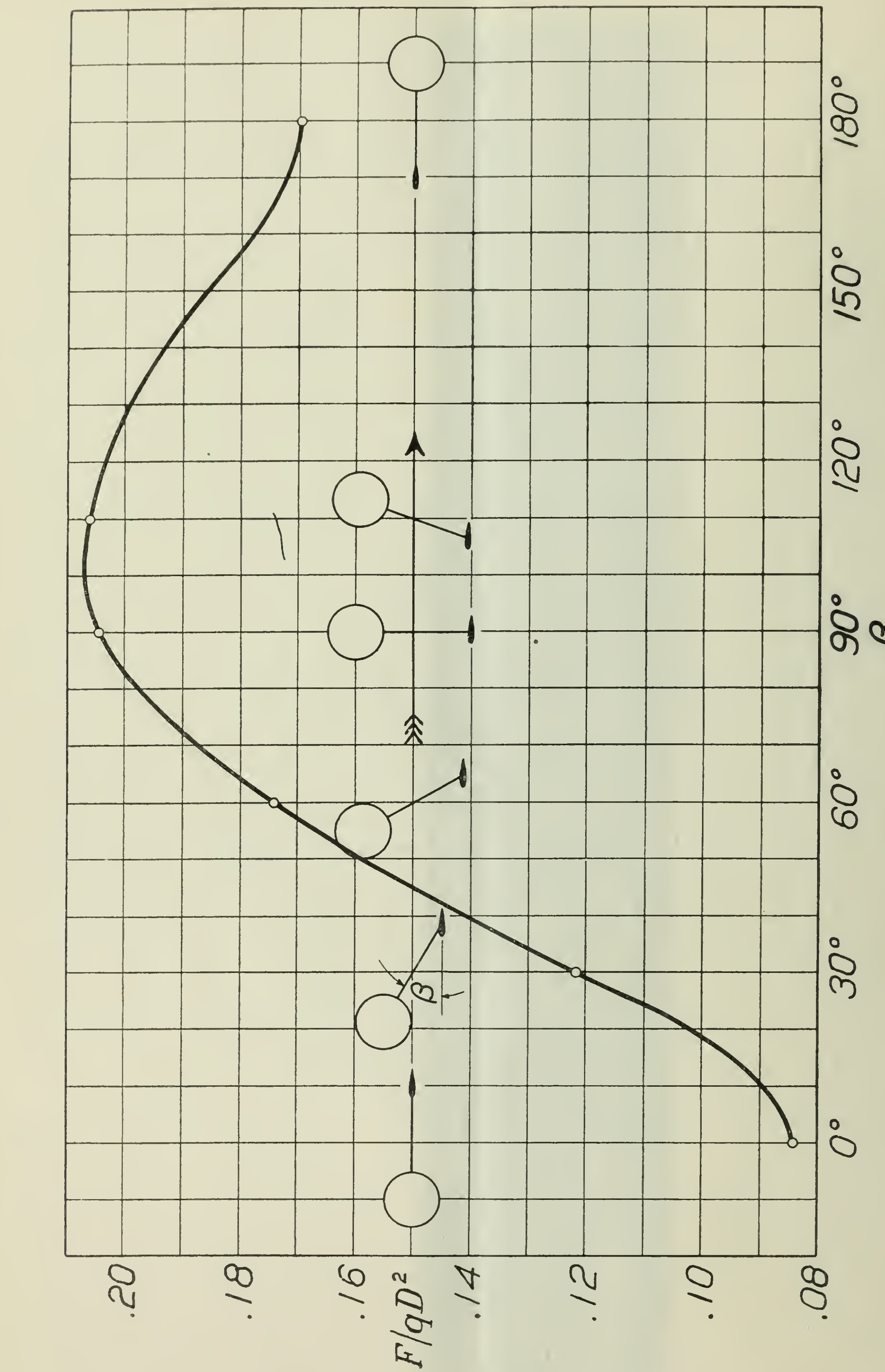


N.A.C.A. trailing Kymograph-airspeed meter.



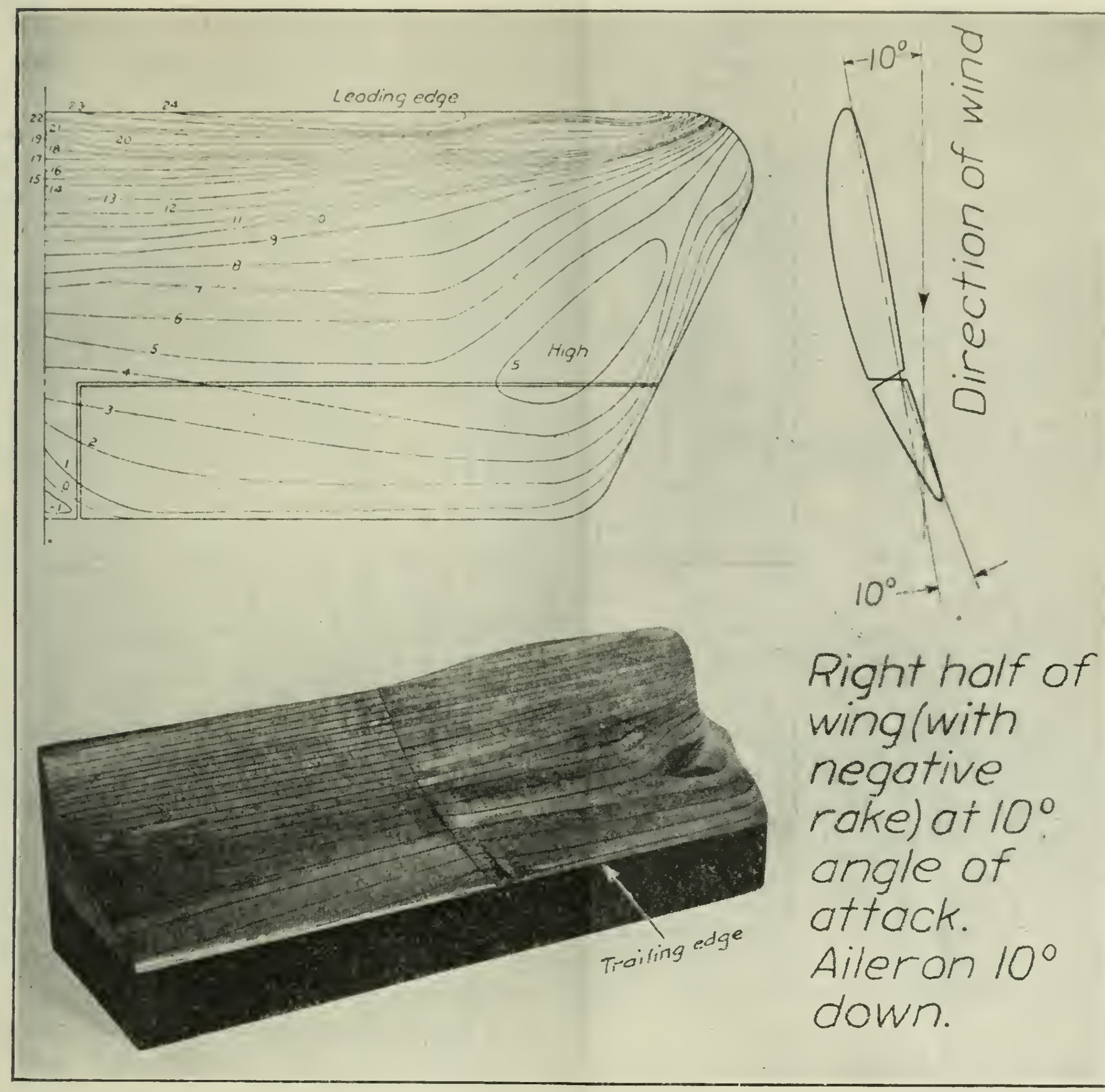
Kymograph Record.

Airspeed records are the sharp curves. Sun records are broadened.



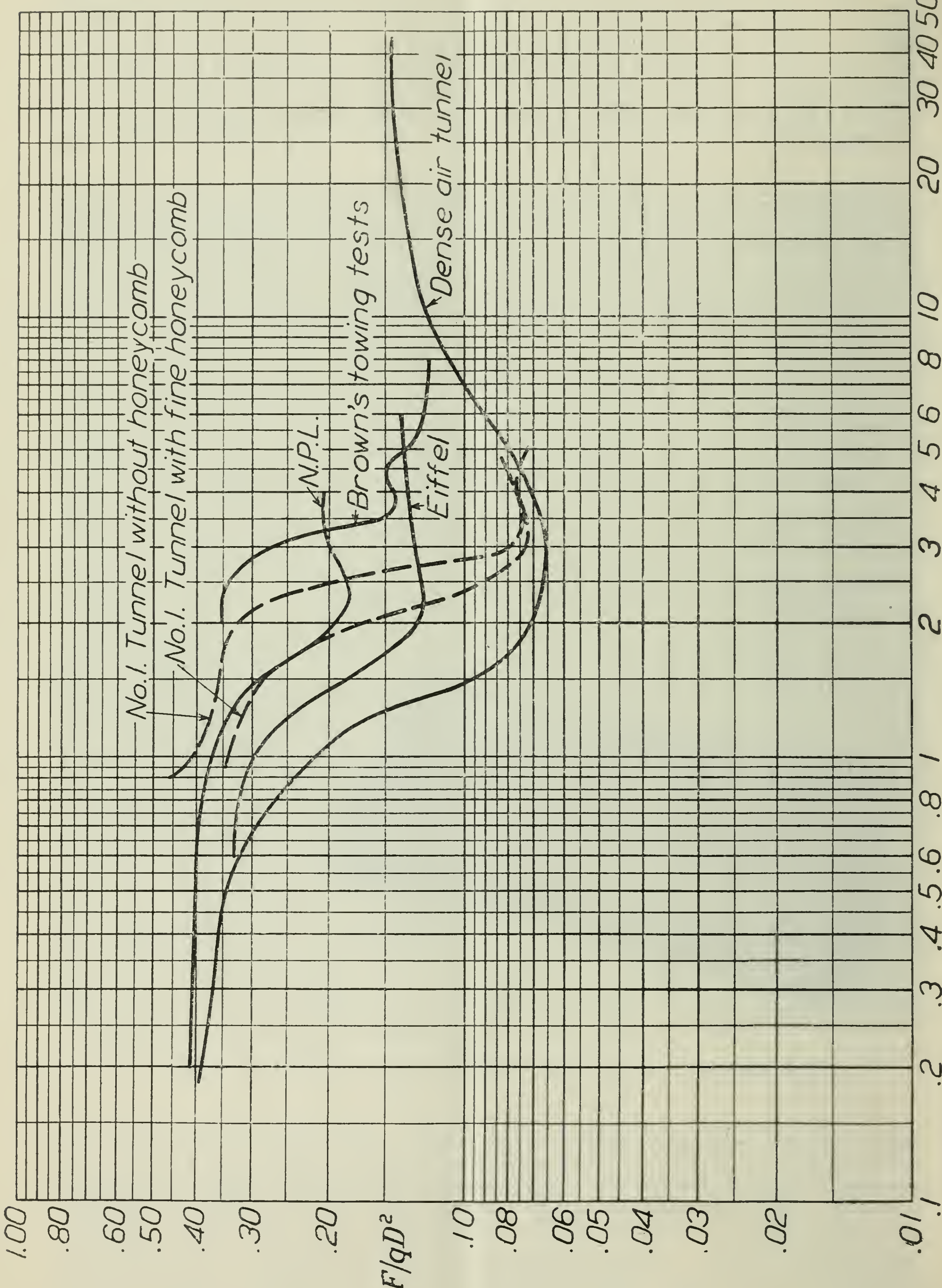
Effect on resistance of sphere of direction of support with reference to airstream.
Reynolds Number is 1.5 × 10^5.

When the aeroplane is flown in a direction away from the sun, the kymograph takes a position along the direction of the relative wind, and a continuous record will be made of the angular position of the sun with reference to this direction. An observer on the ground observes simultaneously the altitude of the sun; and so one obtains a record of the angle between the flight path with reference to the air and a horizontal line. The air-speed is measured at the same time, as is also the angle of attack of the aeroplane itself. Therefore, if gliding flights are

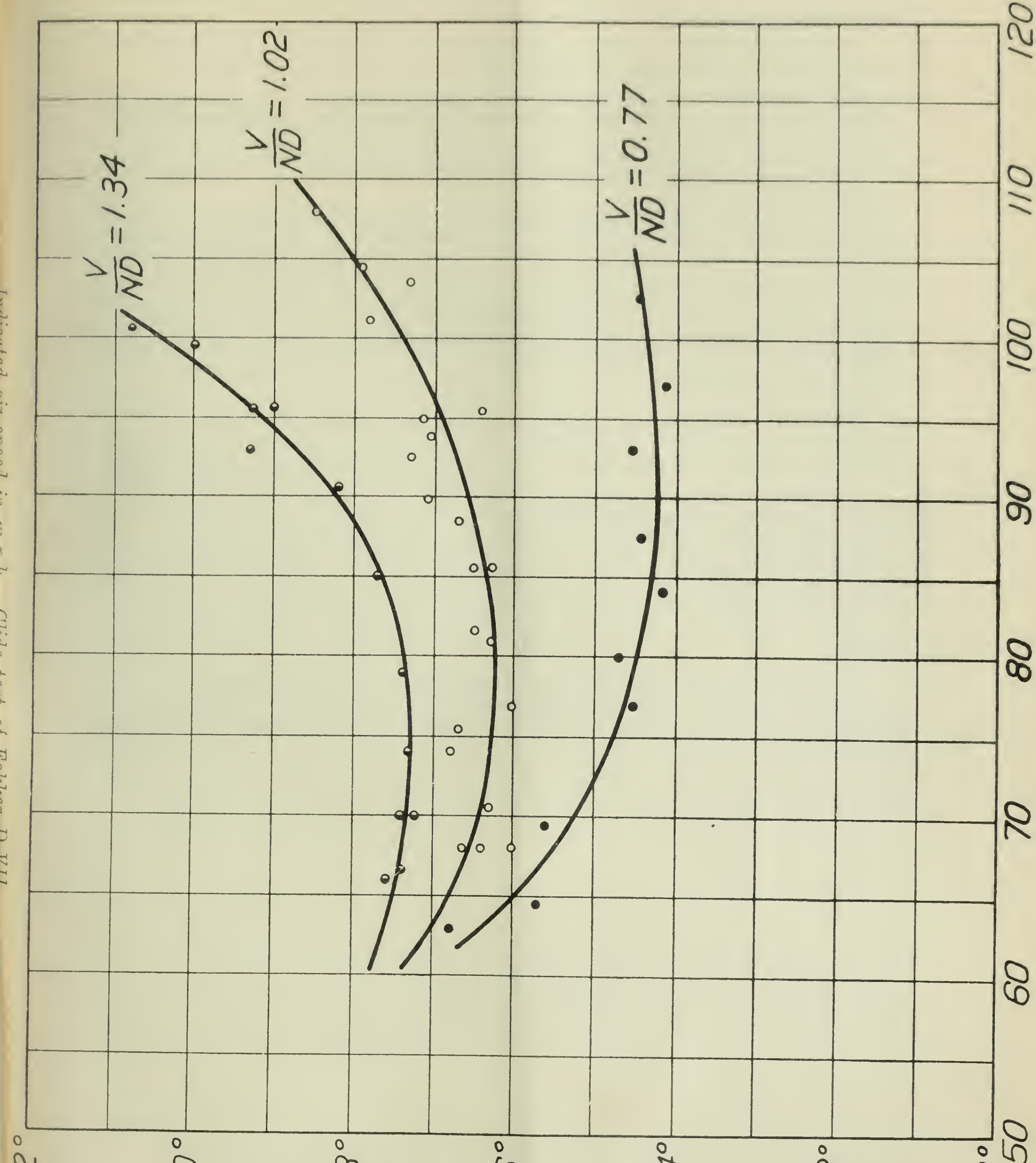


Plan and elevation showing contour lines and built-up model.

taken, values of the ratio of lift to drag may be measured at various angles of attack at known air-speeds. This method is obviously independent of vertical air currents. As an illustration of its accuracy, a chart is shown giving the values of angle of glide with reference to air-speed at different values of V/ND in which V is the air-speed, N is the number of revolutions per second of the propeller, and D its diameter. By a preliminary model investigation it was found that the value of V/ND was 1.02 for the condition of zero torque. These, and all other "free flight" tests under the direction of the Committtee, have been carried



Curves giving resistance of a sphere at different Reynolds Numbers. Reynolds Numbers $\times 10^{-5}$.



Indicated air speed in m.p.h. Glide test of Fokker D-VII.

out by Mr. F. H. Norton and Mr. W. G. Brown with the aid of the Committee's most skilful test pilot, Mr. Thomas Carroll.

With these facilities at the Langley Memorial Laboratory, it is hoped that rapid progress will be made in the elucidation of the scale effect problem.

Unfortunately for the purposes of this paper, the compressed air tunnel was actually put into daily operation for observation purposes only about the first week in April, and so I can report the results of only two series of tests. For this reason, although I have no cause to question their accuracy, they should, I think, be regarded as provisional.

The first scale effect measurements undertaken were on spheres. There is nothing novel in this problem, but some of the results are interesting. Spheres of various sizes were studied in the two tunnels, with their supporting spindles in the direction of the airstream and at various angles to it; other spheres were towed suspended at a considerable distance below an aeroplane in flight; and finally certain spheres were taken aloft by an aeroplane on particularly quiet days and allowed to drop, their motion being determined by theodolite observations from the ground. The results of all of these methods are given on the accompanying diagram.

This test was undertaken both to obtain large Reynolds numbers and to investigate the condition of turbulence in the new wind tunnel. If time were available, I would call attention to several interesting features of these curves.

The second test on the subject of scale effect was made with reference to a type of aeroplane using thick wings and having small parasite resistance. A Fokker D-7 was selected for this purpose. An aeroplane was equipped with suitable apparatus, and a model of one-fifteenth scale was made which was fitted with its proper propeller. Series of measurements on models and in full flight have been made; the aerodynamic characteristics of lift and drag were measured at different attitudes, and the results obtained are shown in the accompanying diagram.

If the use of these scale effect methods justifies our present hopes, we shall be able in a comparatively short time to place at the disposal of the designer of aircraft a wealth of information which should increase markedly the accuracy of his work.

The Journal

OF THE

Royal Aeronautical Society

(Founded 1897 in succession to the Annual Reports)

Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

No. 153 SEPTEMBER, 1923 Vol. XXVII

NOTICES

Associate Fellowship Examination

The second examination for Associate Fellowship will take place in the Library on Monday, September 24th (Part I.), and Tuesday, September 25th (Part II.).

Binding Cases for the Journal

The arrangements made for the binding of complete sets of the journal in blue cloth cases with gilt lettering at a charge of 4/6 per volume, including the supply of the case, are still in force. Members who desire to take advantage of this arrangement should forward their sets direct to the Lewes Press, Ltd., High Street, Lewes, at the same time sending a remittance for 4/6 to the Secretary at the Society's offices. A note stating the name and address of the sender should be included in the parcel to the binders. The complete volume will be returned direct to members postage paid.

Opening Meeting

The inaugural meeting of the Fifty-Ninth Session of the Society's lectures will take place on Thursday, October 4th, at 5.30 p.m., at the Royal Society of Arts, 18, John Street, Adelphi, W.C.2, when Mr. A. Ogilvie, C.B.E., who becomes Chairman of Council on October 1st for the 1923-24, will read a paper on "Gliders and Light-Planes."

Advance Proofs

Advance proofs of all lectures of the coming session will be obtainable on payment of 6d. each lecture or 5/- for the series of 13. A complete list of the lectures together with an order form will be found among the advertisement pages.

W. Lockwood Marsh, *Secretary*.

METAL AEROPLANE CONSTRUCTION*

BY PROFESSOR HUGO JUNKERS.

Before entering upon the subject of my lecture I beg to express my feelings of gratitude for the honour and appreciation of my efforts, implied in the invitation to speak on metal aeroplane construction before so eminent and learned a body as the Royal Aeronautical Society.

In consideration of the general political situation I deem myself entitled to attribute to this invitation the deeper meaning of a token of amiable disposition given apart and beyond my person, from nation to nation, and to see in it an effort of renewing the ties of a genuine humanity, which is well aware of the unavoidableness of conflict and fight, but does not know hatred, and desires to extinguish the sad traces of a devastating war by hoisting the flag of peaceful competition.

I beg of you to be convinced, ladies and gentlemen, that I am particularly

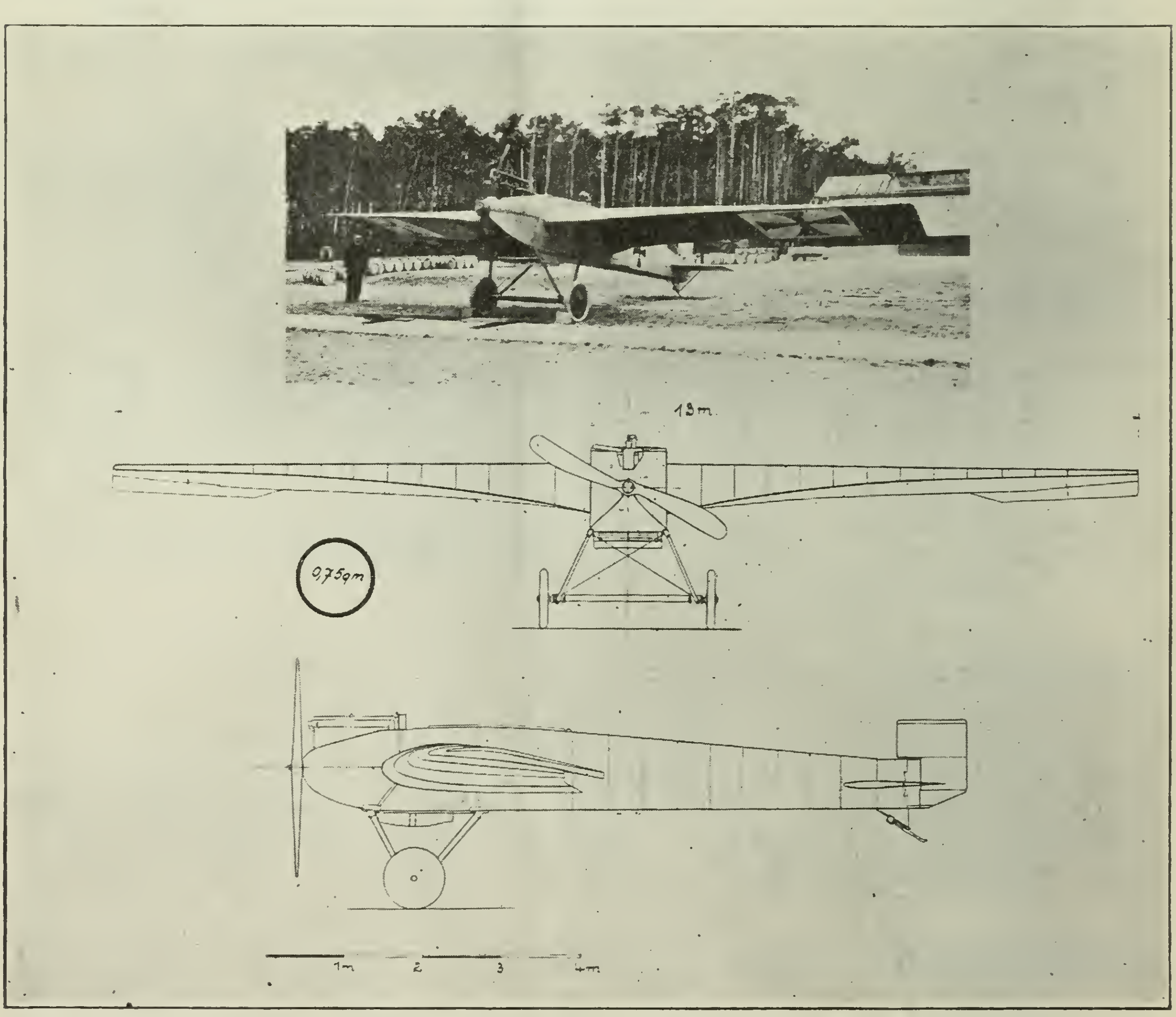
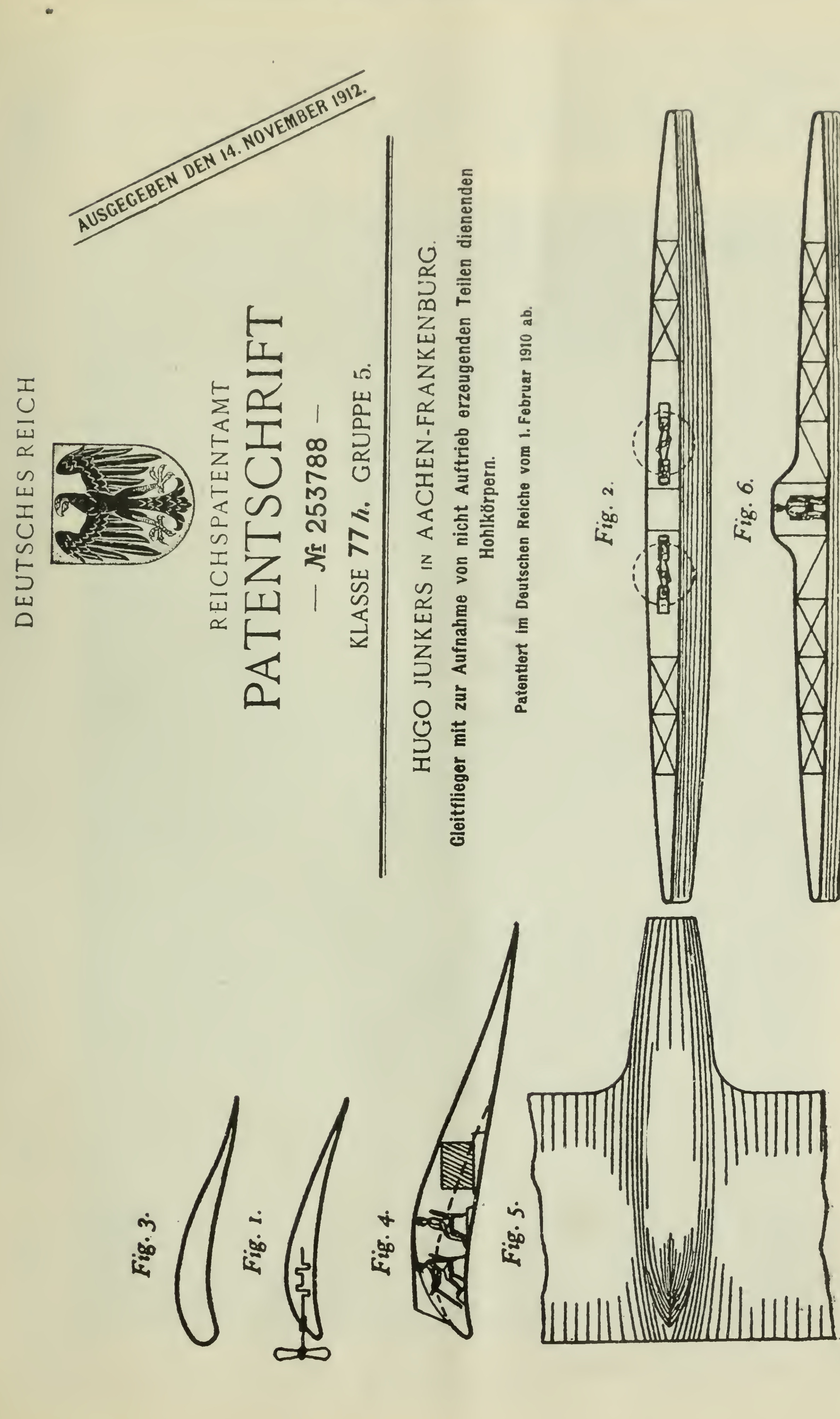


FIG. 1.—*The first iron airplane, J.1 (120 h.p. Mercedes; service weight 1,010 kg.; speed 170 km. (106 E.M.) hour; supporting area* $=24$ m^2).

* It has been found impossible to obtain Herr Junker's reply to the discussion on the Paper, which is, therefore, printed without comment. Figs. 12, 50, 51, 52 are not printed.—THE EDITOR.



DEUTSCHES REICH

AUSGEGEBEN DEN 14. NOVEMBER 1912.

REICHSPATENTAMT

PATENTSCHRIFT

— № 253788 —

KLASSE 77 *h*. GRUPPE 5.

HUGO JUNKERS IN AACHEN-FRANKENBURG.

Gleitflieger mit zur Aufnahme von nicht Auftrieb erzeugenden Teilen dienenden Hohlkörpern.

Patentiert im Deutschen Reiche vom 1. Februar 1910 ab.

FIG. 2.—Patent.

sensible to this side of the question and that this appeal has raised a loud echo on our side.

If I have hesitated, however, an instant to respond to your demand, it was to a certain extent due to the feeling of the heavy pressure put upon German air industry in consequence of the war, a pressure which although finding a certain

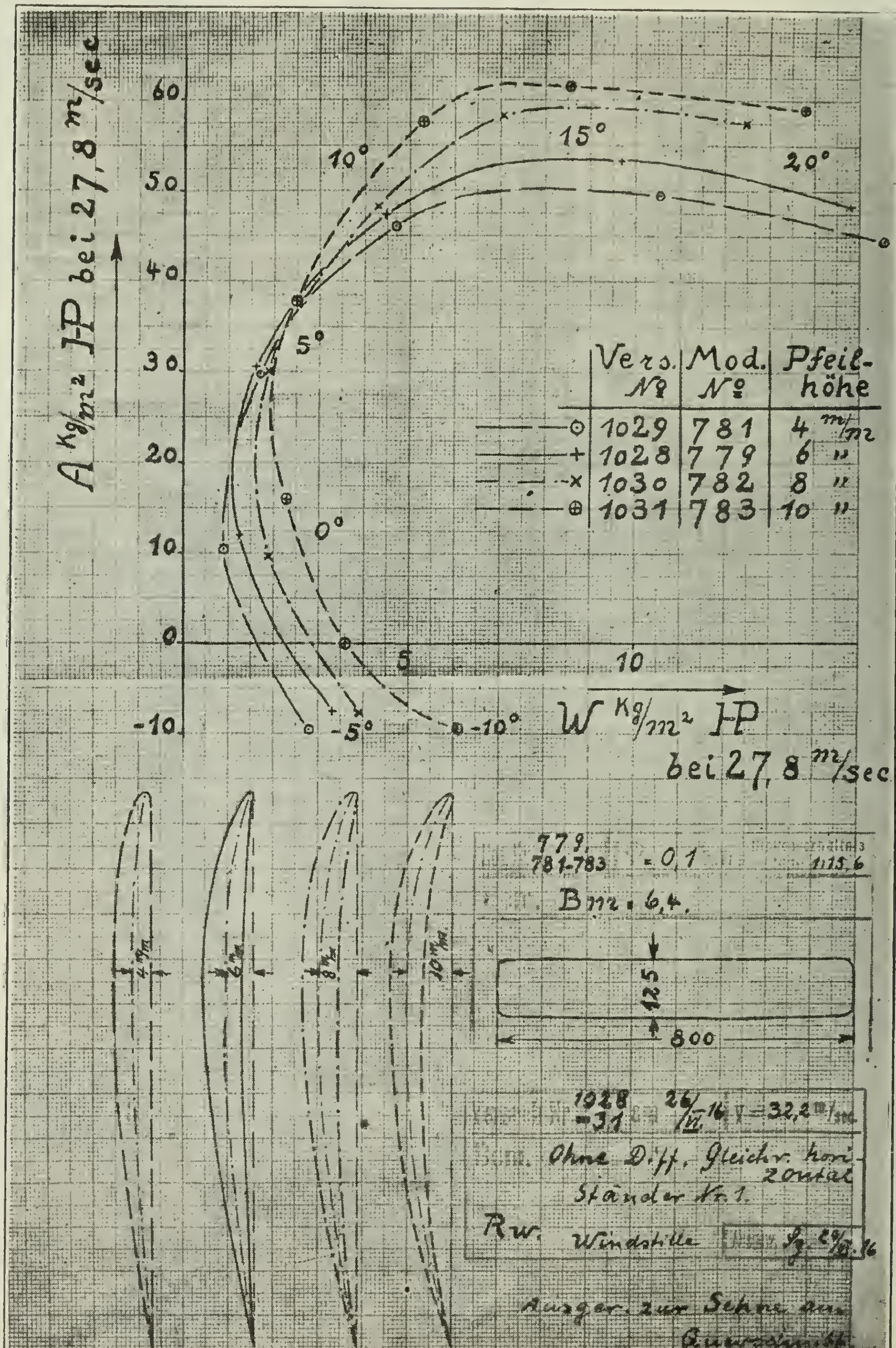


FIG. 3.—*Systematic investigations of wing sections. Effect of the camber of the central line.*

justification as an issue of the war, is nevertheless felt and resented, naturally enough, as a feat of violence overstepping the mark; the acceptance of the invitation possibly then being considered as a sign of acknowledgment of this oppression.

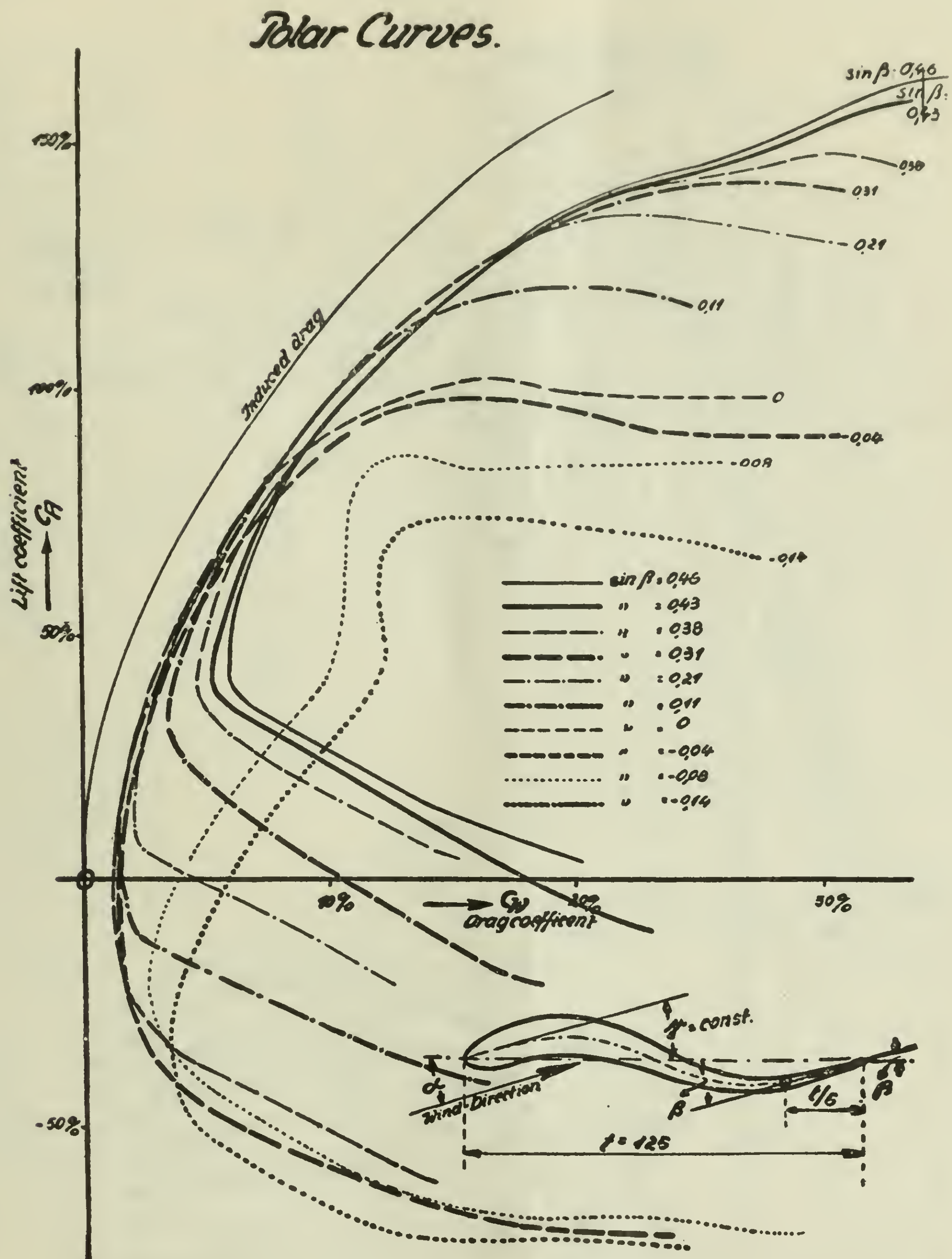


FIG. 4.—*Systematic investigation of wing sections. Tests on the effect of the trailing edge angle β of the central line.*

There had likewise to be taken into account the double-faced sentiment born from a certainly most justifiable instinct of self-preservation; that as on the one side we are not willing to forego our share in the development of aeronautics, we are compelled on the other side, chained by the clauses of the peace treaty, strictly to economise our disposable means, lest we should be defeated in the strife of competition. And not the least, nor worst part of these means is to be found in the results of our investigations, constructions and experiments accomplished under many a hardship.

But all such scruples were wholly suppressed by purely human sentiments and the endeavour to share in the throwing of a bridge over the abyss dividing the nations, a task to the accomplishment of which scientific intercourse is certainly most highly qualified.

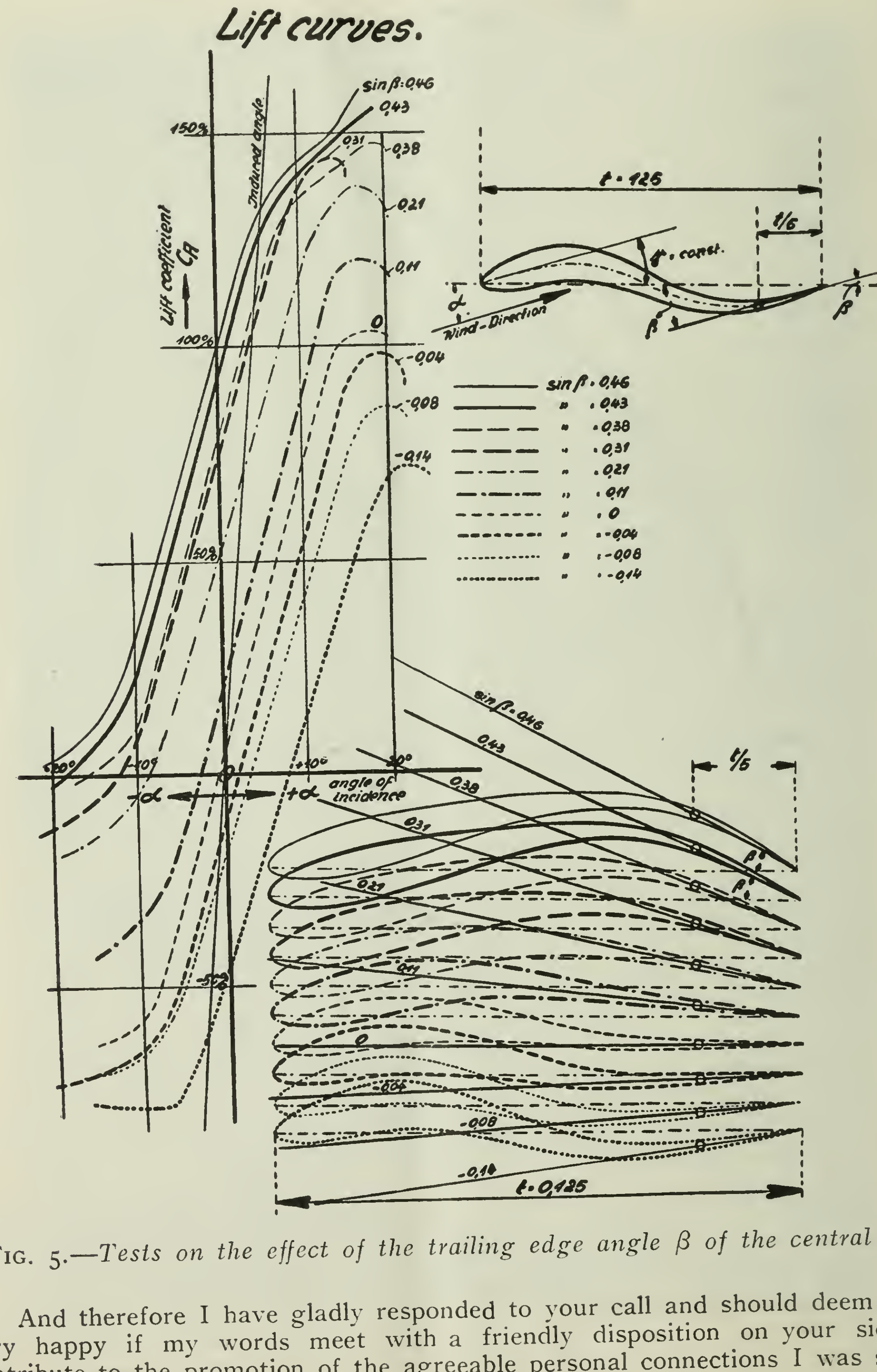


FIG. 5.—*Tests on the effect of the trailing edge angle β of the central line.*

And therefore I have gladly responded to your call and should deem myself very happy if my words meet with a friendly disposition on your side and contribute to the promotion of the agreeable personal connections I was so glad to enjoy in England before the war.

Belonging to the industrial world and consequently obliged to sacrifice all my time and all my efforts to the pursuits of my business, I must forego endeavouring to give you a complete relation of the actual state of metal aeroplane construction from a general standpoint and am compelled to confine myself, with your permission, to sketching an outline of my own work in this technical province.

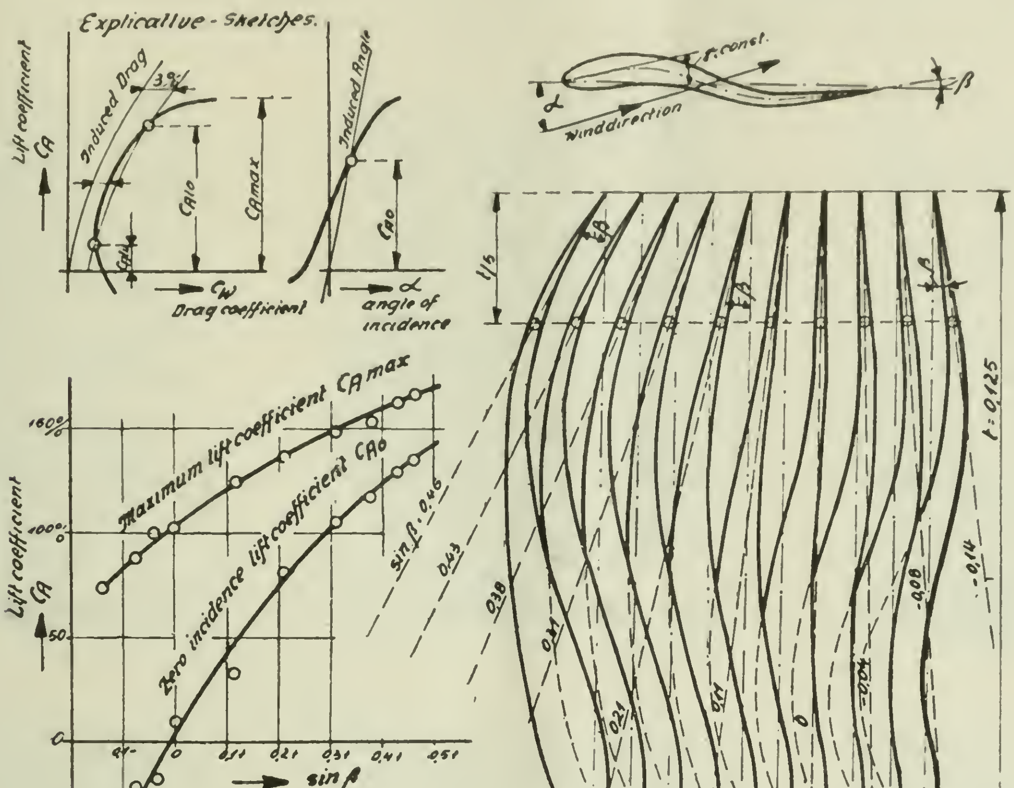


FIG. 6.—*Tests on the effect of the trailing edge angle β of the central line.*

I do not think it would meet with your approbation if I should confine myself to giving a description of my metal aeroplane as *an ultimate and completed product*. The clue to a thorough understanding and full appreciation of the finished article lies in its development; therefore I propose to put in the foreground of my contemplations the evolution of my metal plane and the method

of proceeding and working chosen by me, owing to which I succeeded in the realisation of some new fundamental ideas.

When an innovation is to be carried out in technical matters, the inventive idea represents but a small portion of the work leading to the final product. It will never suffice to carry out the conception with the means by chance at hand, *e.g.*, the working it out in a model. It is necessary to submit the laws of aerodynamics and resistance, the structural shaping, materials, adaptation of

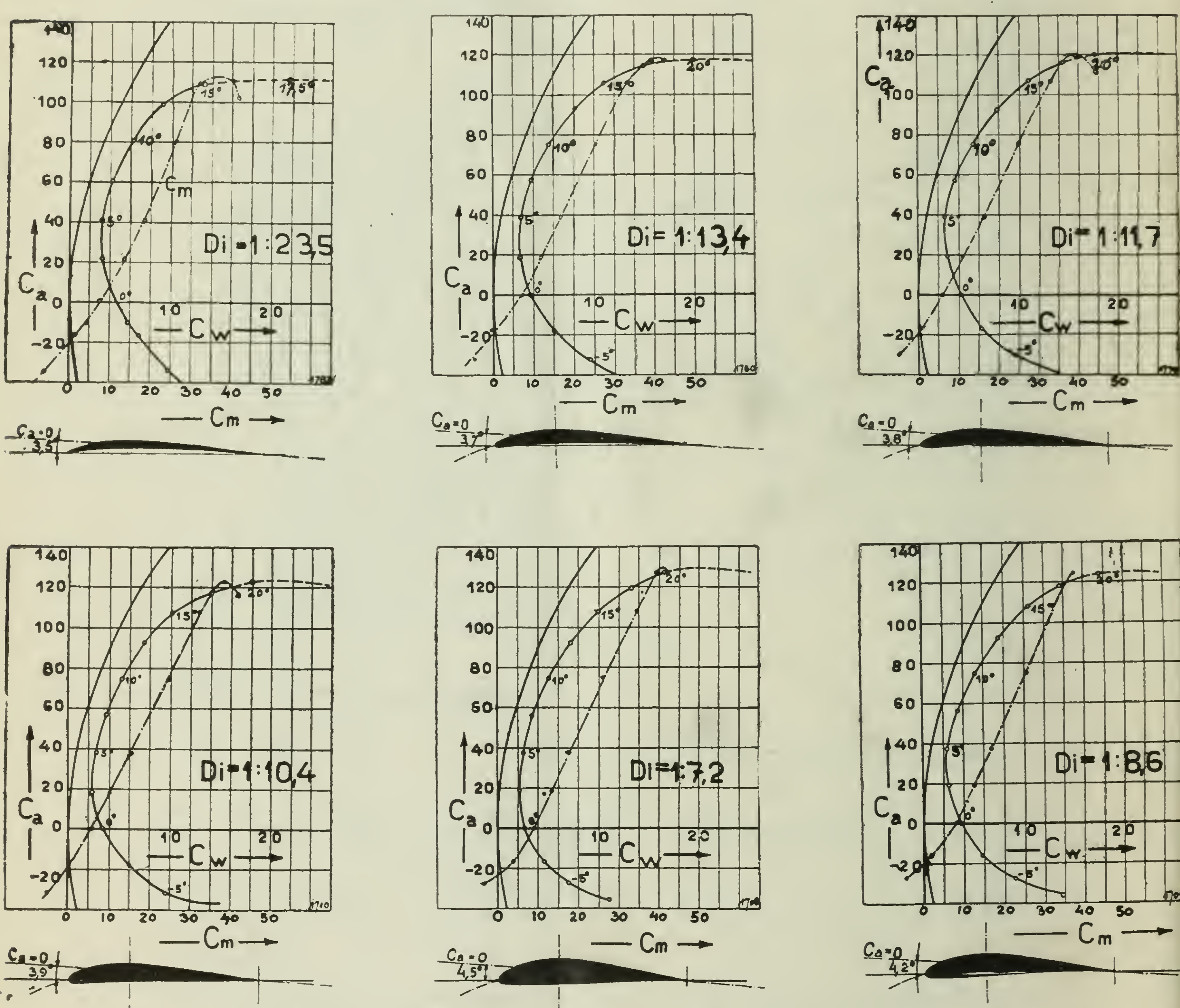


FIG. 7a.—*Systematic investigation of wing sections. Effect of thickness ratio Di (chord height/chord length); polar curves.*

workshops, and even the requirements of the market to a systematic examination before the original idea may be pronounced a success. It is necessary to see how far such considerations can be made serviceable to the ideal end, or if the idea must be modified in order to become feasible. Such an examination must be a necessary point of departure if it is to respect the confines of economy, the approved results of science and to apply her methods. But the work must remain subordinate to the uniform technical aim and therefore be under one control to allow for continuous adaptation, limitation and mutual influencing of

the individual departments busy with it. To repeat briefly, the realisation of technical ideas must be the fruit of an *institute of research* (Forschungsanstalt) uniformly conducted, but covering many branches. Such an institute I was able to create as a result of many years' hard work, aided by eminent collaborators, and I am indebted to this institute for my success in carrying out a technical innovation like the metal aeroplane from the first conception to the practical execution in a surprisingly short space of time and with a comparatively small expenditure.

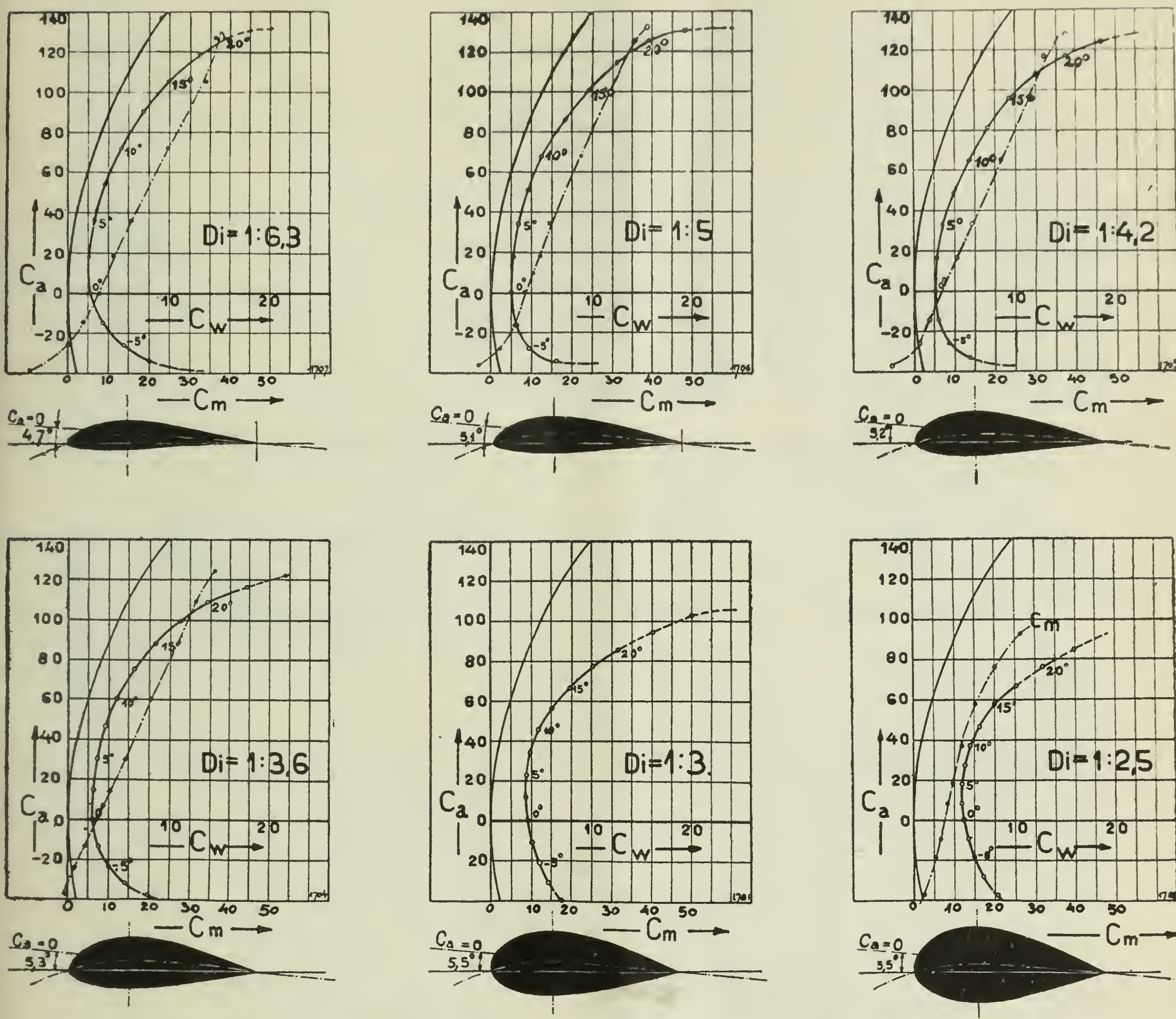


FIG. 7b.—*Systematic investigations of wing sections. Effect of thickness ratio Di (chord height/chord length); polar curves.*

I see the correctness of the method, founded on long preliminary studies, in the success of the very first metal aeroplane constructed by the Institute of Research (Forschungsanstalt), the steel aeroplane J1 (Fig. 1). It was finished within four months, and although diverging in every respect from the usual constructions and representing a complete innovation, had a remarkable performance for an experimental machine. It attained, according to an official report of the army authorities of 1916, an average speed on a return course of 170 km. per hour with an engine of 120 h.p. and a service weight of 1,010 kg., climbing

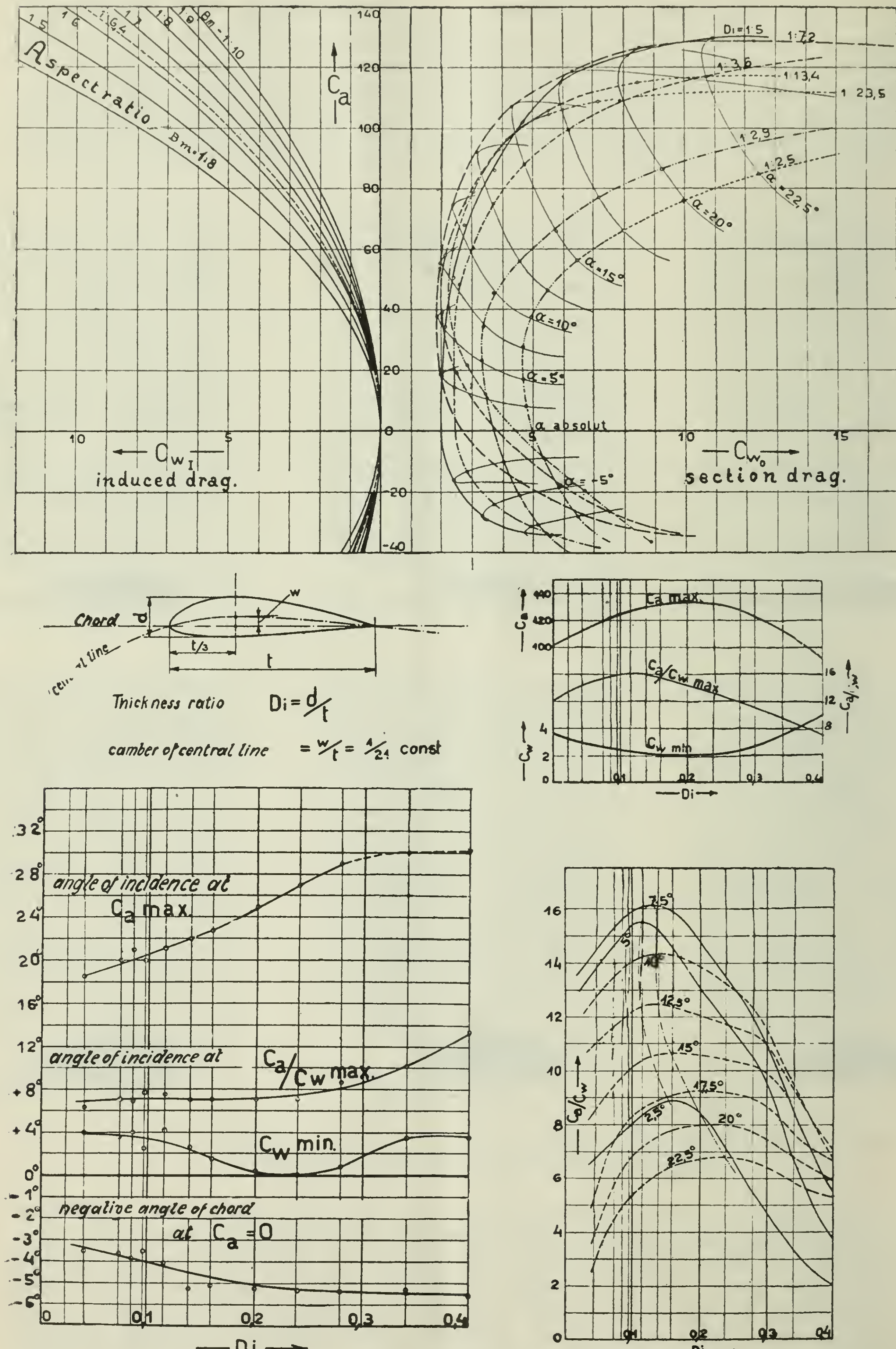


FIG. 8.—*Aspect of thickness ratio; assemblage.*

at the rate of 2-2.5 m. per sec. This corresponds to a ratio lift/drag of $>$ 10, or more accurately 10.7, assuming a propeller efficiency of 75 per cent., resulting from the formula $W/(75\eta P - V_h W v/3.6)$, where W = weight in kg., V = speed in kmm., η = propeller efficiency, V_h = velocity of climb.

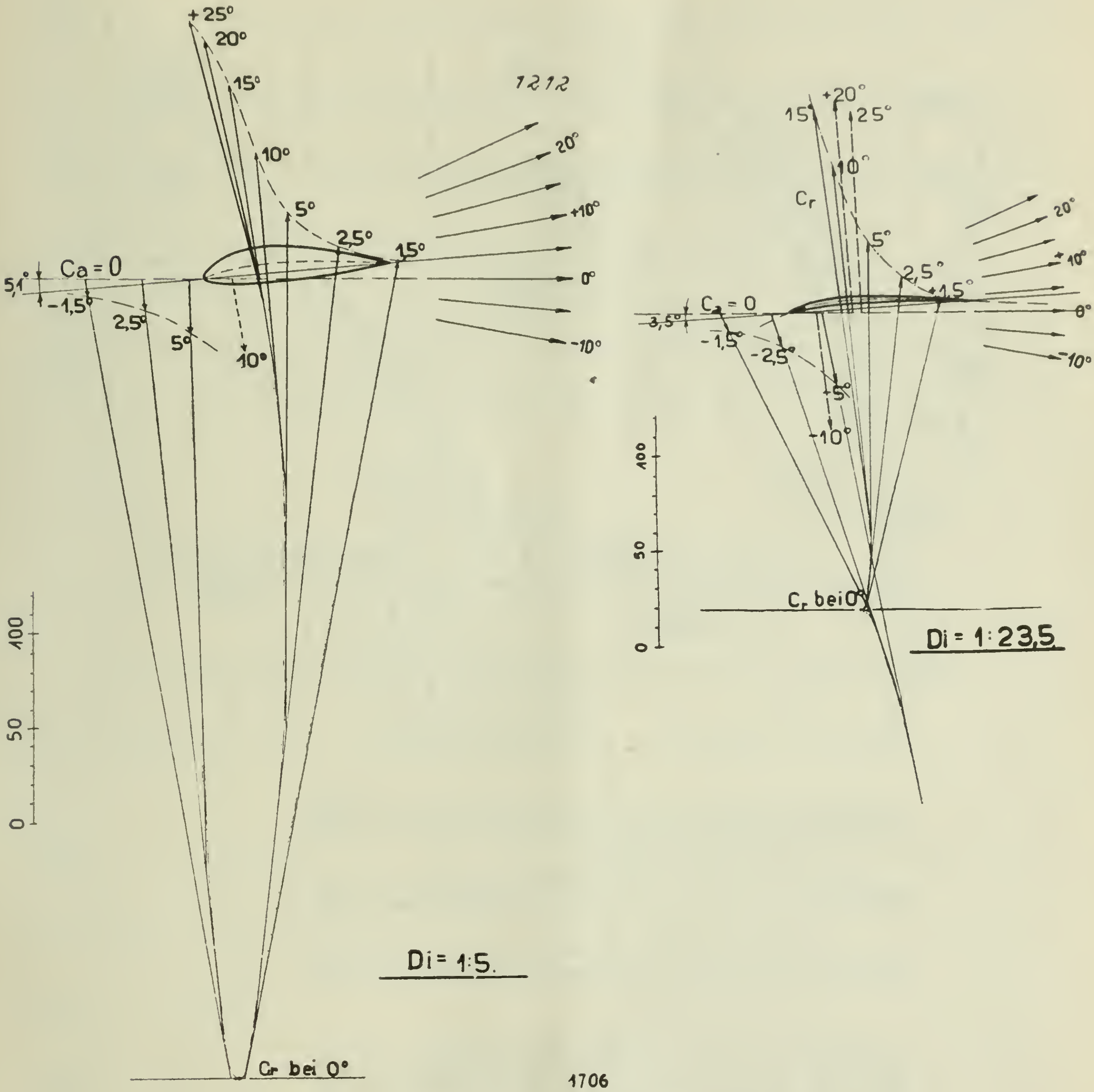


FIG. 9.—*Effect of thickness ratio (Di); diagram of forces.*

The construction of my metal aeroplanes is based on a patent, granted to me as far back as 1910 (Fig. 2).

I was then perfectly aware of the fact that the main end of aeroplane construction is to be found in a greatly diminished parasite resistance. Constructors had begun at the time to surround individual parts, exposed to the open current of air, with a flap or any sort of envelope in streamline form, with the object of decreasing the enormous loss of efficiency caused by such resistances. But this

was not sufficient. The streamlining, or fairing, or covering, must be shaped as a hollow space producing a minimum drag with a maximum lift. This idea is the nucleus of the patent. In other words, the structural parts of the aeroplane, as well as the power plant, crew and useful load, tanks and so on, must be located in the sustaining units, *i.e.*, the wings.

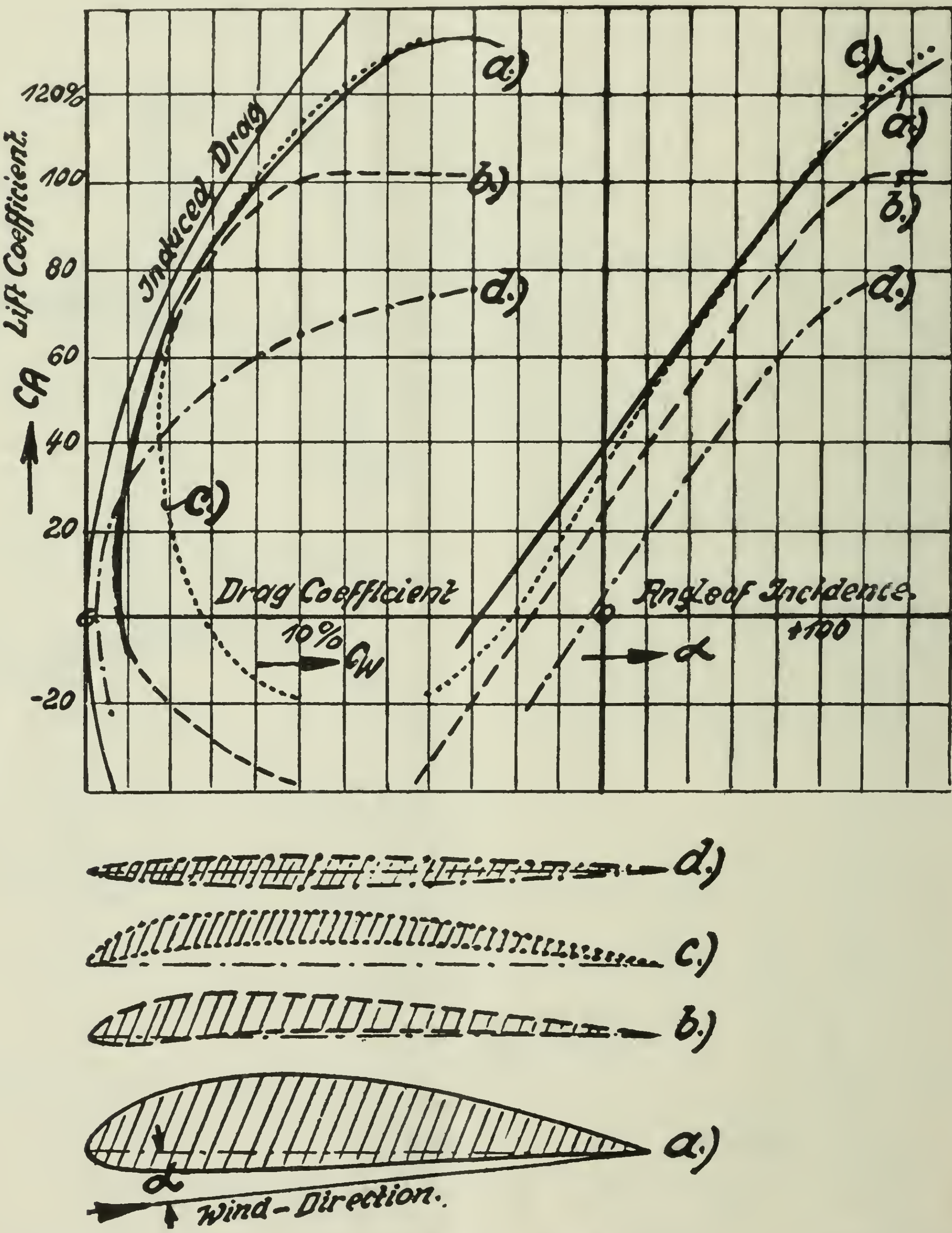


FIG. 10.—*Polar curves of four particularly good aerofoils of various thickness and camber.*

Aeroplanes of comparatively large dimensions afford in a particularly high degree the possibility of carrying out the conception upon which the patent is based.

It was in fact the creation of a swift and economical commercial giant aeroplane which I had in view as the ultimate object of the development. With such

a craft the superior attributes of metal construction would become obvious in a most noteworthy degree.

Among the advantages the first is the greater durability. Wood is subject to the dangers of fire and decay, and splinters when breaking; it bursts and warps from the effect of humidity and change of temperature and the glued joints split; finally it is attacked by insects. No wooden aeroplane, serviceable for any length of time in the Tropics, has been produced as yet. Metal is free from all such drawbacks.

Structural parts made in wood also change shape and size; they swell or warp under the influence of heat and humidity, making necessary a continuous re-setting and trueing-up of the aeroplane. All this does not apply to metal, and a constancy of form is necessarily important in aeroplane wings, slight changes frequently producing a distinct deterioration of the aerodynamic qualities.

Thus metal aeroplanes have the advantage of greater durability, smaller expenditure for repairs and maintainance and of preservation of form.

Besides these we have the superiority of metal from the standpoint of the designer and constructor. The designer is less handicapped in the choice of dimensions and structural contours. Wood is obtainable only in fixed sizes and shapes of trunk and branch, furnished by nature, whereas metal may be obtained in a nearly unlimited variety of qualities and dimensions. We have sheet metal

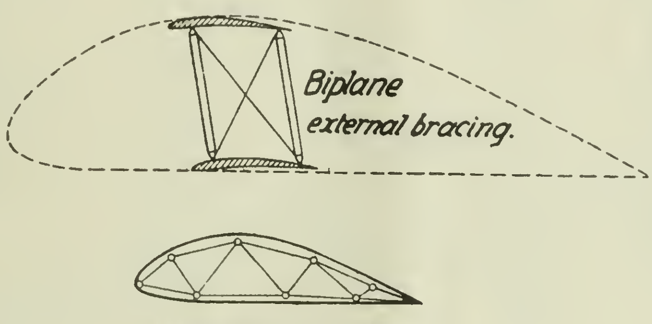


FIG. 11.—*Biplane cell and cantilever monoplane wing.*

down to a thickness of 0.004 inch, plates of more than 100 yards length, tubes, rolled section girders, etc.

Shaping of wood is limited, while metal might be given nearly any form by pressing, forging, casting, rolling, drawing, overlapping, etc.

And it is just as workable as wood for turning, planing, milling, boring, filing and punching.

Connections and joints, confined in the case of wood to glueing, bolting, mortising and wrapping with fabric, all of limited reliability, are much more varied and dependable with metal; as, for example, welding—autogenous or electric—riveting, screwing, folding and soldering.

Furthermore, the strength of metal is constant and can be stated any time in a reliable manner by tests, whereas the properties of wood are liable to change, wood being altogether highly unhomogeneous. Thus, the safety margin to be kept with wooden constructions must be much higher without giving a sufficient guarantee against undesirable surprises.

The result is that the application of modern methods of manufacture, such as mass production, interchangeability, standardisation, wide application of machine work, according to my opinion, can only be fully made use of in metal construction, and that the unreliable wood must be avoided even in the

building of single large aeroplanes, where the consequence of a crash can be disastrous both from the point of view of human lives and cost.

All these advantages are confronted by two drawbacks.

Constructions in wood require less expensive tools. This, however, only applies when the construction is on a small scale and disappears in mass production.

Again, wood is of smaller density. This argument is the one most generally advanced against metal, and with good reason, because this circumstance highly facilitates the attainment of great resistance to axial compression. But we have

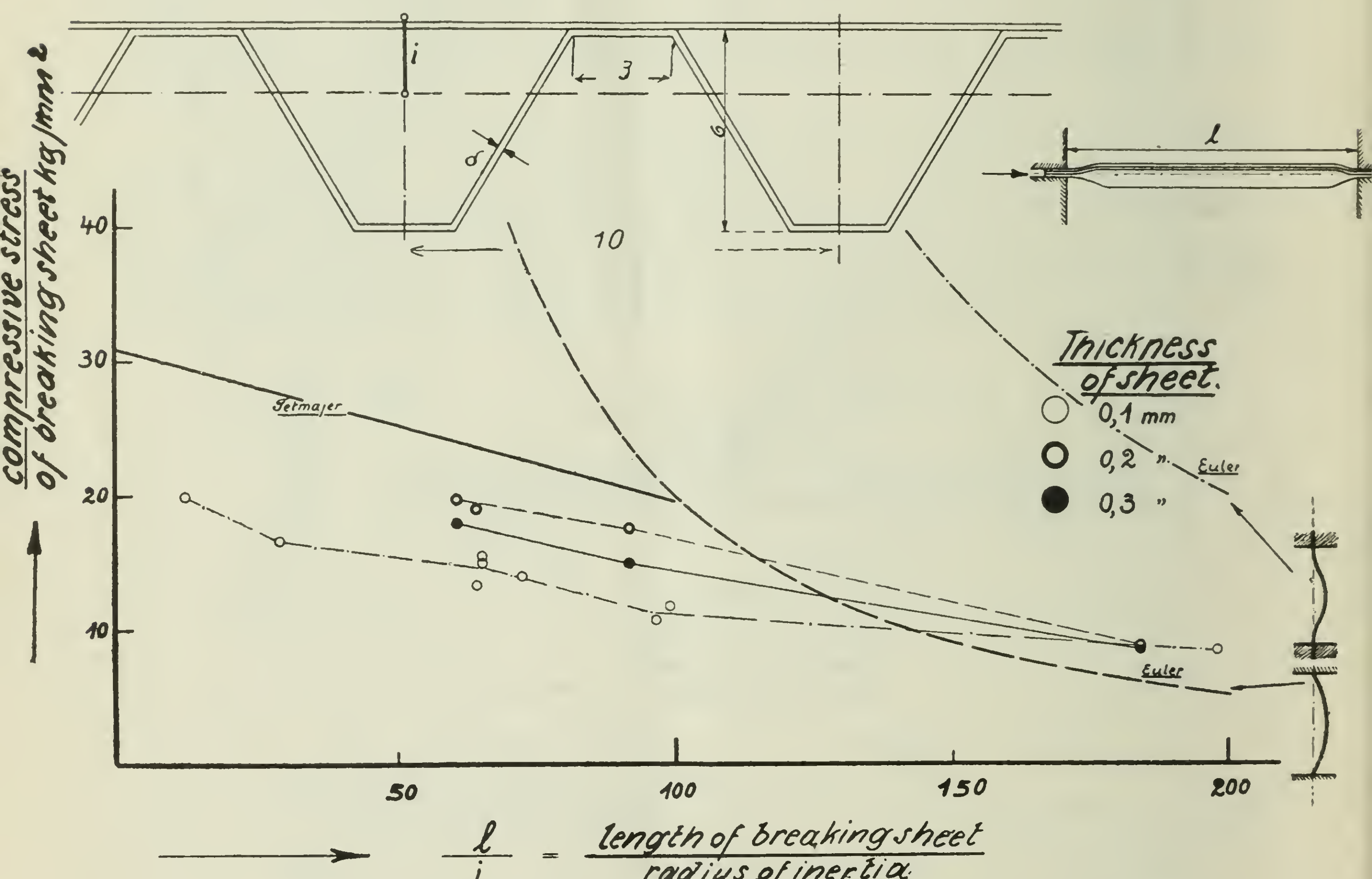


FIG. 13.—*Axial compression experiments with reinforced iron sheet.*

succeeded by means of extensive investigations in overcoming this advantage of wood by appropriate construction and the use of suitable material.

We have to view at present the question of the practical realisation of the thick cantilever metal wing. We had to investigate whether it was feasible to produce a wing which would combine with a thick section a low drag and a sufficient lift, and whether such wings could be constructed in metal in such a way that the aerodynamical attributes would not be suppressed by the drawback of too high a weight.

Up to this time only aeroplanes with the very thinnest wing sections had been constructed and the aerodynamical investigations in this province of science—as, for example, those of Eiffel or of the "Göttingen Aerodynamics Institute"—had likewise been conducted exclusively in this direction. There prevailed the general opinion that good results, both in lift and drag, could only be obtained with a thin wing.

I should hardly have mustered up the courage of starting the construction of thick wings, but for the fact that there existed already at that time highly interesting experimental researches, proving that in the case of rotary bodies, at any rate, the air resistance does not by any means depend on the section at right angles to the flow.

Still, as already mentioned, there did not exist any available aerodynamical investigations concerning thick wing forms; they had to be freshly created.

To this end I built first at Aachen and later on in Dessau a wind channel for the study of lift, drag, displacement of centre of pressure, etc.

My researches were not based on the usual aerofoil sections, but were started with bodies of elementary shape, such as ellipsoids, etc., and it soon appeared, when we passed to ellipsoids growing flatter and flatter, but of a constant perpendicular section relatively to the air flow, that the size of the section was not in fact of any deciding consequence and thick forms were not only admissible, but even—within certain limits—superior to thin ones.

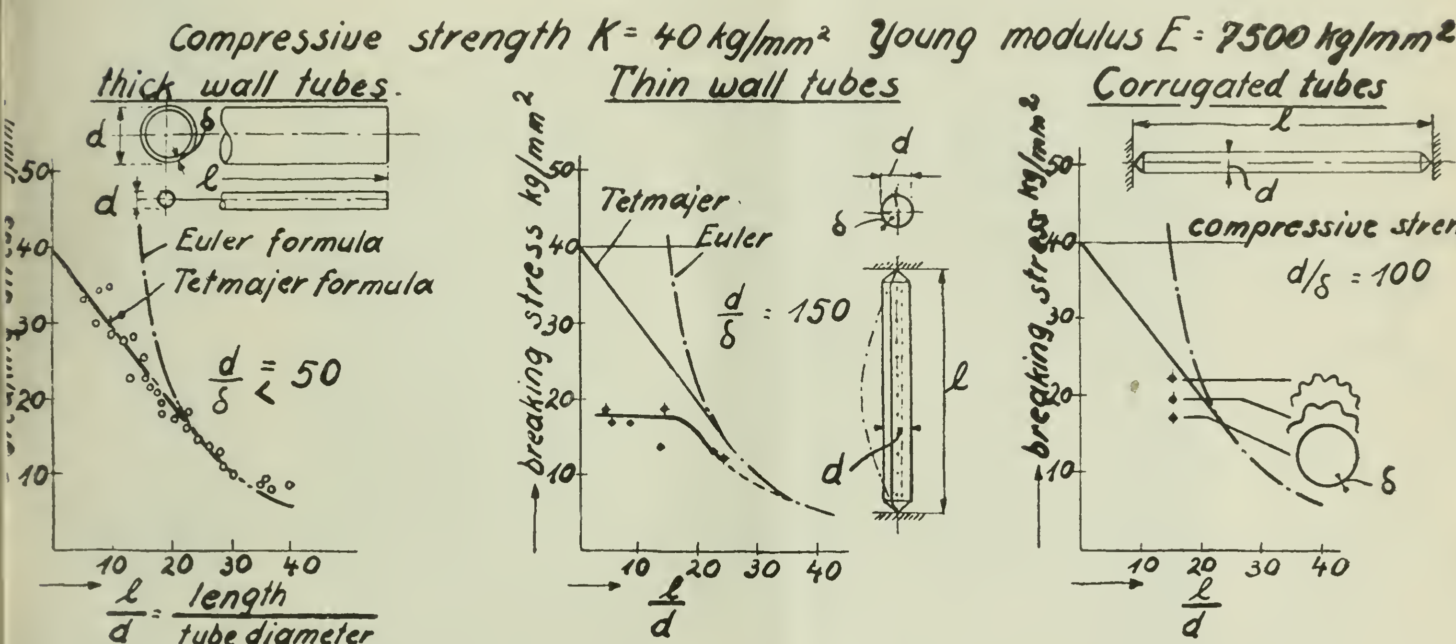


FIG. 14.—*Axial compression tests with duralumin tubes.*

The ensuing systematic investigations of aerofoil sections furnished quite a number of new results of great interest and importance.

Thus it became manifest that it is not so much the shape of the suction side and compression side of the wing profile in itself which has to be considered, but rather that of the central line, by which I understand a line equally distant from the upper and lower surface of the wing profile. The camber of this central line, more especially the magnitude of its leading angle at the head and the rear angle at the tail, are of extraordinary significance for the trend of the curve of lift and drag.

Fig. 3 presents a series of tests executed on models of the constant thickness ratio (chord-height/chord length) = 1 : 15.6 under a varying camber of the central line. (The thin aerofoil had been selected owing to such a model being on hand.) With increasing camber the maximum lift increases, while in the lower portion of the curve there takes place a marked augmentation of drag. The curves are shifted in a sort of parallel motion to the induced drag.

The results of tests on the variation of the trailing angle of the central line are shown in the following Figs. 4-6, representing besides values of c_a and c_w

the maximum lifts, zero angle lifts, minimum drag and the range of lift for values of $c_w < 0.03$, corresponding to the various aerofoil sections.

But the most important and surprising data were furnished by the experiments as to the effect of the ratio of thickness.

In Figs. 7a and 7b are shown c_a and c_w curves of a series of wing forms with thickness ratios varying from 1 : 23.5 down to 1 : 2.5, with an unvarying central line. My most extravagant expectations were surpassed: the thick aerofoils proved not only equivalent to the thin ones of some series, but even superior, within certain limits.

The following resumé of this series of tests shows (Fig. 8) that the most favourable conditions both of lift and drag are found for a thickness ratio of about 1 : 7 to 1 : 5. It will be noticed that even wing forms with a convex lower surface, so strictly shunned up to the present, are quite admissible.

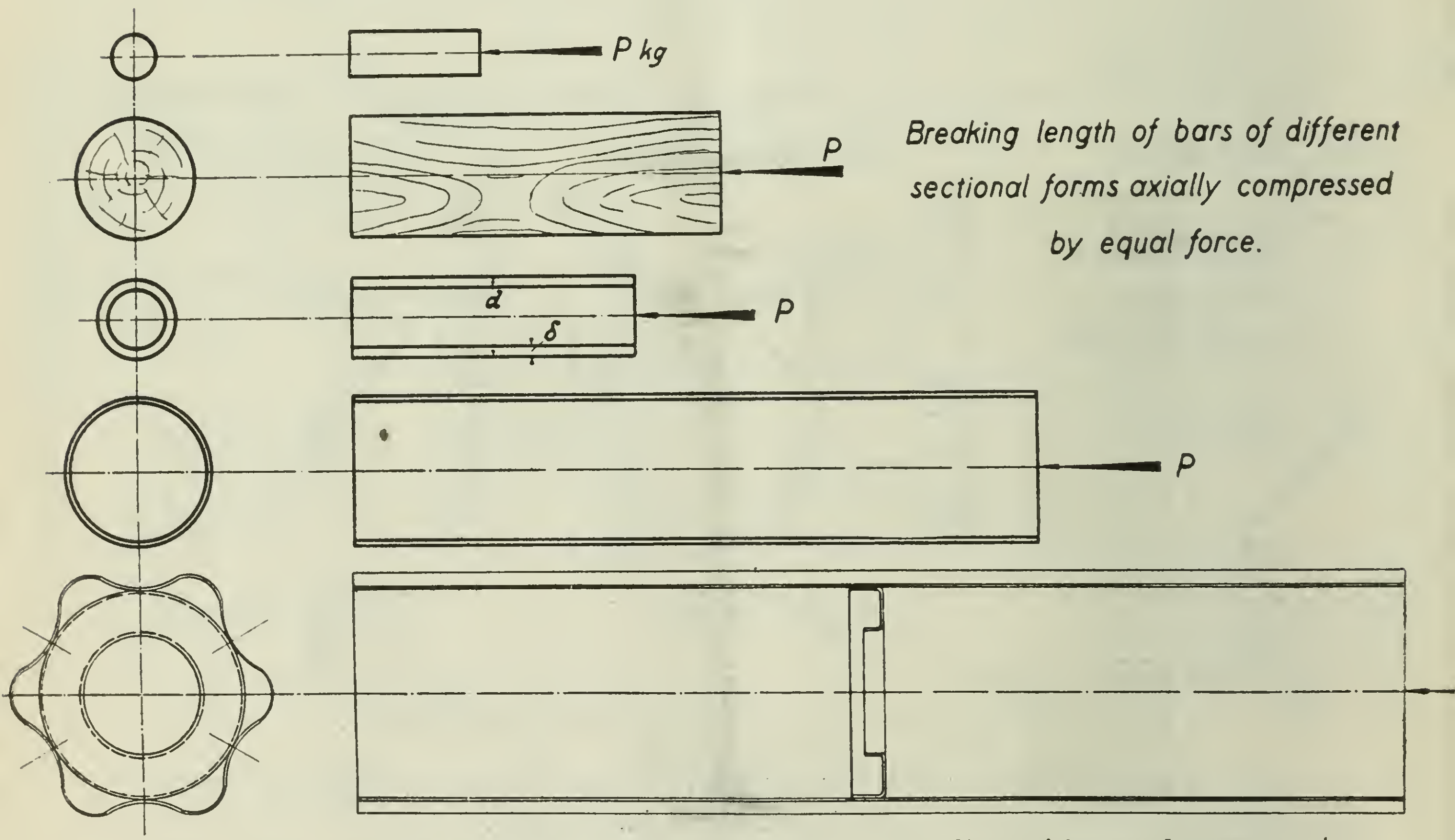


FIG. 15.—*Axial compression; breaking length of bodies with equal compressive stress.*

Even in regard to the displacement of the centre of pressure (longitudinal stability) the thick wing at least not inferior to the thin one. You will observe in Fig. 9 that this displacement is nearly equal for both sorts of sections, a closer investigation having even been somewhat in favour of the thick aerofoil. aerofoil.

It is a consequence of the long lever arm at which the drag component at zero-lift operates, that a high torsional stress upon the wing is exerted in a dive. I shall return to this point later on.

The subsequent diagram (Fig. 10) is particularly illustrative of the superior merit of the thick wing form; it gives the typical polar curves of a thick section besides those of a highly and a slightly cambered as well as a symmetrical thin wing section. The tests were selected from those published in the latest report

of the Götingen Aerodynamics Institute, taking in every case *the* aerofoil, which gave the best result in its group.

If the object be to attain a maximum of horizontal speed with a thin wing form, only a form with a slight camber (*b*) with a minimum drag comes into account, whilst for a satisfactory performance in climbing, starting, alighting, etc., a strongly cambered section (like *c*) with high maximum drag is required. Curve *a* of the thick form envelops curves *b* and *c*; hence the thick section combines the advantages of both the strongly and the slightly cambered thin one—even leaving out of consideration eventual resistance of bracings to be added for the thin section.

Resuming, we see that the need of housing the parts producing drag within the wing coincides in a surprisingly happy manner with the requirement of an aerodynamically good aerofoil.

At the same time the realisation of another demand on the wing is facilitated by the thick aerofoil: that of sufficient strength with smallest weight. This results from a consideration of the forces acting on the wing.

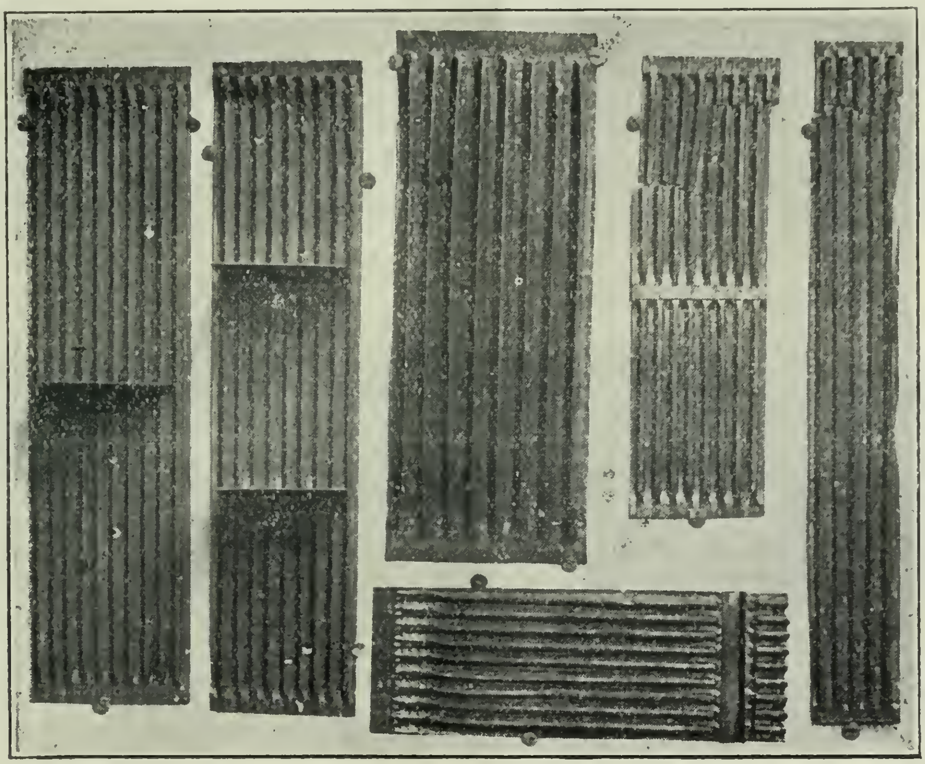


FIG. 16.—*Examples of sustaining iron cover of J.1.*

The characteristic stress upon the aeroplane wing consists in the high bending moments, arising from the concentration of the greatest part of the aeroplane's weight and the total useful load in the middle of the aeroplane; while the counter-pressure of the air is about evenly distributed over the wing; the bending moment increases the larger—in consideration of a favourable aspect ratio—the span is chosen. Also there is always produced in cambered wings a torsional moment, an attempt to twist the wing in relation to the body, which eventually attains very high values in a dive.

The bending moment must be absorbed by tensile and pressure forces. If we consider the wing as a framework, the tensile and compressive stresses increase, the lower the constructional height, *i.e.*, the distance between the extended and the compressed parts.

The problem of great structural height has so far been solved very cleverly in aeroplane construction by the biplane cell. An upper and a lower wing are employed which are joined together by means of struts and wires and so produce a framework. The air resistance of those connecting pieces is, however, very considerable, this circumstance forming the great drawback of the biplane cell.

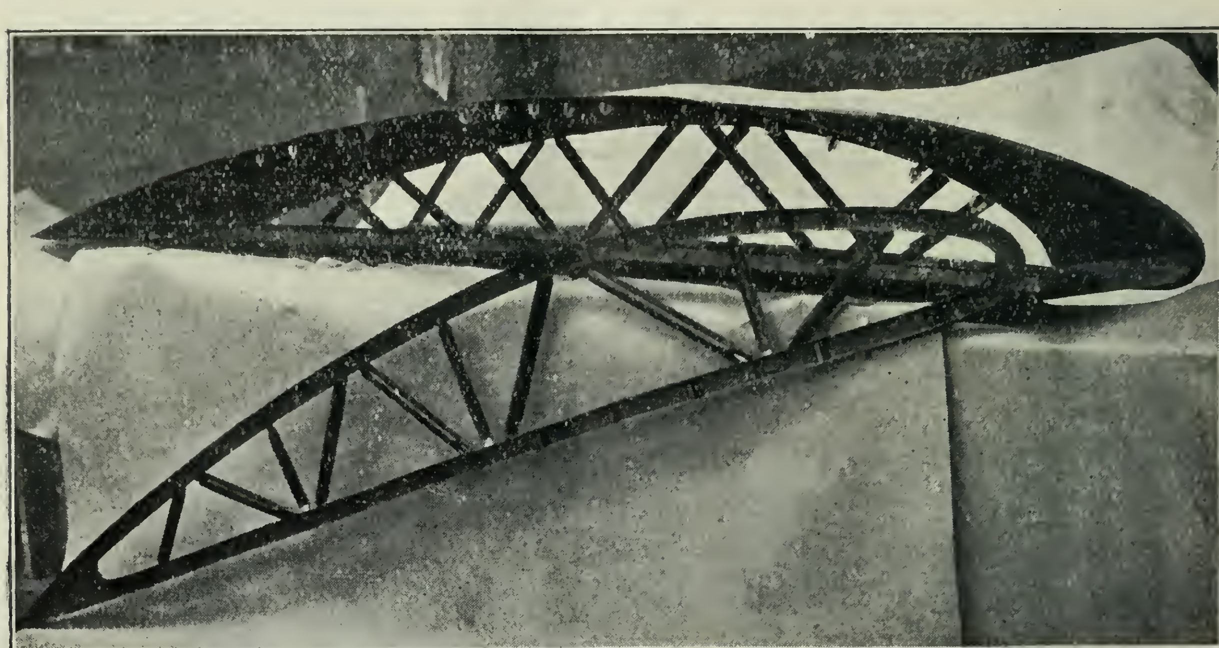


FIG. 17.—*Wing portion and rib of the iron wing.*

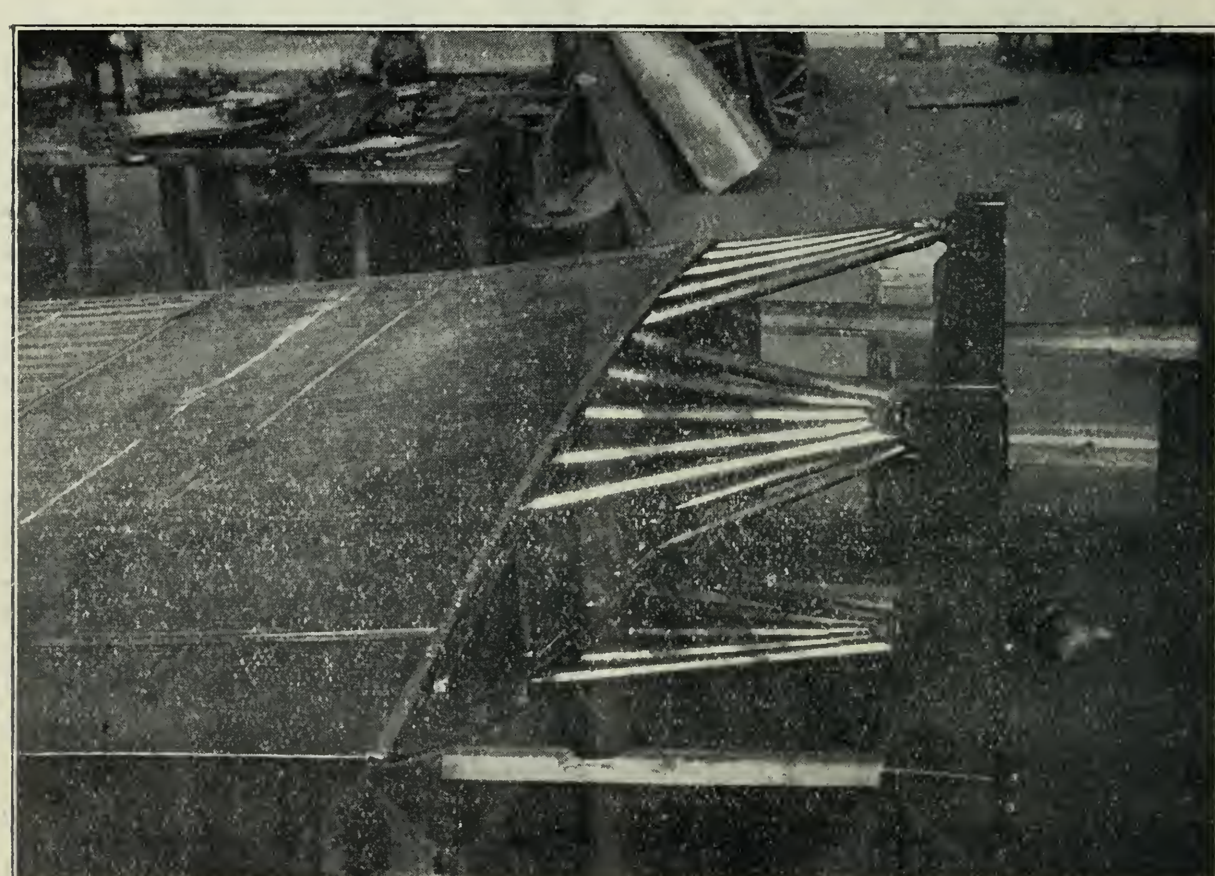


FIG. 18.—*Connection of iron wing and fuselage by claws.*

Now the method of avoiding this shortcoming is indicated in Fig. 11. It consists in enveloping the cell in a suitable thick wing section. In this way the biplane cell is transformed into the thick cantilever wing, the drag of which is distinctly less than that of the biplane cell under an equal lift, and which in addition forms a large hollow space for the location of bodies which would produce drag if exposed. These were the reflections which served as a basis for the patent.

It will now be interesting to examine the biplane question, setting aside the usual standpoint of the braced biplane cell and imagining it to be a combination of two cantilever monoplanes. How will such a cantilever biplane compare with an aerodynamically equivalent monoplane?

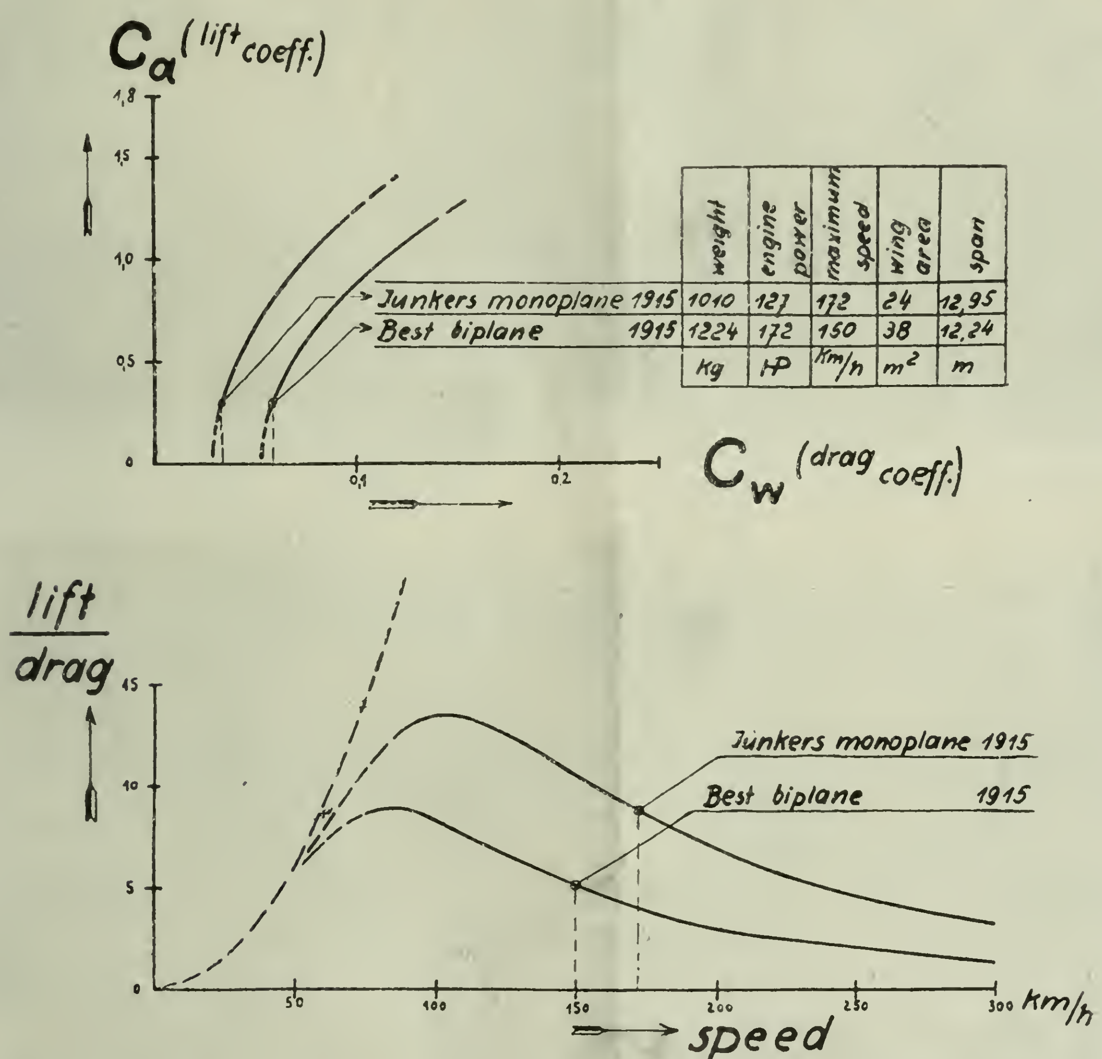


FIG. 19.—*Polars of the first iron plane, J.1, compared with the Rumpler biplane R.C.I.*

Now such mono- or biplane types must be considered as of equal aerodynamical efficiency, which exhibit the same drag for the same aerofoil section, total supporting area and lift. According to Prandtl's multiple plane theory, which agrees very well with experience, the span of the monoplane is certainly greater than that of a biplane of "equal value," the differences not being, however, very considerable (20 per cent. or less), whilst the length of the chord and consequently also its height is in the monoplane nearly double that of the biplane.

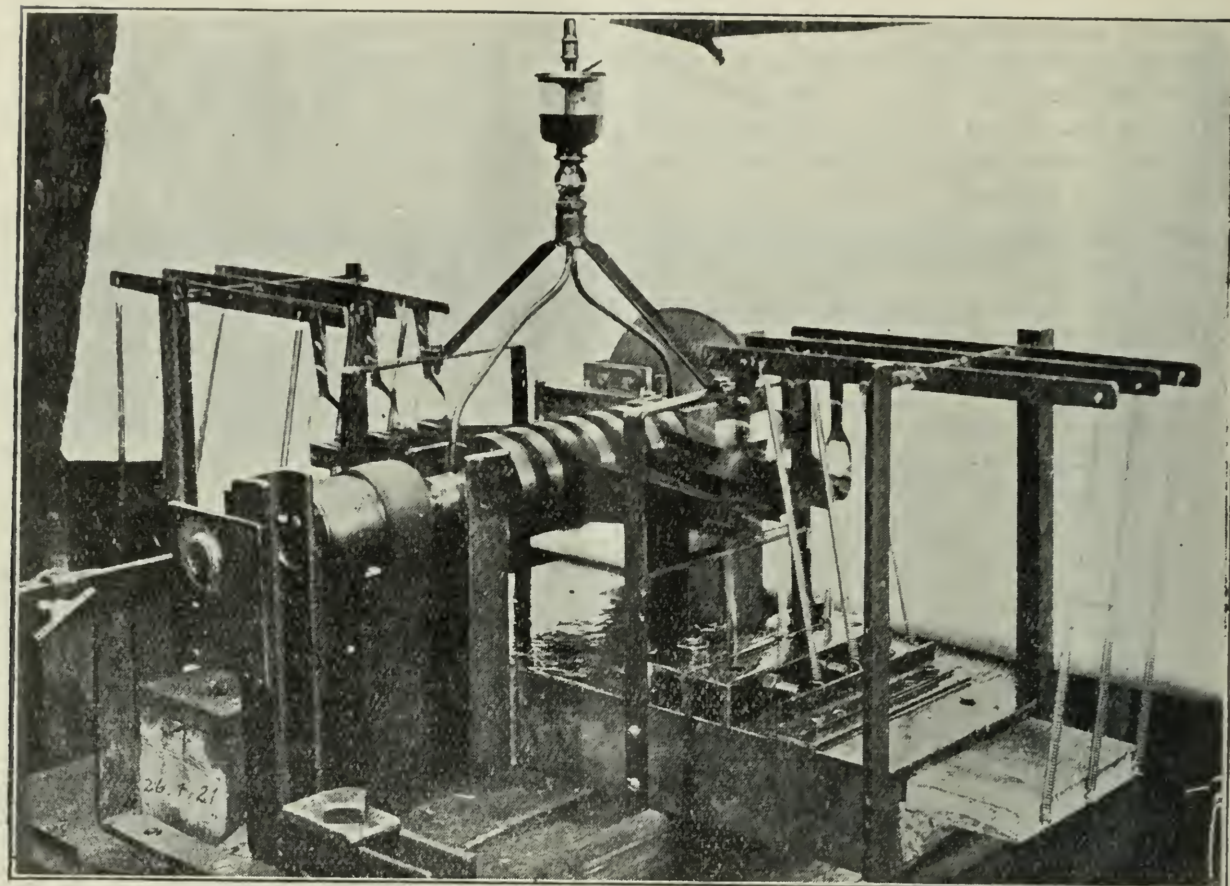


FIG. 20.—*Device for fatigue tests of sheet metal bands.*

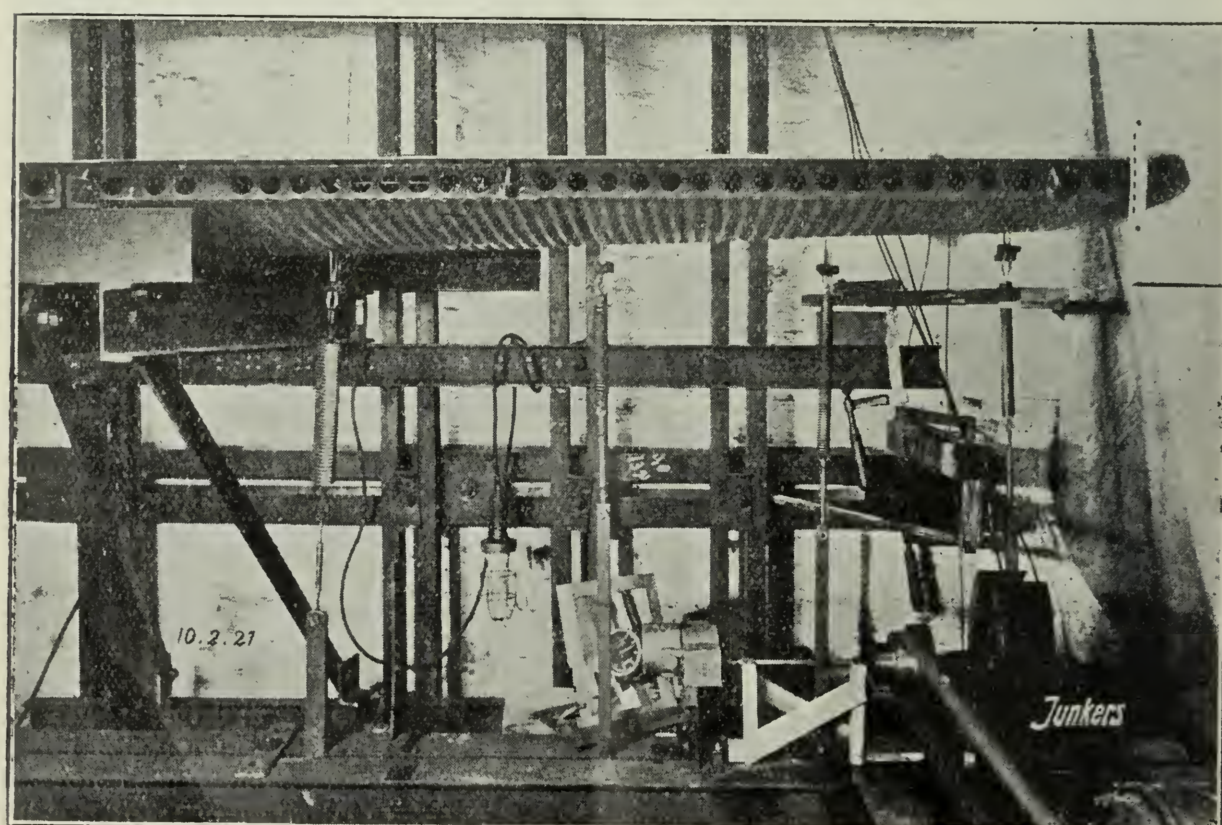


FIG. 21.—*Arrangement of vibration tests on tail plane.*

Thus the ratio of height to span is undoubtedly best with the monoplane; the strength being equal, the latter is the lighter, and in addition has the advantage of having a larger hollow space. Consequently I came to the conclusion that the cantilever monoplane is to be preferred to the normal multiple plane type in every respect.

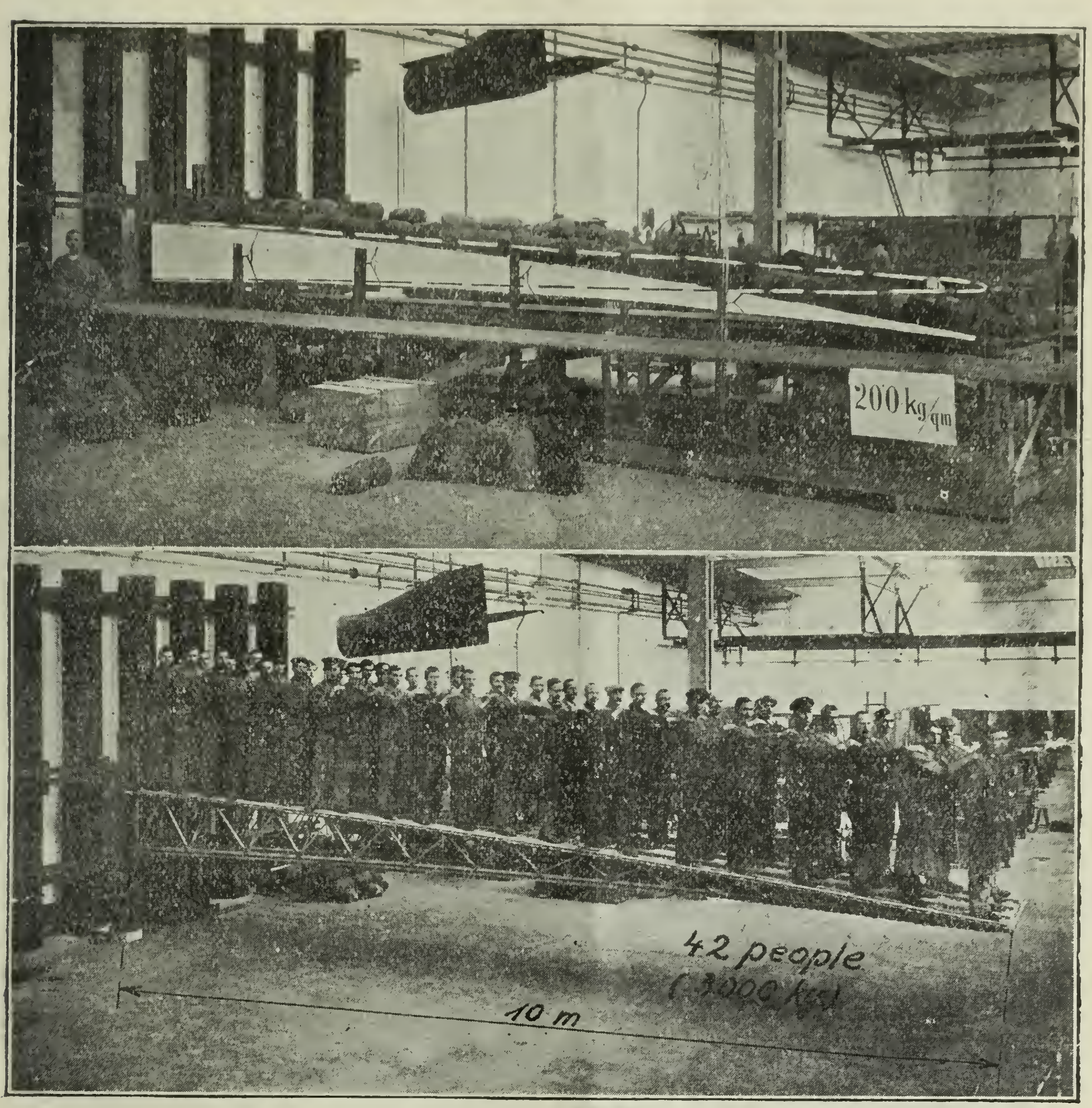


FIG. 22.—*Experimental framework wing: area* = 15 m^2*: weight* = 110 *kg.* (7.4 *kg./*m^2*); loading capacity* = 4,500 *kg.* (300 *kg./*m^2).

If the permissible tensile and compressive stress be given, the weights of the wing parts conveying the air forces upon the fuselage may be readily computed from the structural height and the span. In Fig. 12* is seen the result of such a calculation for a surface loading of 50 kg./m^2, the assumption being made that the outer cover of the wing can fully stand the indicated theoretical tensile and compressive strains.

Such a wing—we shall call it the ideal wing—would have the weight indicated by the lines of the diagram per unit of surface. Thus, the weight per square metre would be in direct ratio to the span of the wing.

* Not printed.

We see that the "ideal wing" is of extraordinarily light weight, lighter by far than the usual wings of wood and fabric. How far we are as yet from this ideal is demonstrated by the dots in the diagram, representing metal wings which have actually been built.

Now the reason for this great divergence between the effective and the ideal weight is to a great extent to be found in the fact that the theoretical compressive strength of the wing parts exposed to the pressure forces cannot be utilised on

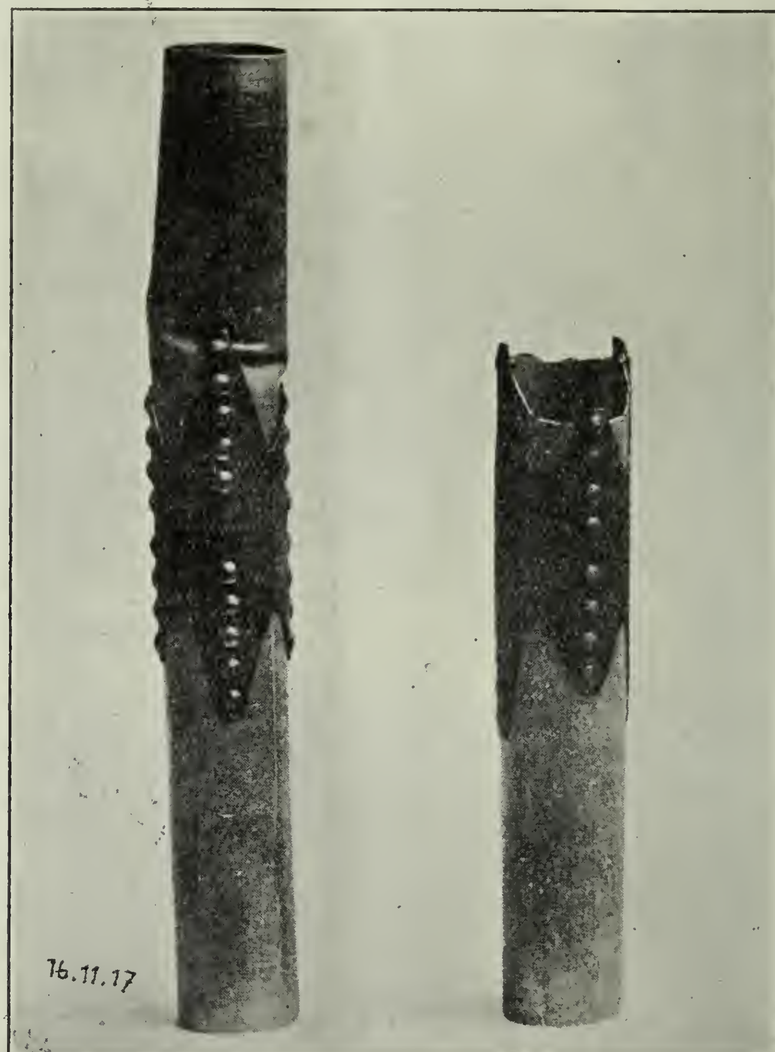


FIG. 23.—*Telescope joints for tubes.*

account of axial compression buckling; or to state it differently, the stress on the members subject to pressure forces may but attain a fraction of the theoretical resistance to compression. Therefore axial compression is the most material question for the construction of metal wings.

I have carried out a series of very exhaustive researches concerning the process of axial compression or lateral flexure.

I tried to apply the well-known fundamental formula of Euler and the results of the experimental researches of Tetmajer with reference to the relation between compresive stress on the one side and breaking length under lateral flexure and radius of inertia on the other side. Our relative tests proved, however, that the range of validity of those formulas did not extend to the thin-walled bodies concerned. If the thickness remains beneath a certain minimum there is produced a

local bulging out which can to a certain extent be remedied by means of special devices (*e.g.*, the application of an undulated stiffening cover).

In Fig. 13 are produced some results from tests of axial compression of sheet iron of various thickness, stiffened by a second layer of corrugated sheet metal. It will be noticed that the actual stress due to axial compression lies, for such thin walls, appreciably below the stress theoretically admissible according to Euler-Tetmajer.

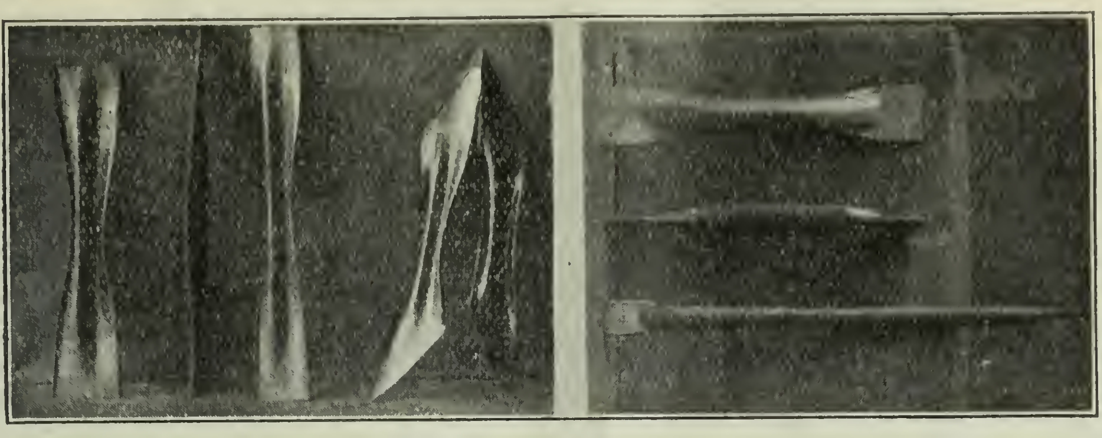


FIG. 24.—*Frame struts.*

A similar behaviour is shown by tubes. Whilst, according to my tests, tubes easily reach the Euler-Tetmajer resistances to axial compression down to a certain thickness of wall (Fig. 13a, $d/\delta \lesseqgtr 50$), this does not hold any more for $d/\delta = 150$. Here we perceive already at a slight compressive stress a local bulging out, causing the tube to give way.

In Fig. 14 is plotted the axial compression breaking length of a number of tubular bars, metal and wooden, exposed to the same compression strength.

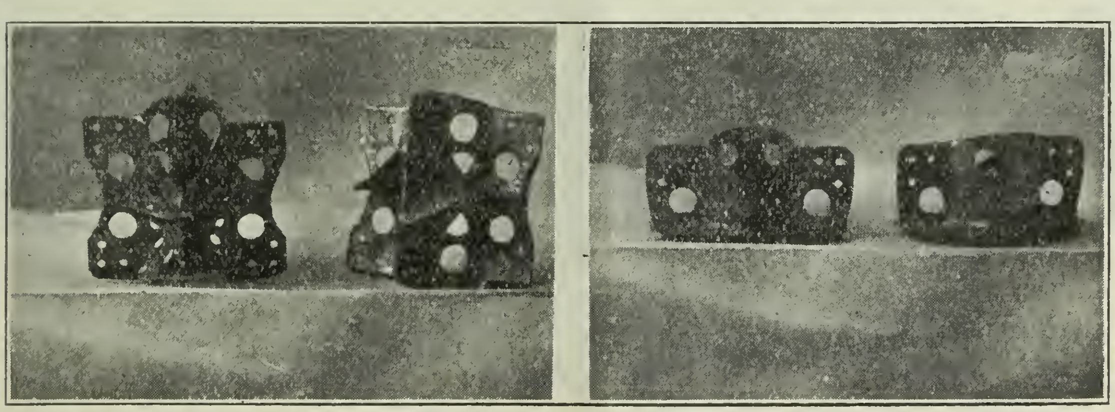


FIG. 25.—*Joints.*

This length can be increased, but only to a certain limit, by a simple enlargement of the diameter (radius of inertia), the thickness of the wall then being diminished in agreement with the constant section of the material. Beyond that limit provision must be made against local deformations. As my experiments have shown, this can be done by the superposition of corrugations on the tube wall, which render the radius of curvature in every part of the tube wall smaller than would correspond to the increased diameter of the tube. In this way a method was found by means of which very light structural parts may be obtained in metal even if they are subject to stress from axial compression (buckling). We have, for example, succeeded in reducing the weight of tubes with a buckling length

= 1 m. under a load of 1 ton to 0.12 kg. by the use of duralumin, whilst a solid wooden bar of the same load and length weighs more than five times this, viz., 0.63 kg.

In the same manner were obtained relations between limiting breaking length under axial compression, radius of inertia and thickness of wall for a variety of shapes and materials.

This preparatory work finished, the construction of a thick metal wing could be started.

In order to absorb, with the use of a minimum weight, the high bending and torsional moments conveying the air forces upon the hull, two principles were applied for the design of the wings, viz. :—

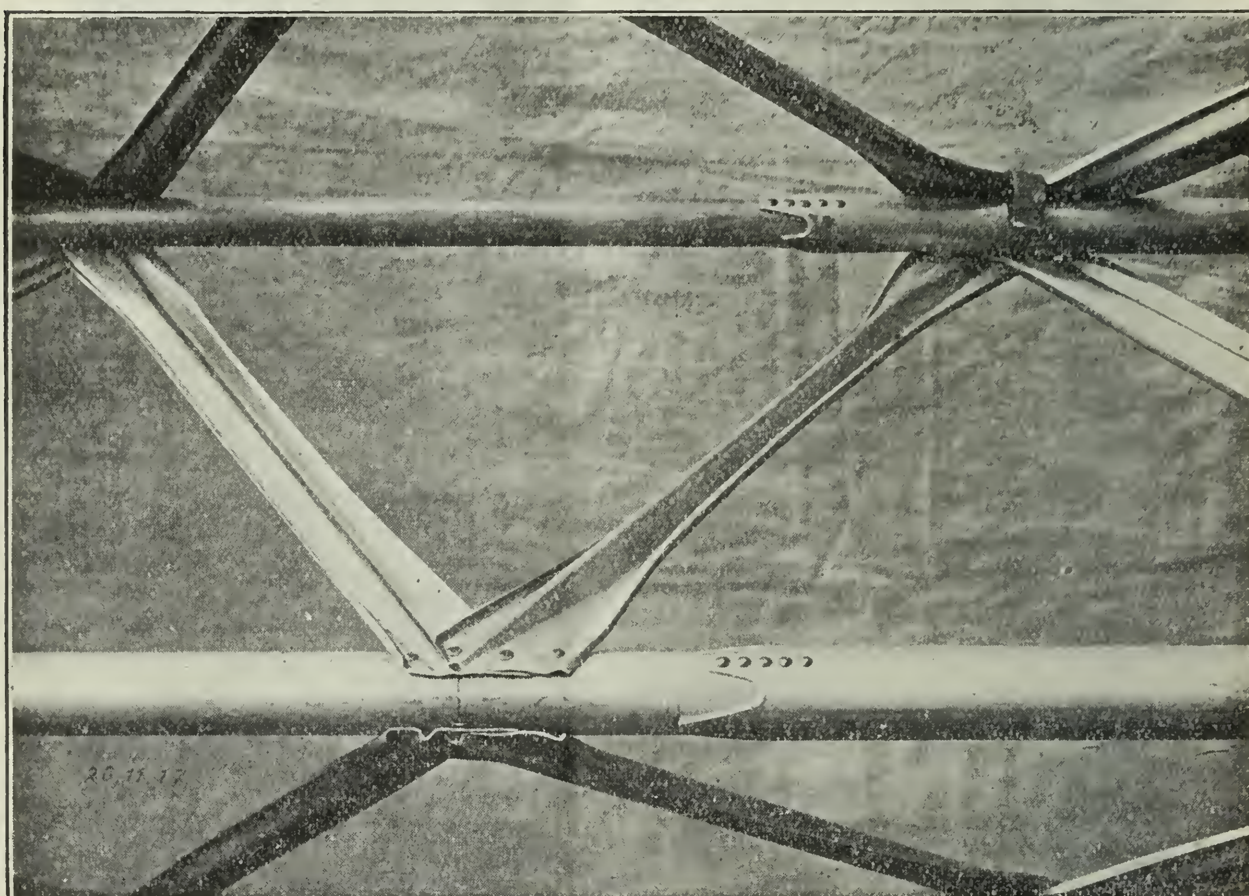


FIG. 26.—*Connection of tubes with each other (telescope joints) and with struts (riveting).*

1. The parts ultimately exposed to the moments were placed as far as possible from the neutral axis so as to give the lever arms of the moments a maximum length and consequently render the corresponding compressive tensile and shearing forces as small as possible.

2. All parts of the material wing cover included must share in the absorption of the moments.

The last named requirement was especially important in the case of iron construction, since the supporting surface had to possess sufficient resistance to local strains, *i.e.*, a certain amount of firmness; but in consequence accounted for a great part of the admissible total weight of the wing.

Thus, the theoretically best design appeared to be the system of the so-called "supporting cover," that is, all tensile, compressive and shearing forces are taken up by the wing cover.

We had at disposal as material of construction sheet iron 0.1 mm. thick used for magnetic purposes, in plates of a very disadvantageous small size and of quite unsatisfactory properties of resistance and elasticity.

To make the surface built of this iron plate resistant and rigid, a second layer of corrugated sheet metal was welded to its inner side (Fig. 16). On the

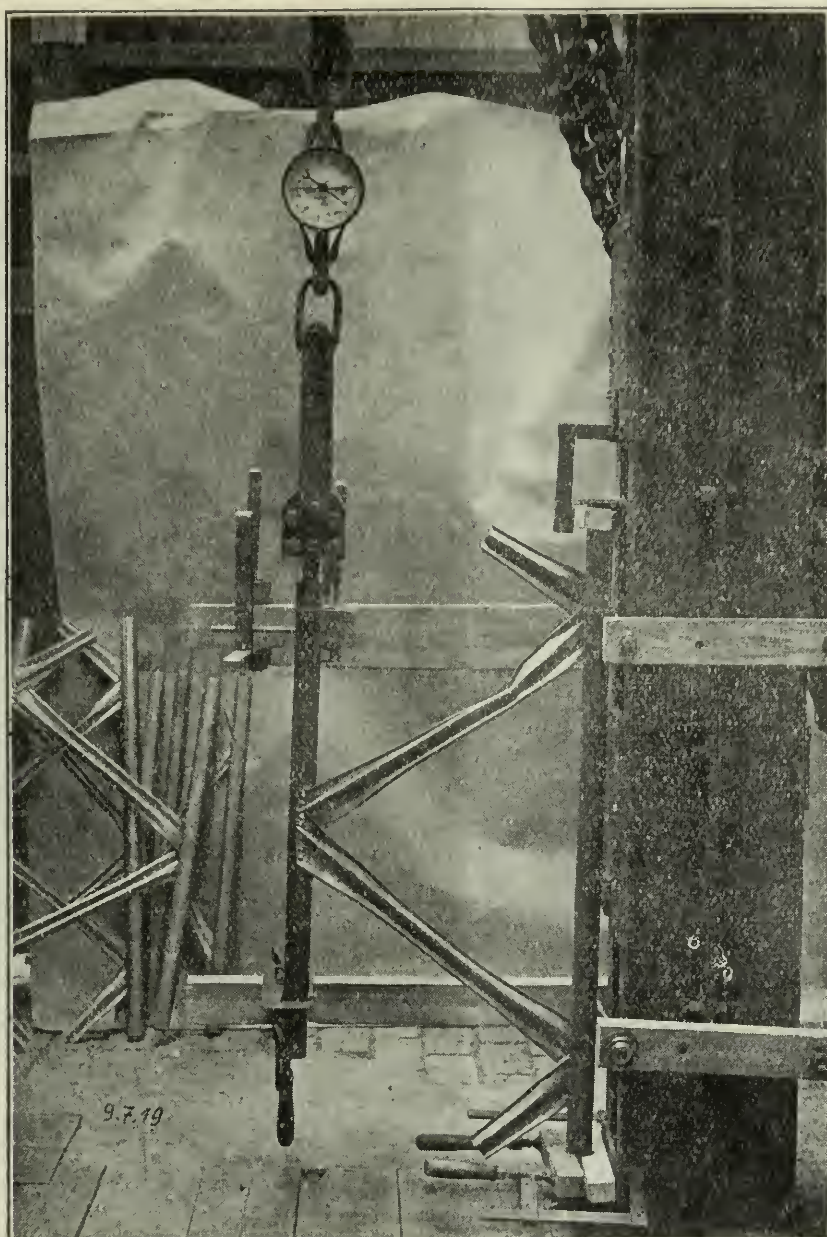


FIG. 27.—*Test of connection between struts and spars.*

basis of the above experiments on axial compression the admissible undulations (radius of inertia) corresponding to the thickness of 0.1 mm. were determined whilst the intervals (viz., the breaking lengths under axial compression) in which the surface had be propped up by bulkheads or ribs (Fig. 17) were given by the dimension of the iron plates.

The short portions or wing sections thus formed were subsequently joined by welding to produce the whole wing. These joints were very difficult. The place of welding had to be reinforced correspondingly to the resistance of the individual wing portions by the insertion of a sort of small iron plate shoe pro-

truding into the undulations; in this way the entire surface was utilised for elastic deformation.

Without due consideration of this requirement, which gives rise to many a difficulty in design and construction, the surprisingly good behaviour of the metal wing would not have been achieved; on the contrary, a splitting up of the surface and the disastrous breaking of the wings due to unsymmetrical loading of the material would have unavoidably ensued.

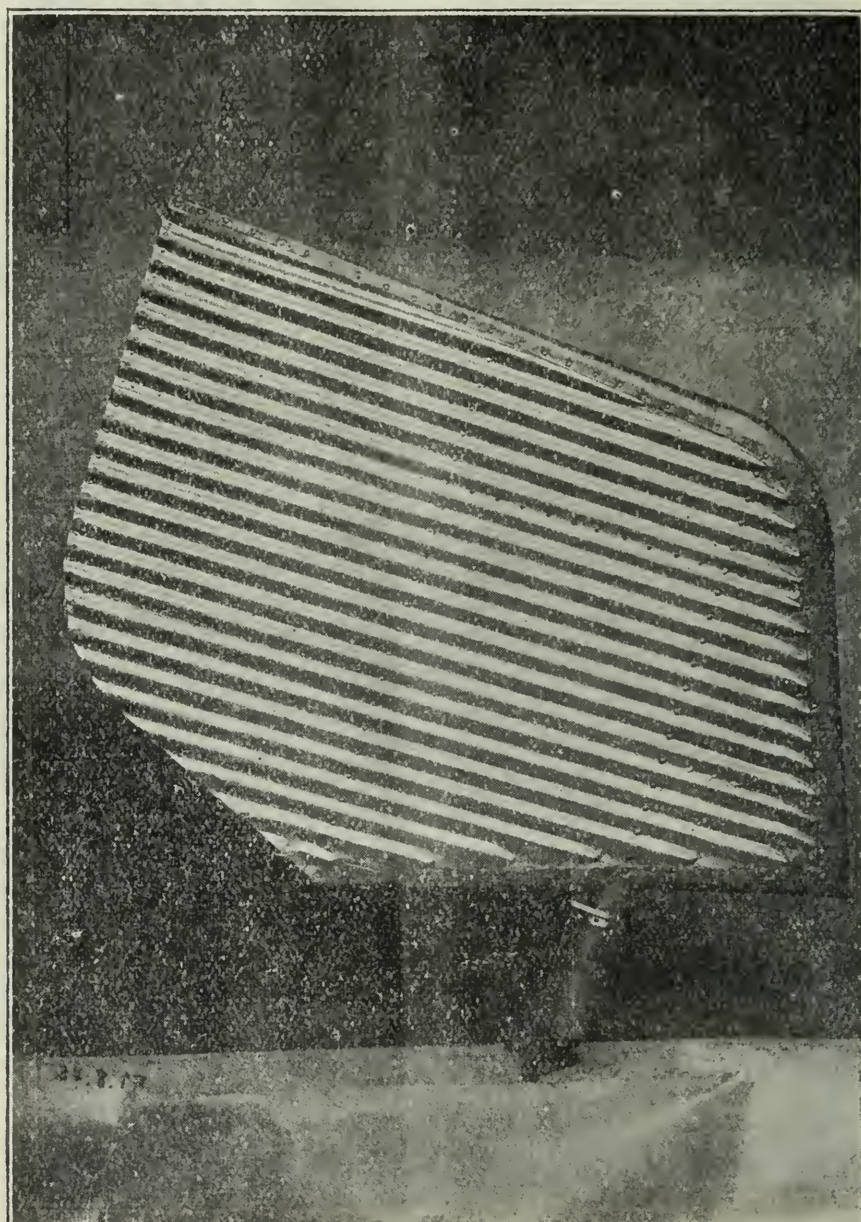


FIG. 28.—*Rudder; illustration of connection of corrugated plate surface to leading and trailing edge of wing.*

After test wings of increasing length had been constructed and tested in the usual manner by sand loading until failure, the building of a whole aeroplane was started, the single-seater monoplane, J1.

Further difficulties had to be overcome in joining the wing to the body of the machine.

Since the space necessary for the location of engine and pilot had to be placed approximately at the point of intersection of body and wing, it could not be

interfered with, and the supporting cover could not be conducted without interruption across the fuselage. Hence the forces of tension and compression distributed over the surface had to be concentrated at special points and be transferred from them in roundabout ways from one side of the body to the other—a task which, on account of the low elasticity of the disposable material, alone proved to be about the most difficult I ever had to encounter in my engineering experience.

Fig. 8 shows one of the many experimental contrivances designed to this end, all of them however yielding no satisfactory solution.

After the lapse of four months (all preparatory work, testing wings, etc., included) the aeroplane was finished, the achievement being so much the greater

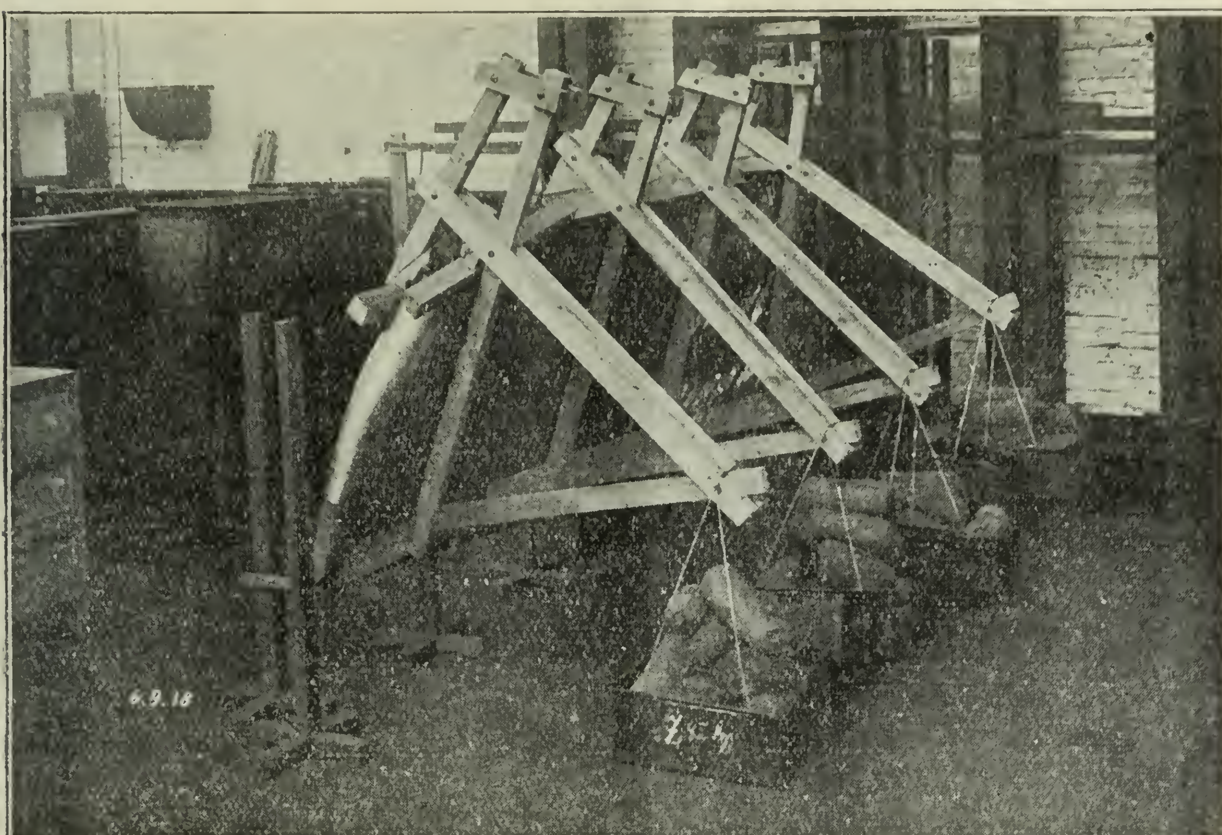


Fig. 29.—*Breaking test of wing for torsion.*

considering the perfect novelty and extraordinary difficulty of the task to be accomplished, much harder in fact than the subsequent duralumin constructions (Fig. 1).

The span of the aeroplane was 12.95 m., the length overall 8.62, the supporting area 24 square metres, the total weight about 1,010 kg. The engine was a Daimler Mercedes of 120 h.p.

The first trial flights were made at Döberitz in December, 1915, and the above mentioned speed test was made in January, 1916, when a speed of ca. 165 km. or 102 miles per hour was attained.

In Fig 19 is plotted an approximate polar curve of the aeroplane based on the above tests together with a few wind tunnel experiments, which do not, however, aspire to great accuracy. This is compared with the polar of the best German plane at that time, the Rumpler biplane R.C.I.

This iron plane has been flown by many excellent aviators, among others by the well-known aeroplane constructor Fokker, who stated in a test flight that its

speed exceeded by 20 km. per hour the fastest machine of that period which had started simultaneously.

Although the welded iron wings fully responded to the requirements of resistance, I abandoned the system of "supporting cover" and altered the construction of the wings entirely. This change had become necessary on account of the great weight of the iron wings, amounting to some 12kg. per m^2 with a length of (one) wing of 6m., and was the cause of a very bad climb of the aeroplane.

This heavy weight was not only the consequence of the metal selected (iron), but also of the particular design which allowed only to a small degree the utilisation of the compressive strength of the material.

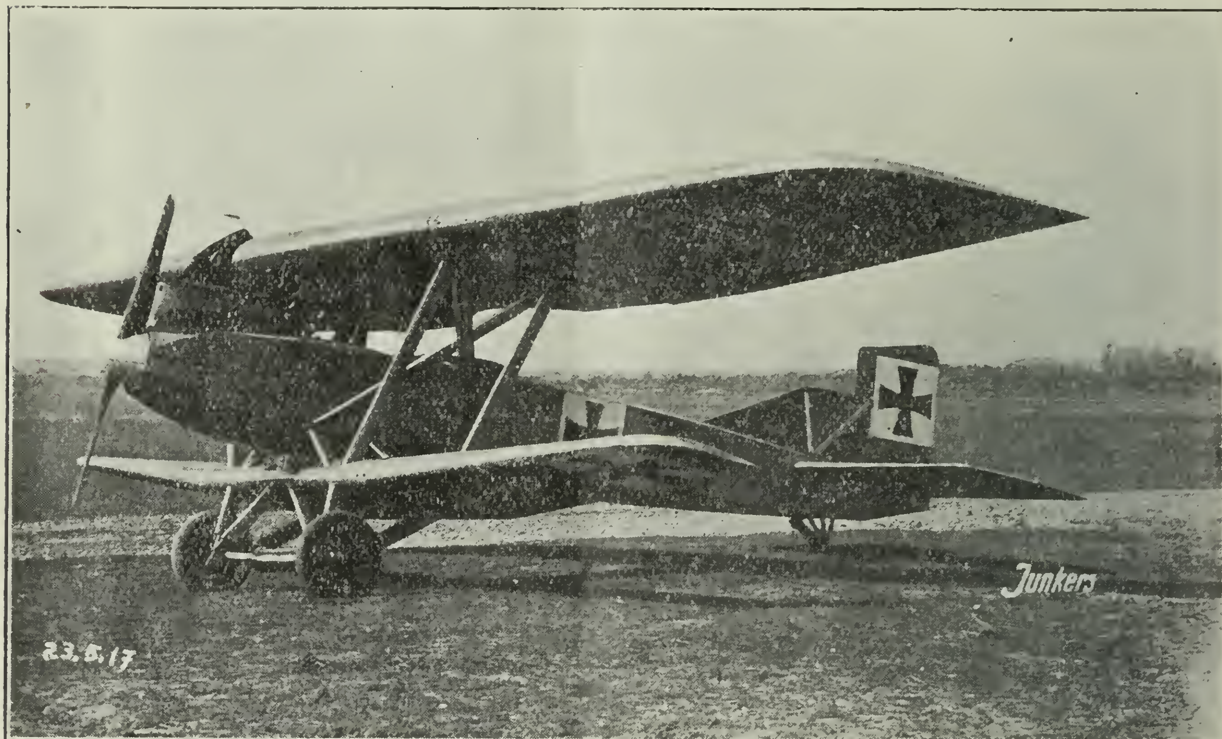


FIG. 30.—*Infantry armoured aeroplane J.4 (Junkers J.1); 200 h.p. Benz; service weight 2,176 kg.: speed 155 km. (97 E.M.)/hour; climb 2 km. in 32 min.; supporting area 50 m^2.*

I have already mentioned that the necessity of using the "supporting cover" (*i.e.*, no internal spars) system resulted from the fact that with the use of iron the weight of the cover capable of fully absorbing the local stress made up nearly the whole of the disposable wing weight.

With regard to the unit of weight, duralumin has a smaller tensile and compression strength than a good steel. It is, however, of lower density, and thus for the same weight (*i.e.*, same tensile and compression load) it will be thicker. It will therefore be possible to make the wing cover lighter with regard to local loading, for the corrugated duralumin will have deeper corrugations than the thinner iron covering of equal weight, and will then be capable of transmitting greater bending moments.

This circumstance made it feasible to work with a simple corrugated plate cover of small weight. Thus we were freed from the otherwise imperative necessity of letting the cover itself partake of the absorption of the high moments, and were enabled to use for the absorption of these moments special structural parts, which on account of their appropriate shape allowed a much

higher stressing of the material in regard to axial compression. Exhaustive tests in this respect proved that as concerns buckling drawn tubes stand best among all section-forms of equal wall thickness.

It was, therefore, soon decided to let the covering surface partake only of the local stress and part of the torsional stress, and to meet the high bending moments by a frame or trestle of tubes in the interior of the wing.

It became necessary to make a close study of the new constructional material in order to accommodate and to adjust design and workshop treatment to its properties. The investigations included resistance of material after varying treatment by heat; so-called fatigue phenomena; corrosion and modes of connecting duralumin parts with each other, etc.

The final result of all this experimental research was that duralumin will meet, when correctly treated, all requirements coming into question. But it must

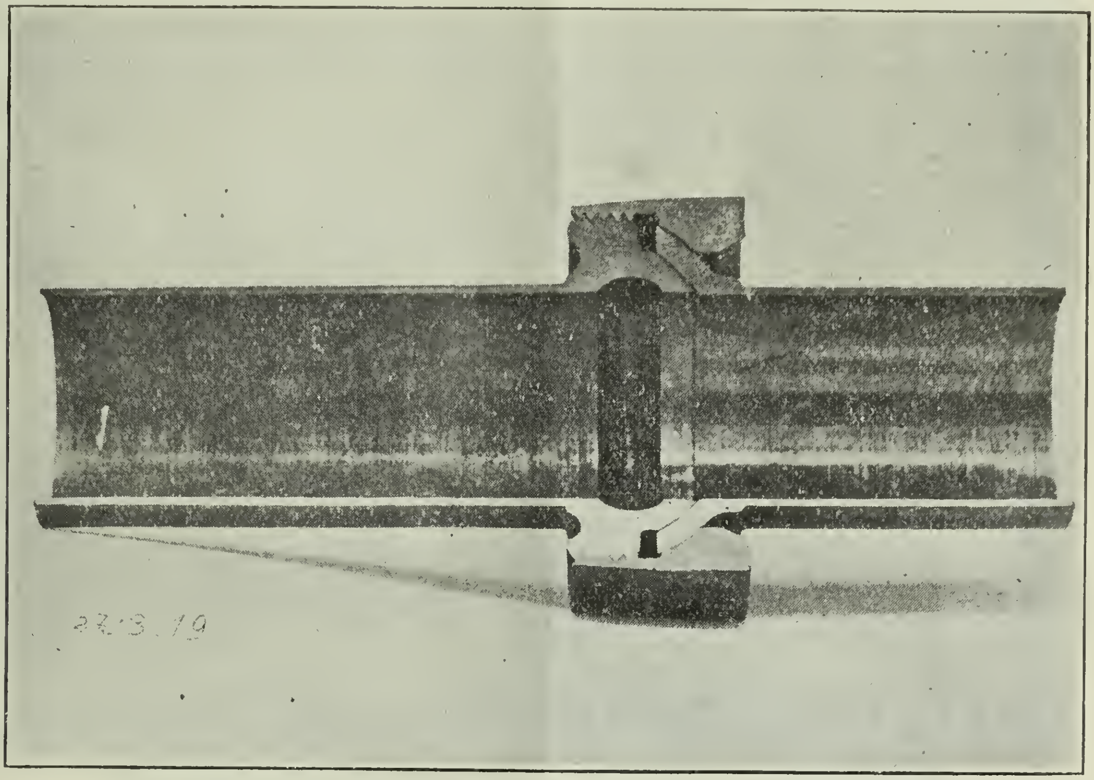


FIG. 31.—*Ball and socket joint between wing and fuselage.*

be said that a good deal of work was necessary to find a suitable treatment by which some of the rather troublesome properties of the material could be overcome, for example, its liability to corrosion. In regard to "fatigue" under varying load, it appeared that duralumin is in no way inferior to iron (Fig. 20 illustrates the arrangement of the fatigue tests). More important is the bad welding of duralumin. We have not yet succeeded in removing the very marked deterioration of the material produced by the welding process, which by itself is quite feasible. But we have found that welding can in all cases under consideration be readily replaced by riveting. The rivets are not loosened to any appreciable degree by the vibrations, to which the whole aeroplane is subject by the work of the engine. We tested this in the manner shown in Fig 21. It shows, for instance, a tailplane made to oscillate by an electromotor, the rotation of the motor corresponding to the impulse-number of the aeroplane engine. The result was that rivets showed no loosening, even after 1,000 hours, and under a stress exceeding the strain occurring in a running work.

But before we could attack the construction of framework wings, there had to be created appropriate designs for such, together with the most suitable forms of construction selected on the strength of systematical tests with respect to resistance, weight and care of manufacture. To such tests were exposed all the single parts, as spars, struts, joints, telescope or pipe clip connections, etc., as well as their assemblages to girders and complete wings.

The first wings tested were of a simplified shape (Fig. 22), the head and tail parts being omitted as immaterial for resistance, and the middle substituted

FIG. 32.—*Ball and socket joint between wing and fuselage.*

by a row of triangular girders connected in parallel. The tests of a series of such experimental wings of increasing dimensions proved that we were on the right track; the construction of regular wings could be attacked.

The basis of the construction is the spar. For these duralumin tubes of suitable diameter and thickness are taken, which thickness must not remain below a critical figure determined by buckling tests described above. If drawn tubes are not in the market in the required sizes, they may be substituted by plates round

and riveted. Experience, as a result of exhaustive tests, has proved, however, that they had to be carefully constructed under expert control.

For connecting two tubes lengthwise telescope joints are used (Fig. 23), after a patented procedure resulting from a long series of experiments.

After that the spars must be connected to the girders. The struts serve this purpose (Fig. 24), appearing either as tubes flattened at their ends, or as so-called Z-shaped struts. The fastening of the struts to the tubes is effected according to circumstances, either by means of special welded joints made of iron and tested for resistance (Fig. 25) or by riveting (Fig. 26).

The resistance of the struts and their connection to the spars is examined on special testing girders by the application of an axial force to one tube, while the other is held fast (Fig. 27).

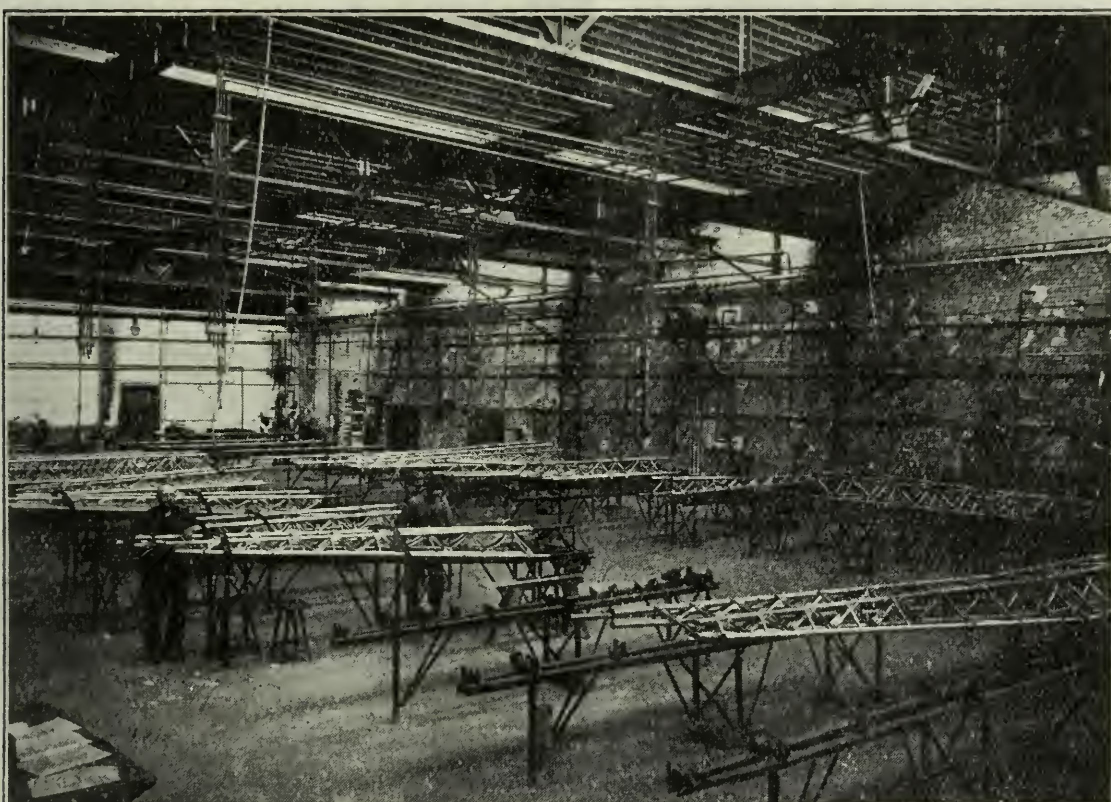


FIG. 33.—*Manufacturing the frame works for J.4.*

The cover of corrugated sheet metal must satisfy the high requirements exacted by aerodynamics and still be of light weight.

Wind channel tests have proved so far that the air resistance on the wings is but imperceptibly increased by the corrugations which augment the frictional surface of the wing by approximately 15 per cent.

From the standpoint of resistance the bending stress, due to accidental and local forces, such as shocks and jerks in transportation, treading upon by men, etc., is of importance. The problem was to find the most advantageous length and height of corrugation with which the wing surface will stand a given load and has a minimum weight; numerous tests were required for this.

The connection of corrugated plates with each other, and more particularly with smooth plates, is not always a convenient job, and often required much constructional acumen combined with great experience in the treatment of duralumin. You perceive a typical instance of such connections in Fig. 28, showing a rudder.

The transition from the smooth sheet metal covering of the leading edge to the corrugated plates upon the lateral areas is clearly discernible, and equally so the formation of the trailing edge.

Fig. 29 shows a loading test for torsion of the completed wing performed until failure.

The first entirely completed aeroplane type in duralumin was the armoured machine J.4, constructed in 1917, the first aeroplane manufactured in larger series by the factory (Fig. 30).

The building of this aeroplane was the result of a request by the Army authorities for a heavy armoured aeroplane, which was to be of the biplane type, as was expressly stated.

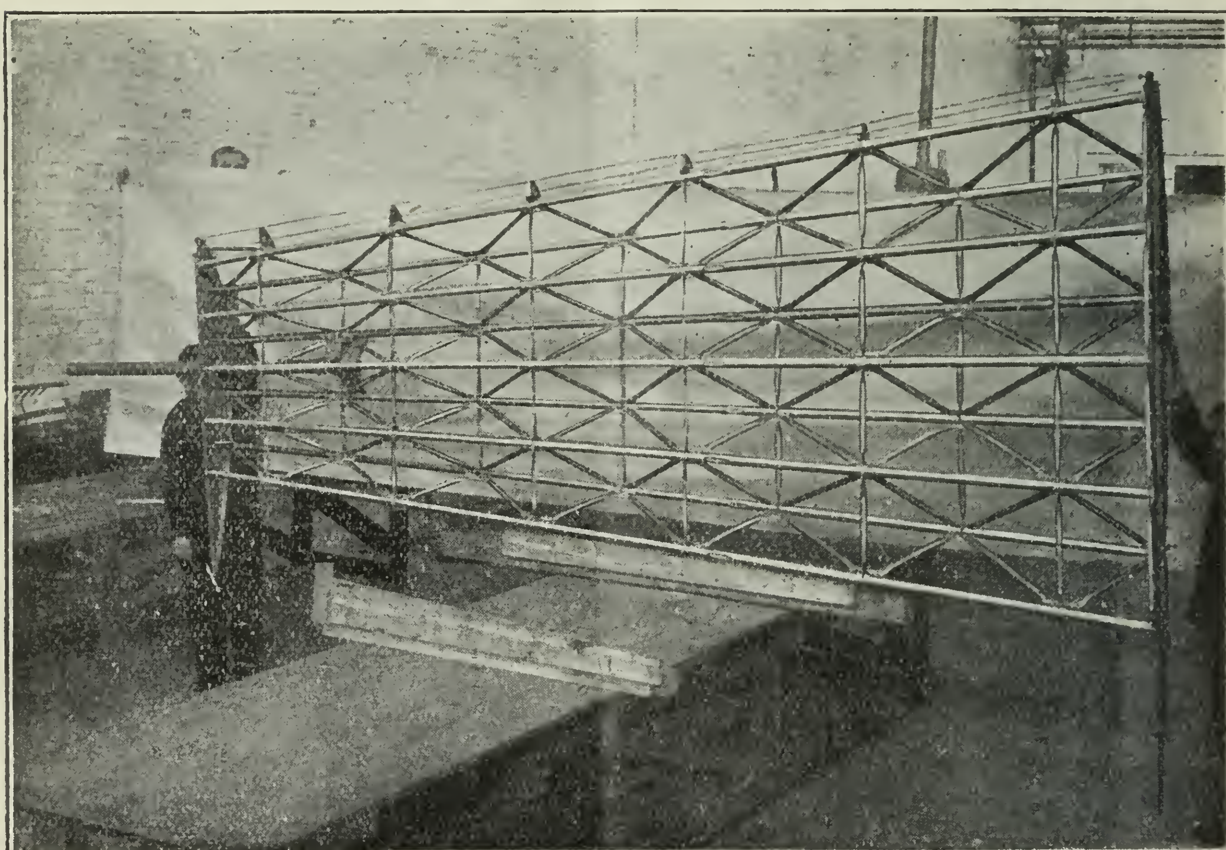


FIG. 34.—*Tube frame work.*

In this plane the fuselage is suspended on both sides to the middle parts of the wings by four struts, a canopy being thus formed. The wing is composed of three sections as a framework wing, the spars running from the wing's tip to the canopy portion, where they are screwed to the spars of the latter by special ball and socket joints (Fig. 31). Each one of those spars represents therefore a direct prolongation of the wing spars. The wing can be detached from or attached to the middle piece in a few minutes (Fig. 32). The screwing joint has shown up well, and has been preserved also in the subsequent aeroplane types.

Fig. 33 shows the method employed in the manufacture of the wings. The spars are put down in special gauges and connected to the girders. The latter are joined to the framework (Fig. 34), upon which is laid the covering surface, and conected to the spars by rivets (Fig. 32).

The riveting of the struts and of the corrugated plate-cover to the tubes is done by a special patented procedure, the rivet head being made in the interior of the tube by counterpressing an aurol pushed into the tube (Fig. 36).

A feature of the armoured machine is the peculiar armoured box (Fig. 37), which served as backbone of the fuselage structure and proved such a success that in many a nose dive and capsizing, both engine and crew remained untouched.

The apparatus is constructed for safety from distant firing. The construction was statically indeterminate (redundant parts), and the spars could be damaged repeatedly without impairing the flying capacity of the machine. Fig. 38 shows part of a wing of such an aeroplane which has stood more than 400 hits without failing.

The weight of the aeroplane was 2,176kg., with a supporting area of 50 sq. metres, the engine 200 h.p. (Benz) being, however, perceptibly overloaded.

In spite of widespread prejudice, I did not refrain from the further study of the monoplane. It was of the utmost importance, therefore, to revise the relative position of the fuselage to the wing.

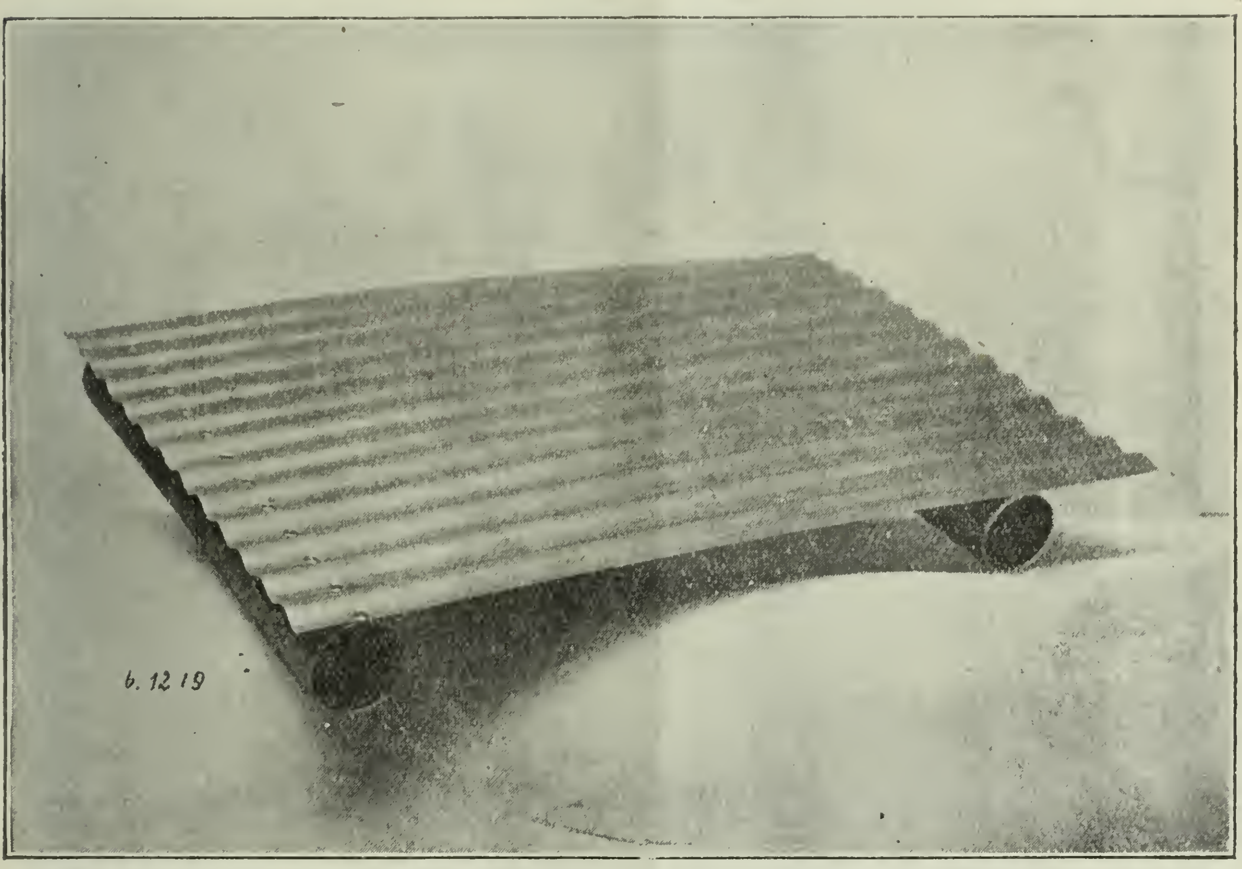


FIG. 35.—*Sample of corrugated plate cover to the spar.*

From a constructional standpoint, it is important to have the wing built as a solid unbroken structure, not interrupted by the fuselage, upon which structure the body is set directly, as in the low-decker, or to which it can be suspended, as in the parasol.

For the next aeroplane type, the pursuit single-seater J.7/J.9 (of 1917), I chose the arrangement of the low trim (Fig. 39). The middle part of the wing is developed into so-called " central framework," to which the part carrying the fuselage and the engine is immediately superposed. Such a central framework is represented in Fig. 40. The struts necessary in a " parasol " machine are here omitted, and by this means both air-resistance and weight are diminished. But the chief merit of the design lies in the safety ensured to the crew by the deep trim of the wings in case of bad landing and a damaged landing gear. The

wings are first to come in contact with the ground and absorb a great part of the shock. This behaviour of the machine has been observed in numerous instances.

The low trim of the wings was at the time of its first appearance a complete innovation, which in spite of its undoubted constructional merits had given rise to

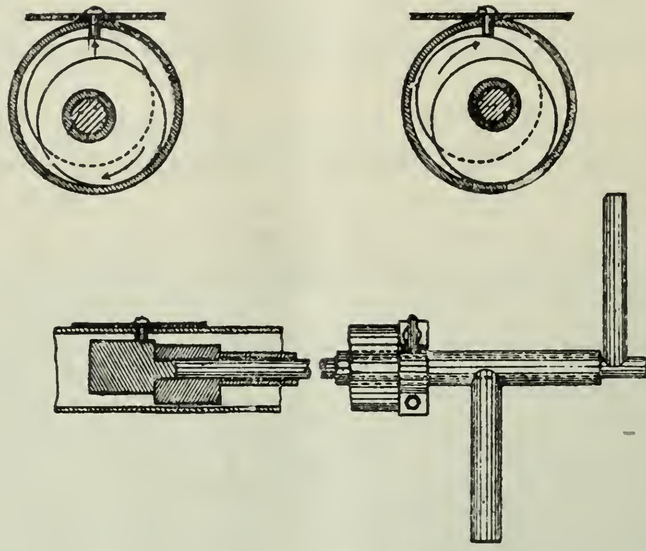


FIG. 36.—*Device for rivet connection of tubes and plates.*

strong objections. It was in particular contended that the high position of the centre of gravity might prove a cause of detrimental lateral instability, and flying the machine was at first considered a risky procedure. All such scruples were fully refuted by the success of the machine.

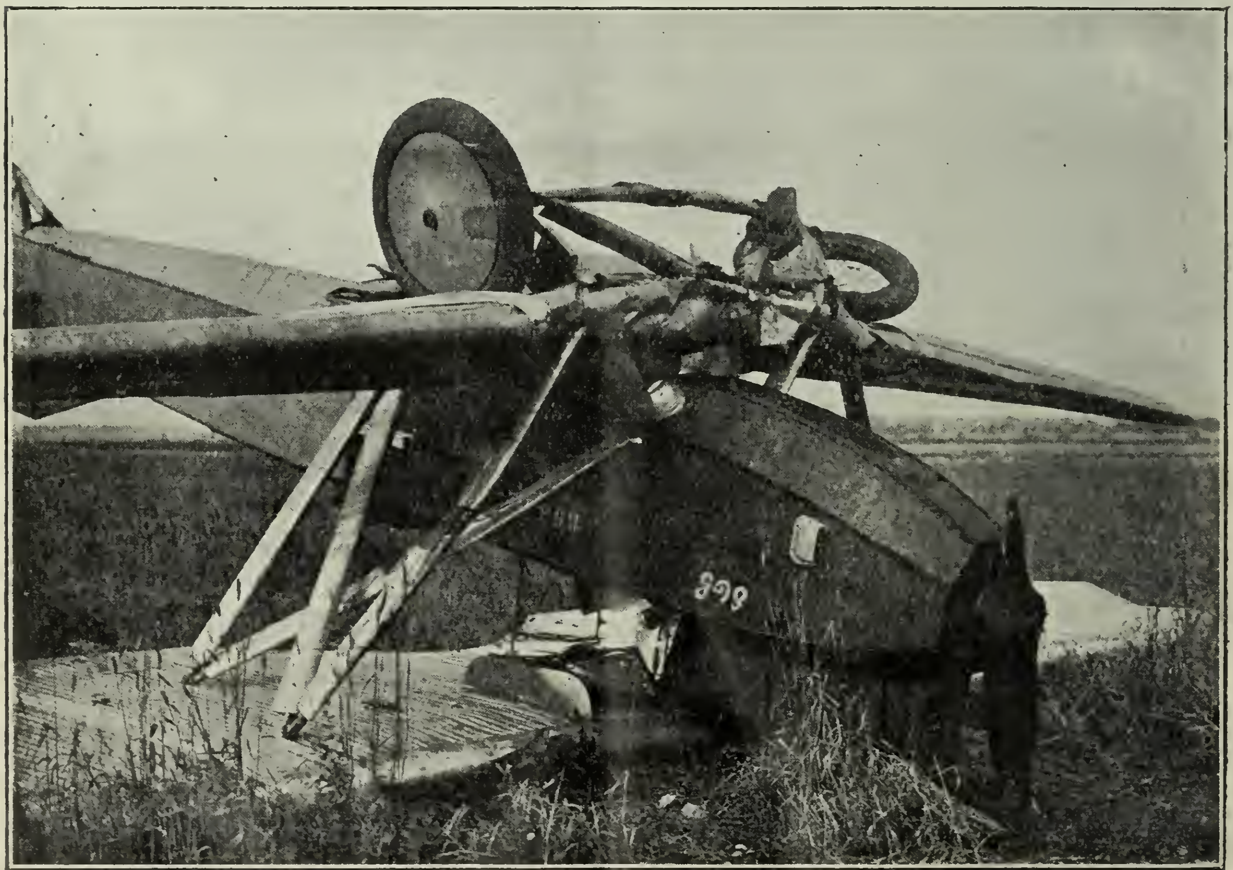


FIG. 37.—*Armoured box for J.4.*

The question had been broached, which of the two arrangements of the wings should be given the preference from the standpoint of aerodynamics—the parasol or the low wing trim.

My wind tunnel tests for the respective wings had first yielded results tallying with those lately published by the Göttingen Institute: that the trim of the wings

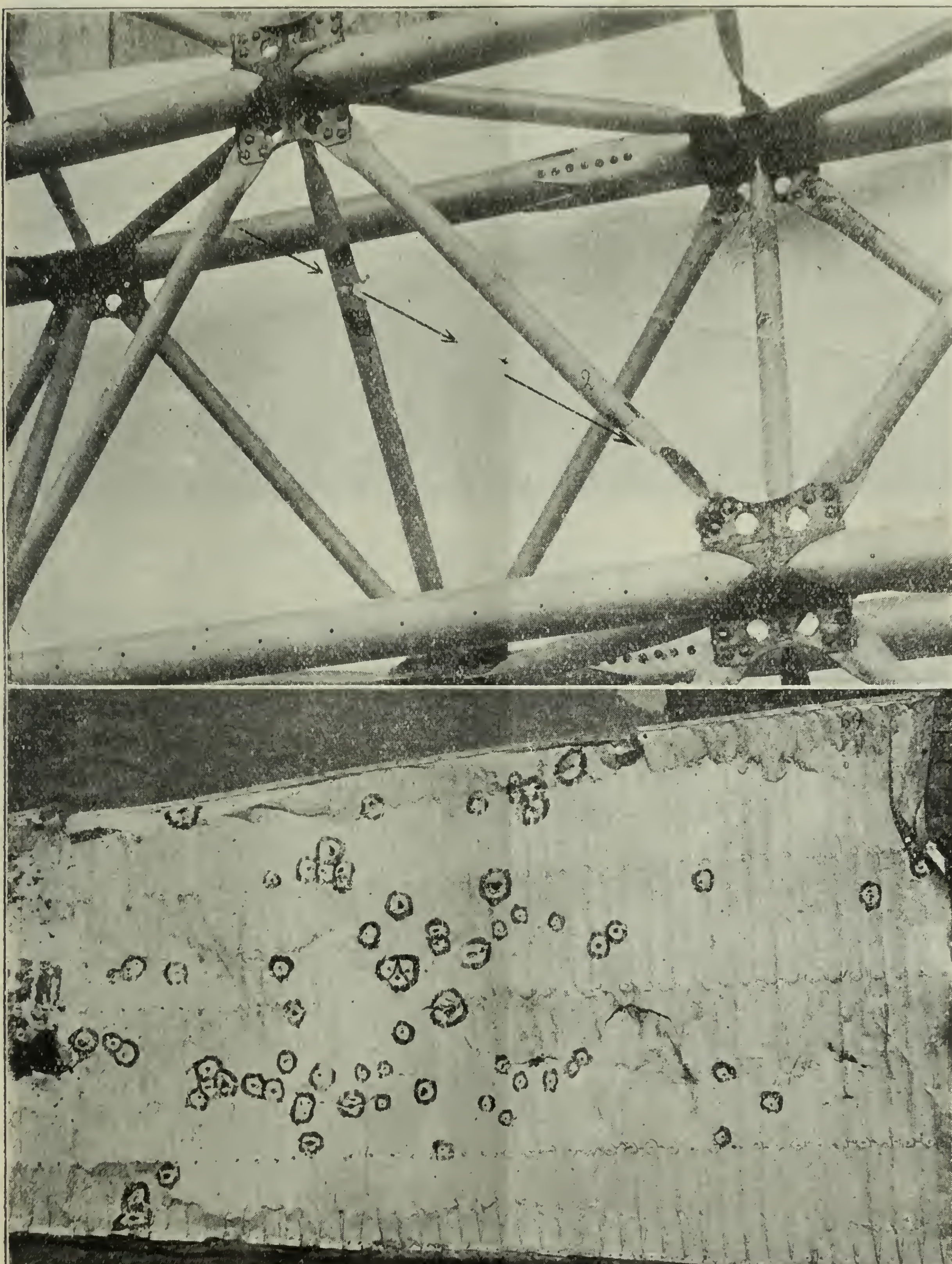


FIG. 38.—*Shots on J.4.*

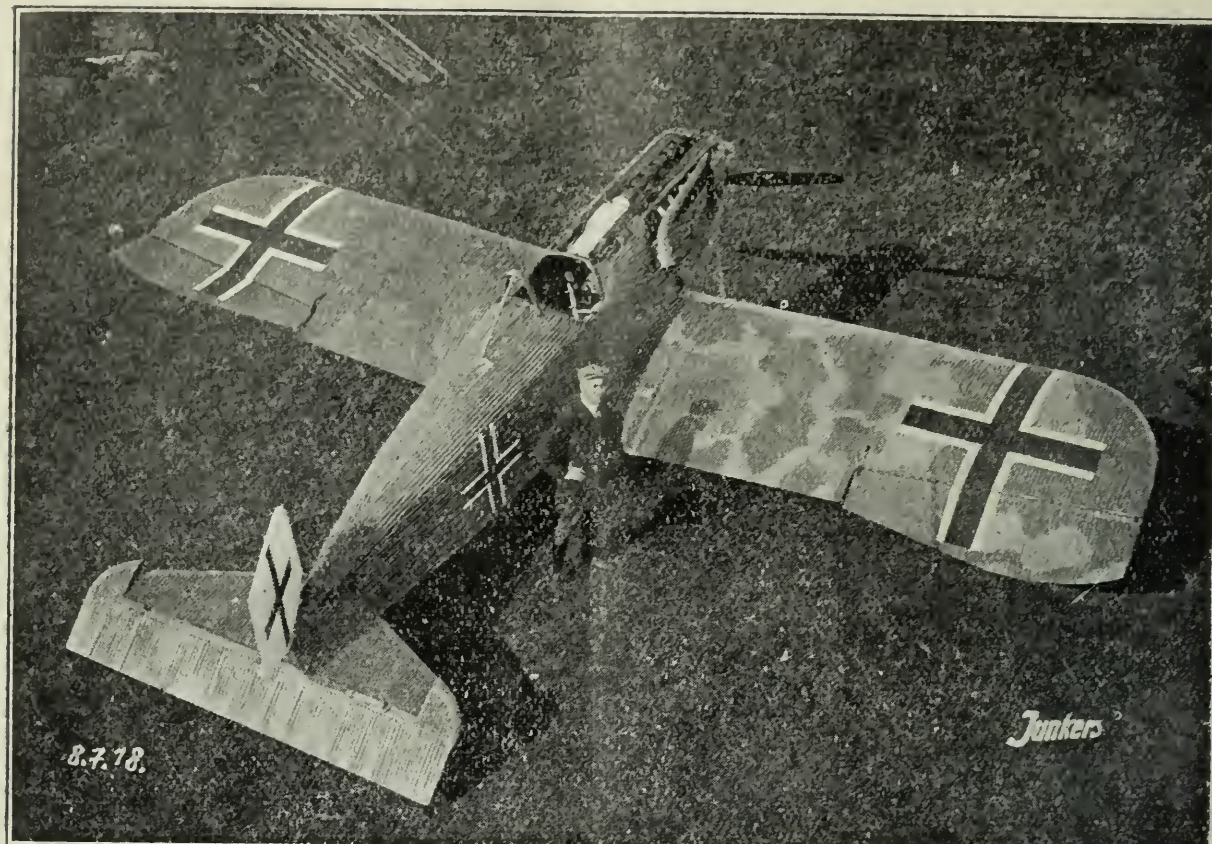


FIG. 39.—*Fighting one-seater J.7/J.9 (Junkers D.I.); 185 h.p. B.M.W.; service weight 835 kg.; speed 240 km. (150 E.M.)/hour; ceiling 6 km.*

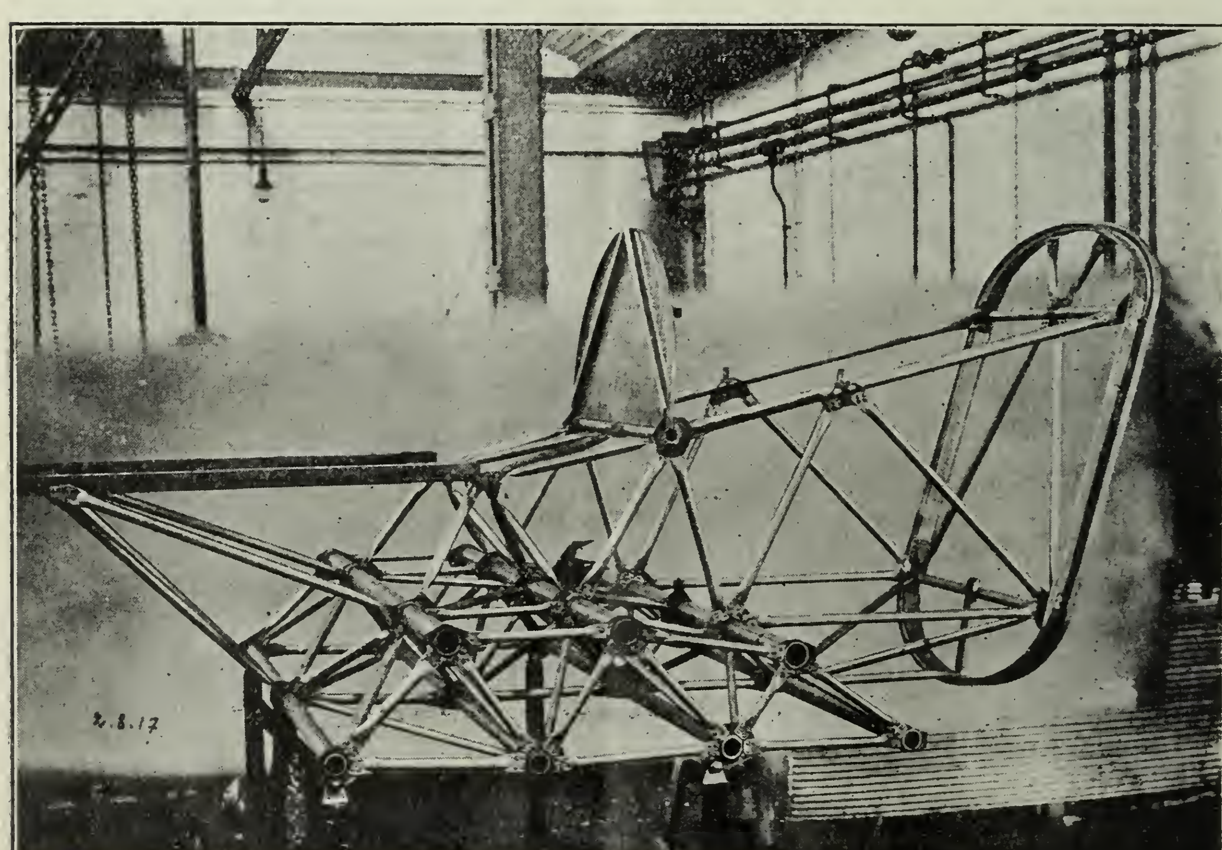


FIG. 40.—*Central frame work for J.7.*

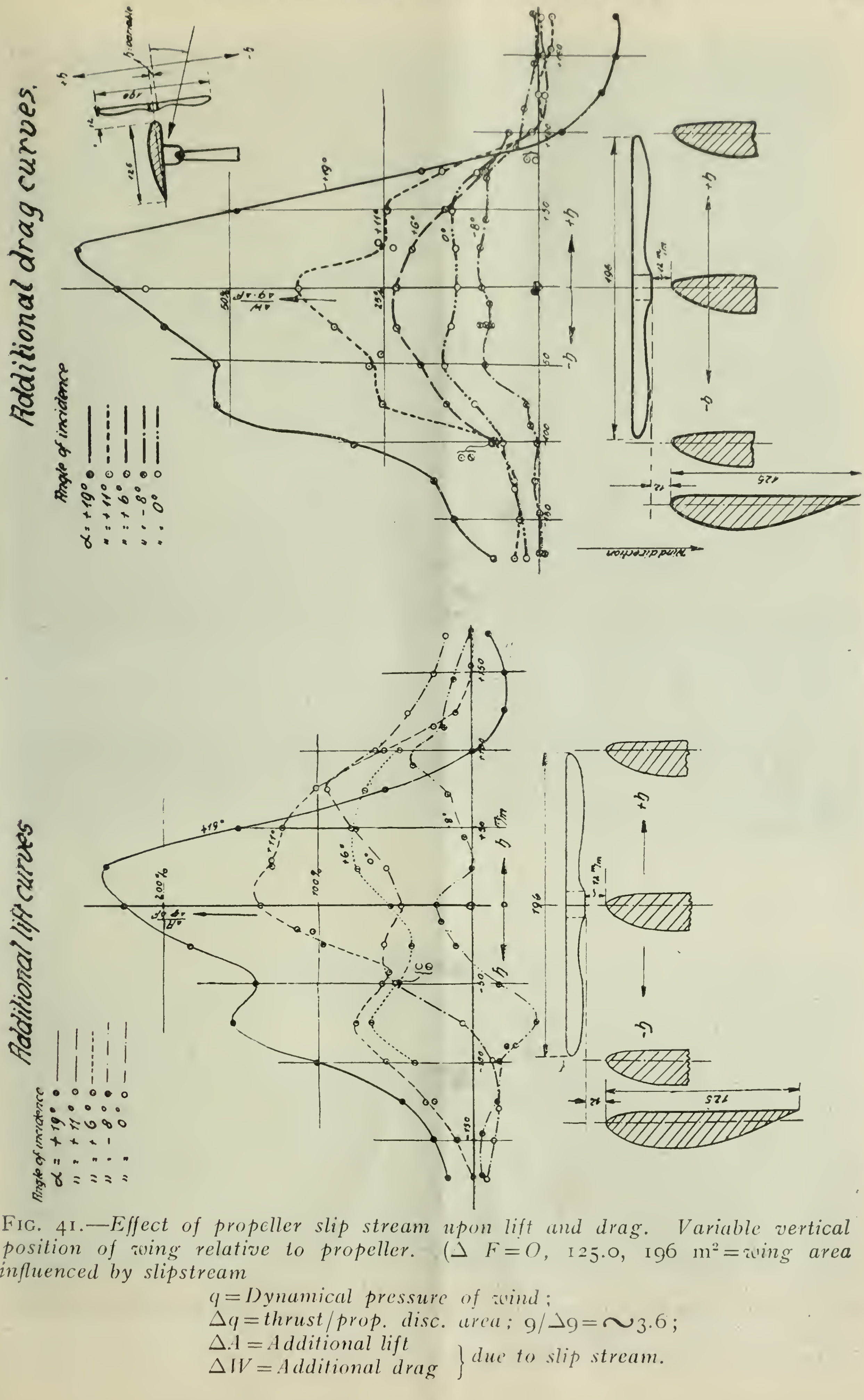


FIG. 41.—*Effect of propeller slip stream upon lift and drag. Variable vertical position of wing relative to propeller.* (Δ *F=O,* 125.0, 196 m² = *wing area influenced by slipstream*

q = *Dynamical pressure of wind;*
Δq = *thrust/prop. disc. area;* 9/Δ9 = $\sim$3.6;
ΔA = *Additional lift*
ΔW = *Additional drag* } *due to slip stream.*

above the body or level with its upper edge gives a better ratio lift-drag than the trim under the body. It is most useful, if the smooth run of the upper wing surface be not disturbed either by superstrictures or apertures. In this sense the parasol type has the advantage.

In subsequent experiments the effect of the propeller slipstream upon lift and drag has been considered. Fig. 41 shows such a test. We discern that the additional lift produced by the airscrew is higher when the screw is placed above the wing, and the additional drag lower. The same results were arrived at with other positions and sizes of the screw, particularly also with pusher propellers, likewise in the presence of a fuselage.

Later calculation shows that, especially with a very strong propeller slipstream,

FIG. 42.—*Navy two-seater J.11: 185 h.p. Benz; service weight 1,420 kg.; supporting area 27 m*^{2}*; speed 175 km. (108 E.M.)/hour; climb 3,000 m/26 min.*

i.e., in aeroplanes of very high engine power, the favourable influence above mentioned can compensate for the aerodynamical drawback of the high trim of the body.

Since then I have worked on both methods.

A two-seater quite similar to the J.7 was also produced, and later on changed into a hydroplane (Fig. 42) by the addition of floats. This was our first step in the development of the hydroplane.

The choice here made in favour of the two-float plane instead of the flying boat which is preferred abroad, came quite naturally since here the question was to progress as quickly as possible. It may be that from purely aerodynamic considerations the flying boat is superior, and that it also fulfils better our æsthetic requirement for a construction which forms an organic whole.

From the point of view of production, however, as well as of operation, the possibility of changing, at least for small and middle-sized types, a land aeroplane into a hydroplane by the simple substitution of floats for landing wheels and *vice versa,* is of first-rate importance. In case of damage the exchange of a float is also easily accomplished, while the flying boat must, for each repair, be removed

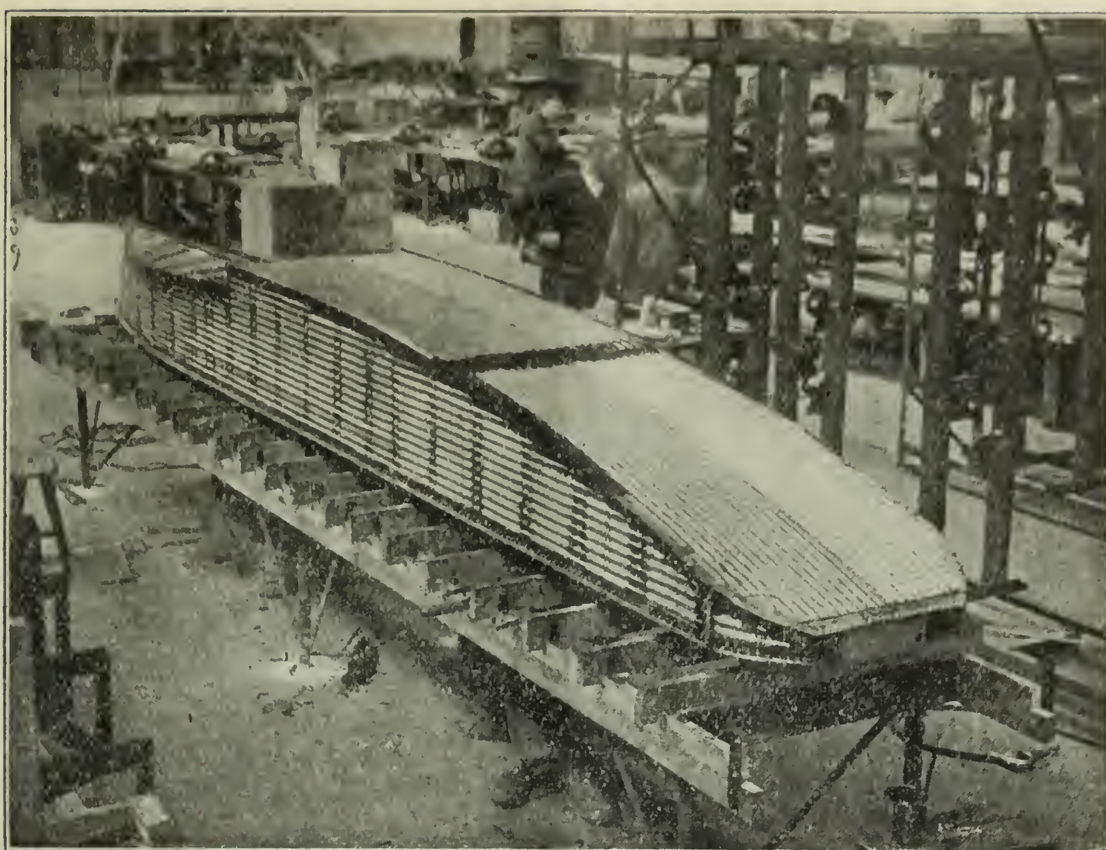


FIG. 43.—*Duraluminum float.*

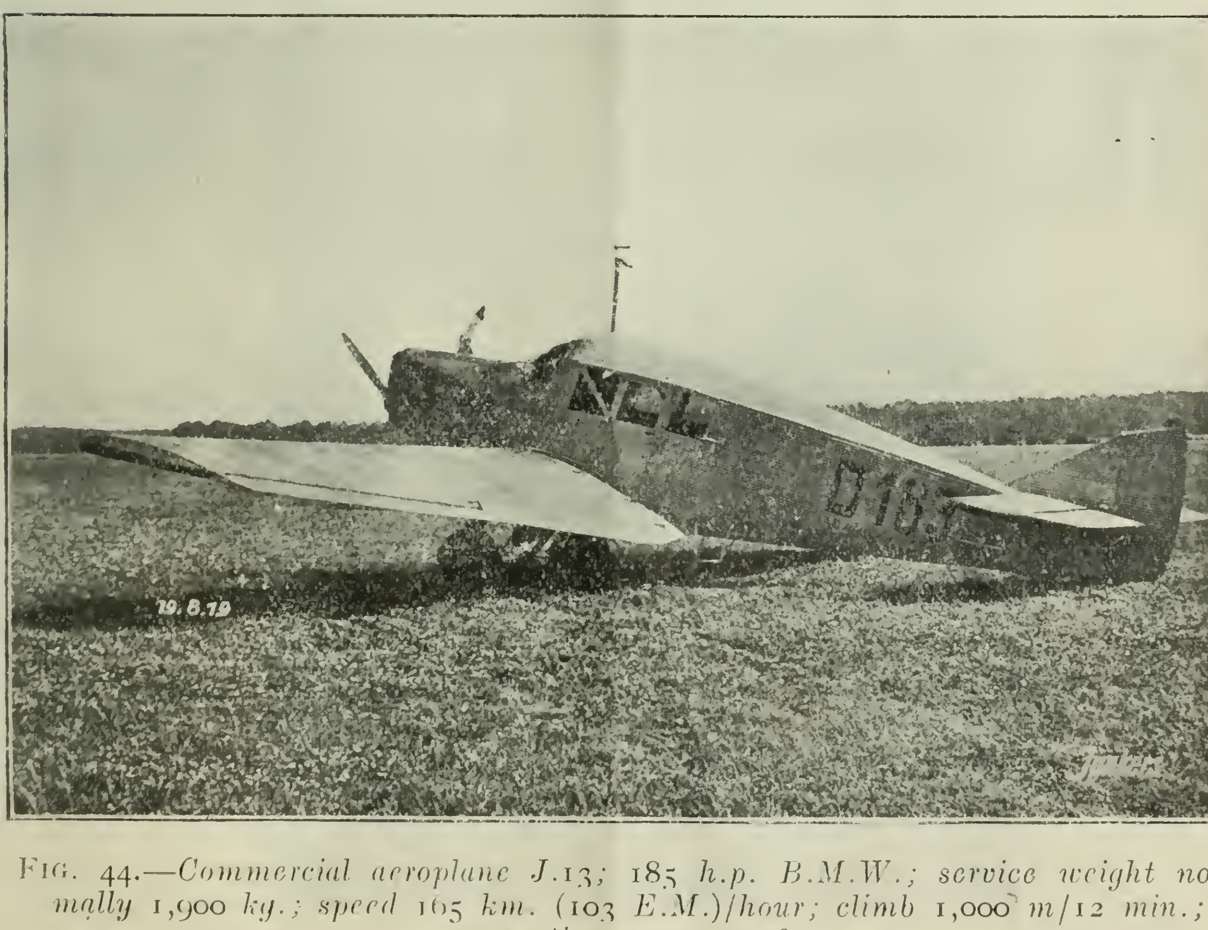


FIG. 44.—*Commercial aeroplane J.13; 185 h.p. B.M.W.; service weight normally 1,900 kg.; speed 165 km. (103 E.M.)/hour; climb 1,000 m/12 min.; supporting area 40 m^2.*

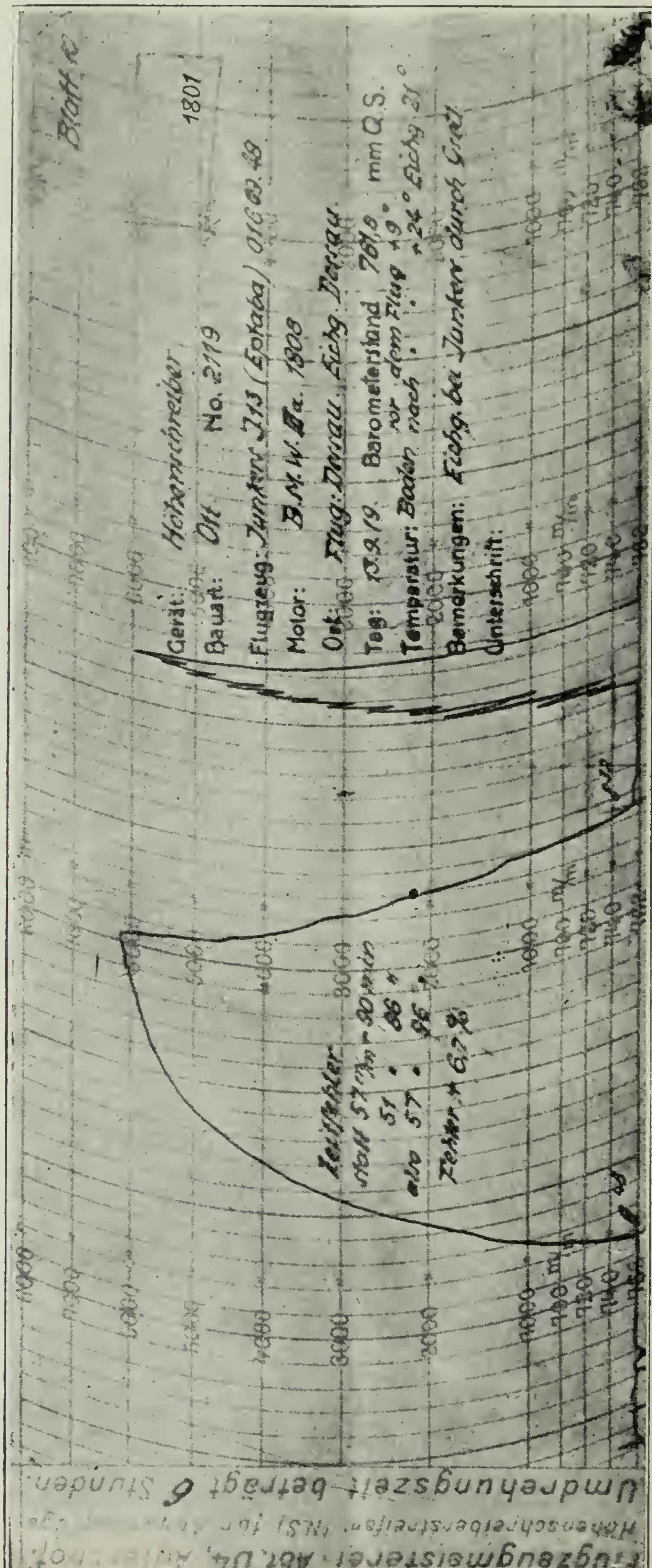


FIG. 45.—Pressure registration of altitude record flight.

from work for a longer space of time. As regards seaworthiness, the float plane is not in any sense inferior to the flying boat.

Our float aeroplanes have done very well in practical service, equally as concerns flying performance—I might quote here the results of the Tyrrhenic Cup Competition of Naples in 1922—or as regards resistance and excellent watertightness of the floats, manufactured of duralumin like the wings (Fig. 43).

J.7 was the last war aeroplane made by us. After the armistice we set up a programme for the construction of commercial aeroplanes. It comprised:—

A giant aeroplane of 740-1,000 h.p.;
A middle-sized commercial machine of 160-185 h.p.; and
A small type for the same purpose of 50-70 h.p.

The first one finished was the well-known commercial limousine six-seater, J.13 (Fig. 44). Its characteristic feature lies in the solution of the problem of

FIG. 46.—*Commercial J.13 on sledge runners.*

creating a large hollow space for the passengers and at the same time of achieving enormous progress in regard to economy.

The aeroplane showed already in its first longer flight, with a 160 h.p. Mercedes, the average speed of 165km./h. (102 miles), certainly a good performance for the low power. It also has on its record the flight of September 13th, 1919, when, with a 185 B.M.W. engine and eight persons (525kg. useful load, this figure not representing, however, the total amount of the available load of the machine), a ceiling of 6,750m. was reached in 86 minutes (reduced figures, 6,920m. in 81.6 minutes). The record is corroborated by a self-registering barometer, and officially entered (Fig. 45).

You might have heard that this commercial aeroplane has been used from tropical Columbia in South America up to the snow fields of Northern Canada and Finland. It has proved to be utterly satisfactory in all its principal features; the drawbacks, which became apparent, were easily removed.

When it first came out it was superior in economy to most of the types extant, even now it cannot be considered antiquated, and has proved fully able to compete when the prohibition against flying was abolished.

By the appliance of runners (Fig. 46) the machine has been adapted also

to starting and alighting on snow, and has been used successfully in this capacity; Amundsen has taken along such an aeroplane on his last Polar expedition.

J.13 has also done satisfactory work as a hydroplane, with attached floats (Fig 47). In this quality it also comes in consideration for use in the tropics, where the surface of the water frequently represents the only available landing place. So we have but recently had a report from the Sociedad Colombo Alemana de Transportes Aereos, in Columbia, according to which the regular air traffic of the Company with J.13 hydroplanes has gone on in 1922 without a single accident.

The small aeroplane was executed in two types. The one is a cabin aeroplane, J.16 (Fig. 48); most of you will be already acquainted with it by paragraphs in the papers. The second shape is that of an open aeroplane, J.19 (Fig. 49). Both are of the parasol type, the above mentioned aerodynamical superiority of which I could not well forego when account is taken of the low engine power.

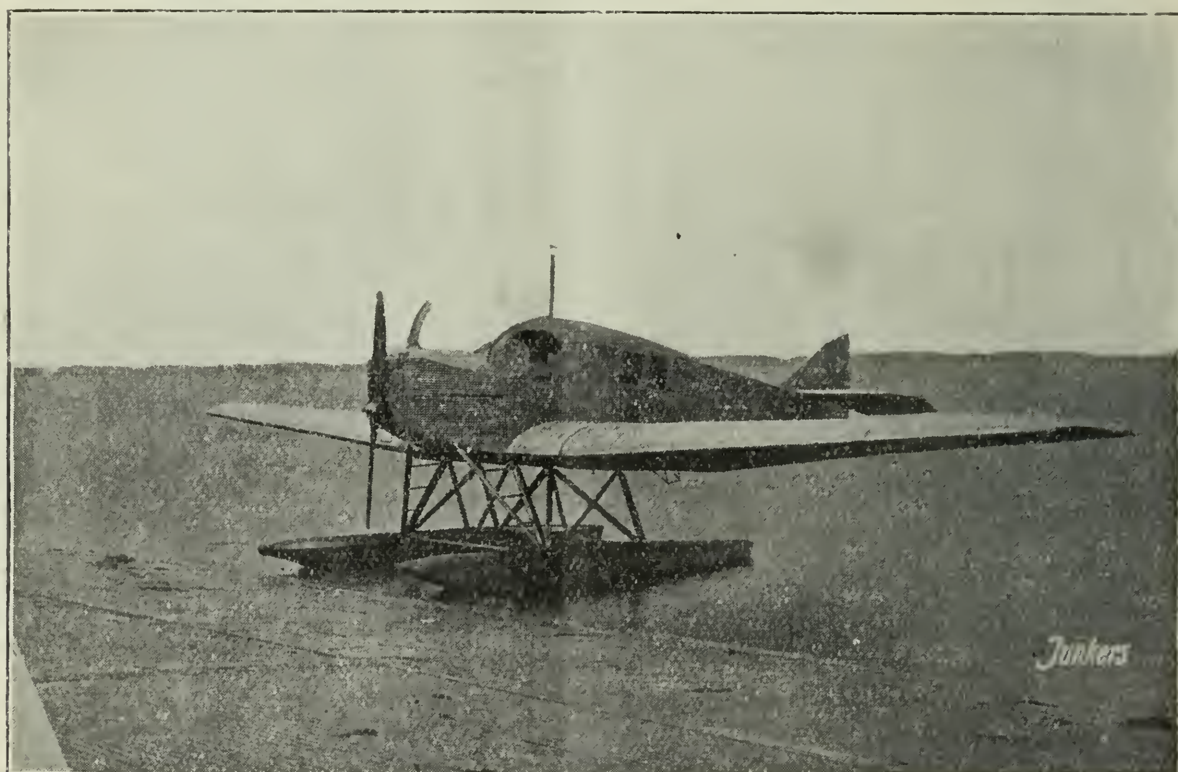


FIG. 47.—*Sea plane J.13.*

But the advantages of metal construction become relatively more pronounced in the large aeroplane than in the small one. Only in this case can the principle of locating within the wing all parts giving resistance, be carried out on a large scale. In my opinion the commercial aircraft of the future will be the fast giant aeroplane, and for this reason I have quite early begun the preparatory work towards the construction of such a craft. Fig. 50 shows the design of an "R" aeroplane of the year 1917, with an engine power of four times 260 h.p. It is a war machine. The engines are housed in the thick wing and readily accessible.

Fig. 51 presents a project, designed in 1918, of a flying boat of four times 1,000 h.p. The aspect ratio is here already distinctly higher than in the preceding design.

The engines provided for the last named giant plane are opposed-piston heavy oil engines, at the development of which I have been working for many years. In this case the horizontal arrangement would be particularly appropriate, as readily housed in the wing.

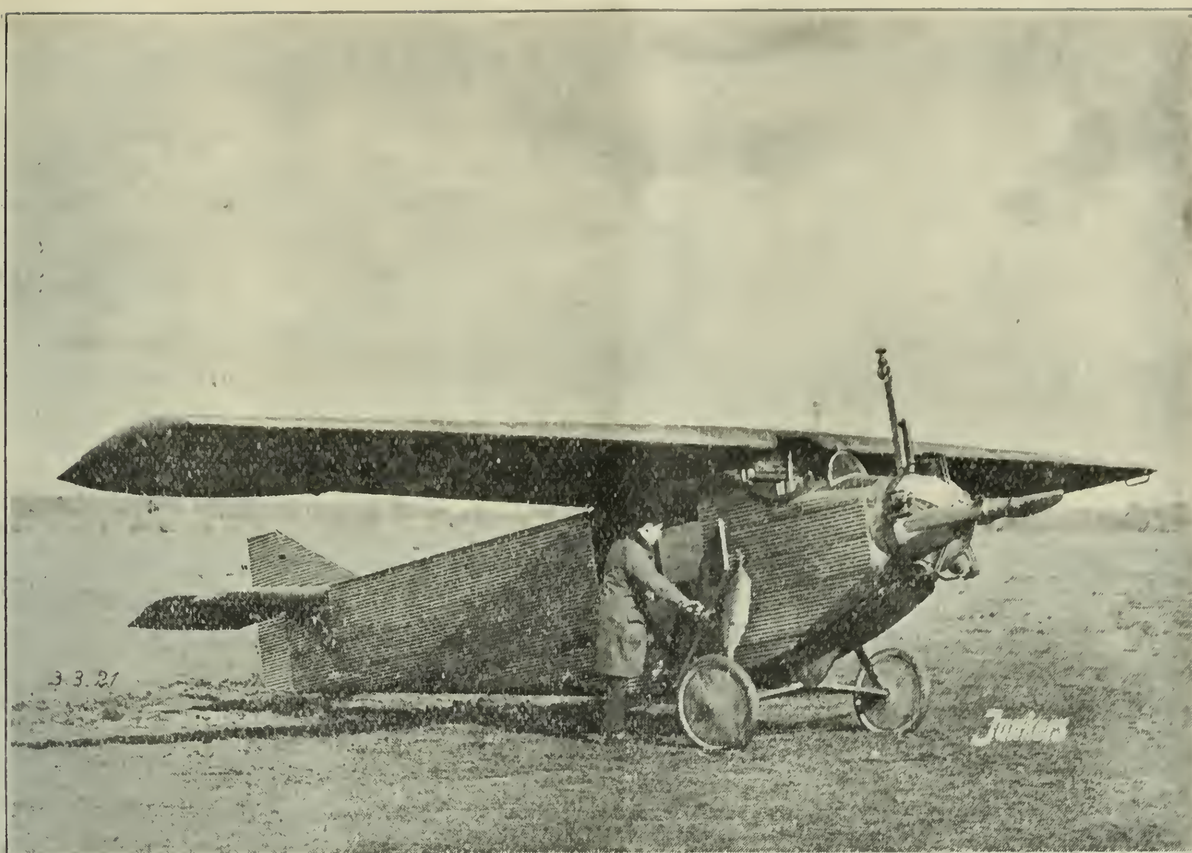


FIG. 48.—*Covered little commercial plane J.16.*

FIG. 49.—*Open little commercial plane J.19.*

My experience in engine construction has satisfied me that the heavy oil engine is especially fitted for the aeroplane. Setting aside the greater economy and less danger from fire, in which qualities the heavy oil engine forms a necessary complement of the metal aeroplane, it is also more reliable in operation and easier to control than the carburettor motor. It is therefore also more fit to be developed than the last named, and I hope that my engine factory will be able to contribute to its evolution in the direction determined upon.

Not a single one of the named giant plane projects has so far been constructed, owing to the clauses of the Peace Treaty, *i.e.*, on account of the great risk caused by the uncertainty in the interpretation and execution of these clauses. The building of a large aeroplane, meant as a precursor to the giant craft (Fig. 52 shows the assemblage of it, and Fig. 53 part of the wings), has been stopped since 1921 for these reasons.

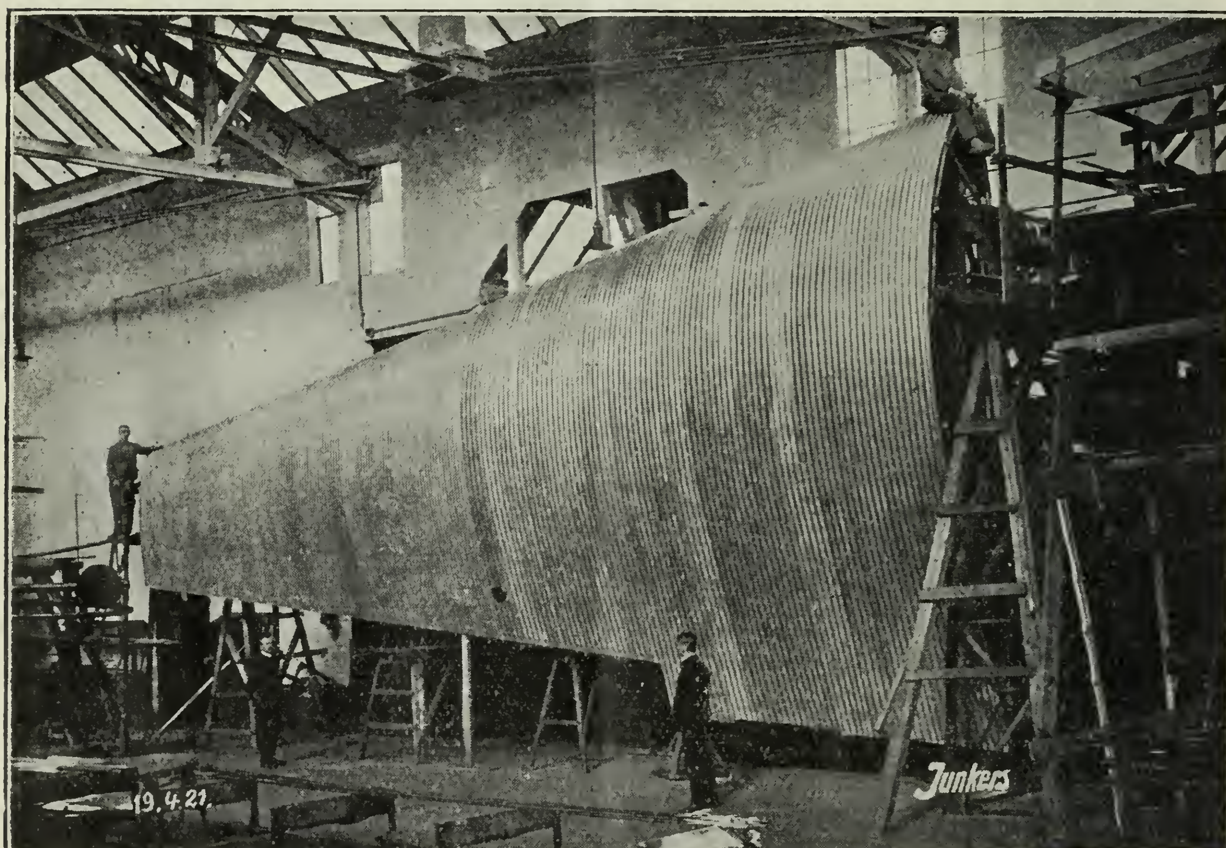


FIG. 53.—*Part of wing of the large plane J.G.1.*

In recent times the work on giant planes has to be confined to occasional but certainly valuable preparatory studies, and the development of detached parts, this work representing, however, very useful material for a subsequent swift and safe construction of giant aeroplanes in metal.

Whether this prohibition of the construction of giant planes, due to political scruples, is in the interest of the nations foremost in civilisation is a question not to be discussed here. At any rate those nations whose widespread colonies render a fast means of communication highly desirable within their confines as well as with one another and the mother country, will certainly be most interested in the development of the aeroplane, the quickest of all vehicles.

What enormous advance there is yet in store for aviation, cannot be doubted by anybody who examines the possibilities of aeroplane construction and applica-

tion with the unprejudiced judgment of an expert; he cannot help but acquire the conviction that air travel will not be inferior in extent and importance to railway and shipping.

May aeronautics have the good fortune that England, which has accomplished pioneer work in navigation and railway construction, and which has also furnished such important contributions to the scientific, experimental and constructional branches of this, our latest province of communications, that this England will lend its powerful and active assistance to the furtherance of such lofty aims in free competition with all the nations of the globe.

AERIAL NAVIGATION

BY A. P. ROWE, A.R.C.S., D.I.C., B.SC.

(*Awarded Pilcher Memorial Prize*).

The motives prompting the desirability of human flight from the legendary period to the present time have been very numerous. For the last two hundred years, apart from the war period, we may reasonably assume that the predominant factor has been the rapid transport of personnel or material from one point to another. With this aspect of aviation, aerial navigation is intimately concerned. I want you at the outset to distinguish clearly between the problem of aerial navigation over comparatively short routes—such as from London to Paris—and what is surely the true realm of aircraft, the long distance flights from England to America, India and Australia. In favourable weather the European routes require no more of the pilot than that he should be able to read a map with what, for want of a better term, I will call aeronautical intelligence.

It is not with this mode of navigation that I propose to deal, but rather with the art of navigating a craft over

(1) Land which is unknown to the navigator;
(2) Land which, by reason of its nature, is not capable of being mapped, *e.g.*, desert;
(3) Large areas of water;
(4) Land or sea, but above clouds, such that the pilot may take advantage of favourable weather conditions, or, in time of war, may proceed to his object of attack without interference from A.A. fire.

This is the real problem of navigation, and I propose giving a short account of the various methods by which aircraft may be navigated under these conditions. As I proceed, I hope to differentiate between those methods which will be used when definite trans-Atlantic and similar air services have been established, and those with which the civil aviation pilot is at present more intimately concerned.

Dead Reckoning Navigation

Navigation by dead reckoning (D.R.) is essentially a question of geometry. Provided that the navigator can keep an accurate record of his ground speed and direction, then his position may be read off from his chart at any time. The problem in still air is, therefore, a simple one. Unfortunately, we have to consider the effect of wind just as the maritime navigator has to consider tides, and the problem becomes more complicated.

In still air, it is sufficient to take readings of an accurate compass and an air-speed indicator. We may extend the method to navigating in a wind by the addition of either a drift indicator, giving the angle between the course steered and the course made good, or by arranging a timing scale, used in conjunction with the altimeter, enabling the ground speed to be determined. In practice, the observer's instrument may conveniently combine these two factors. The speed and direction of the wind may then be determined by observing drift or taking a ground speed observation on two different courses, preferably separated by a fairly large angle, or, without changing course, by taking one drift and one timing observation. Probably the most convenient instrument for the use of the observer is the Wimperis wind gauge bearing plate (Fig. 1) in which drift is measured by tail bearings, either of a ground object or of a flare dropped into

the sea. The wind speed and direction may be read directly from the instrument. It is important that the principle upon which this instrument is constructed should be fully appreciated. Consider a horizontal bearing plate having a transparent centre as shown in Fig. 2.

Let A-C represent the direction of the fore and aft axis of the machine, and let a rod pivoted at A be laid across the bearing plate, such that A-B is parallel to the direction of drift of the ground. Let the bearing plate be oriented correctly and a line A-B drawn across it. If now this procedure is repeated on a number

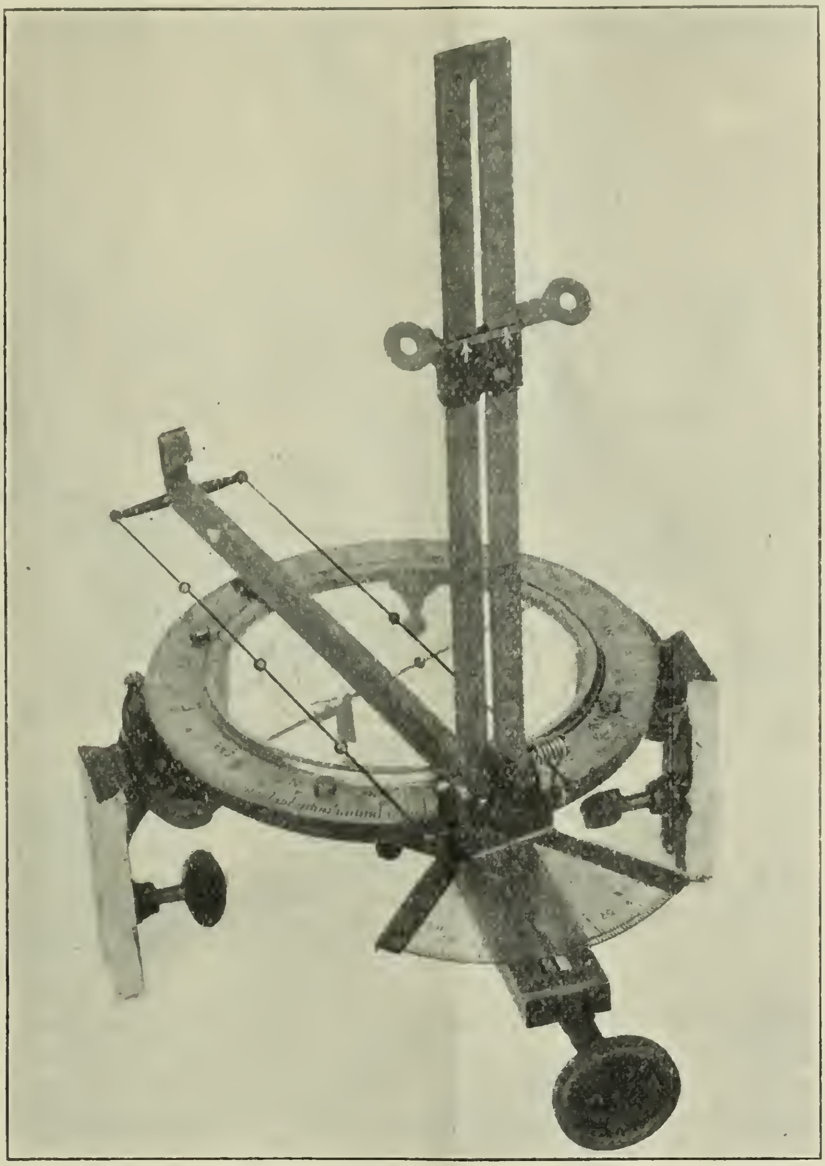


FIG. 1.

of courses, a wind star is obtained. The length P-C will then give the wind velocity to scale and the wind direction may be read off from the scale on the bearing plate. The point P may be obtained, of course, with two lines only, but it is obviously preferable to have at least a third and then to take the mean point of the resultant "cocked hat." As already indicated, if it is not desired to change course an observation of ground speed may be made and the point P in Fig. 1 located, such that A-P represents the observed ground speed.

It is sometimes convenient to take forward observations of drift, and for this purpose the Wimperis course setting sight may be employed. The instrument, shown in Fig. 3, employs the vector triangle principle described above.

It is necessary, however, to provide means enabling a pilot to navigate his machine by D.R. without the aid of an observer. The method must be such that observations are easily made, and that their reduction is simple in the extreme.

An American suggestion developed in England by Flight Lieutenant Capon consists of marking the wings with lines representing drift. The pilot sights some ground object ahead and has then only to watch the object until it crosses the marked wings, when the drift angle may be read off. If this is repeated for one other course, the wind speed and direction may be reduced by means of a course and distance calculator, or other simple device. Another simple method for the use of the pilot has been suggested by Mr. Wimperis, but has not yet been

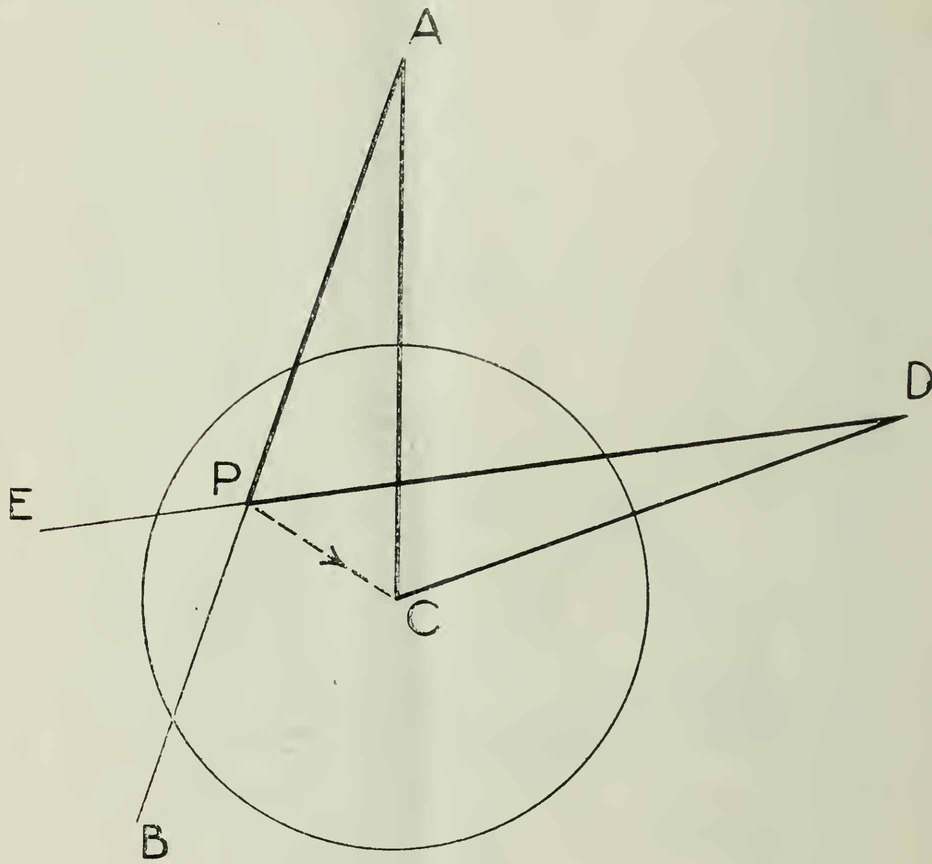


Fig. 2.

tried. It is proposed that the pilot should emit a puff of smoke from his machine when above a sighted ground object, and then fly in any manner he chooses for a measured period T, obtained by stop-watch timing, and to arrange that at the end of this period he is coincident with the puff of smoke and heading toward the ground object above which the puff was emitted. Suppose he reaches the ground object in time t, then the course steered toward the object obviously gives the wind direction and the wind speed is given by

$$\text{Wind speed} = \text{air speed} \ \{ t/(T+t) \}$$

These methods of enabling a pilot to determine the character of the wind involve the sacrifice of a certain amount of accuracy, but the former is likely to be of great use to civilian pilots at this stage of aeronautical development. The second method has not yet been used.

We have seen, therefore, that as long as it is possible to take the drift of the machine with respect to a ground object, the wind speed and direction may be satisfactorily determined. The methods described allow of an accuracy of about 5 per cent. of the wind velocity, and the wind direction can be estimated to within two degrees.

Astronomical Observations

I will now go on to the question of navigation by astronomical observation. This method is, perhaps, of little use at the present time, but for long distance flights of the future it will be a necessity. I want you to note particularly that D.R. records should always be kept as the flight progresses. If above cloud, and drift observations are unable to be taken, the meteorological forecast must be accepted, or if the latter is not available, then the nature of the wind observed at the last drift observation must be assumed to have continued, unless the pilot or observer has reason to believe that a change has occurred.

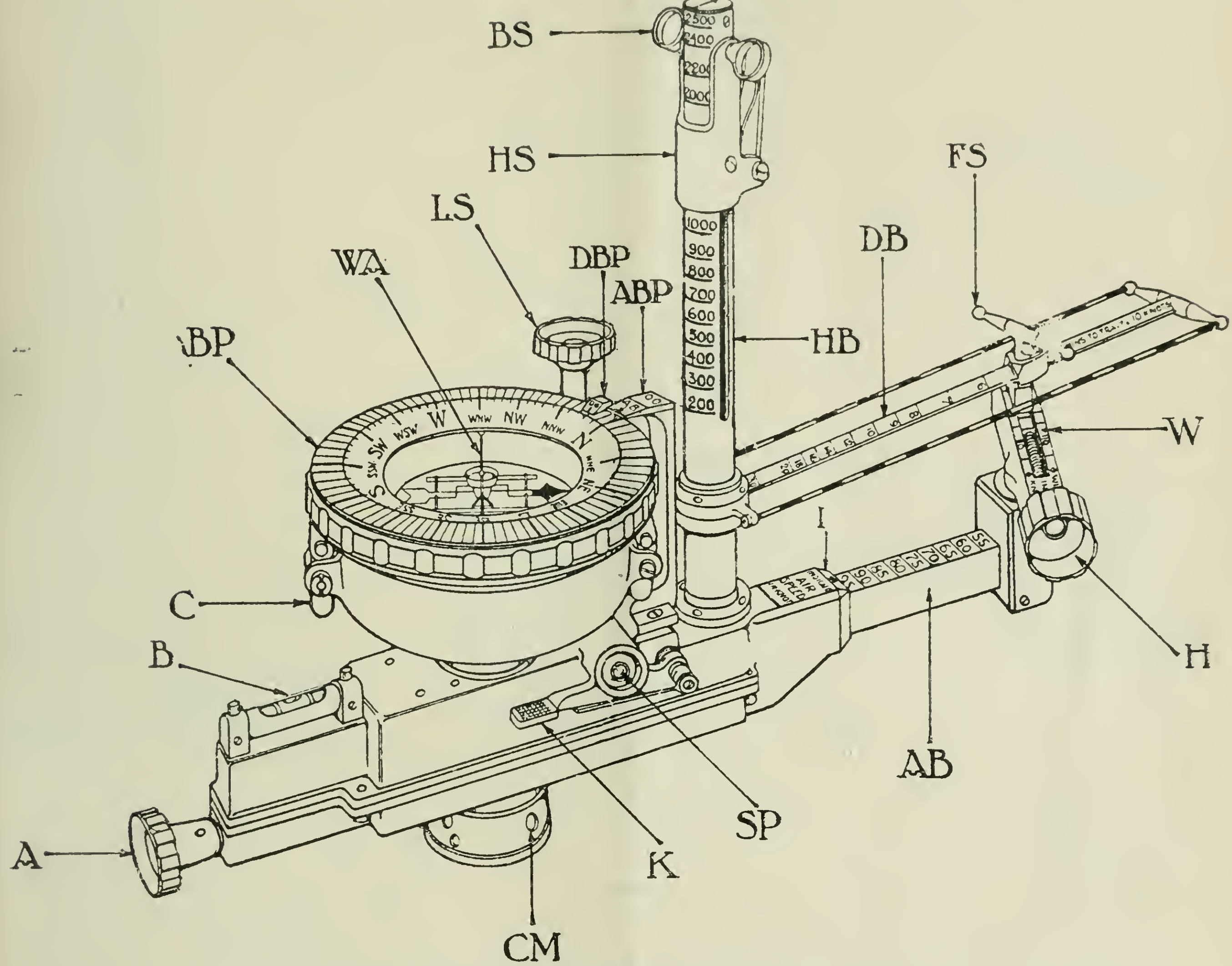


FIG. 3.

Astronomical observations serve only as a check on D.R. calculations. The basic principle underlying this type of navigation is a simple one. If the maritime navigator is able to take bearings of two lightships (*i.e.*, two positions fixed relative to the earth) then his position is known definitely. In the same way, the air navigator can determine his position from observation of two objects which are fixed relative to the earth. The place on the earth's surface immediately beneath a given heavenly body is called the "geographical position" of that body, and at a given time on a given day such a body may be regarded as being fixed relative to the earth. One astronomical observation allows a position line to be drawn upon the navigator's chart. Such an observation necessitates the use of a sextant, a chronometer keeping Greenwich mean time (G.M.T.) and a nautical almanac. Two astronomical observations, giving two position lines

cutting at an angle, obviously locates the navigator's position definitely. For example, an observation that the altitude was 60° would show that the observer was at some point on a circle, the centre of which was the geographical position of the heavenly body and the radius was 30° (*i.e.*, 1,800 nautical miles). His D.R. record, however, enables him to choose that part of the circle in which he is interested and it is sufficiently accurate to draw a straight line on his chart, this line being a tangent to the circle. The direction it takes on the map is obviously at right angles to the line joining the geographical position of the heavenly body to the observer's position, *i.e.*, at right angles to the azimuth of the heavenly body at the instant it was observed.

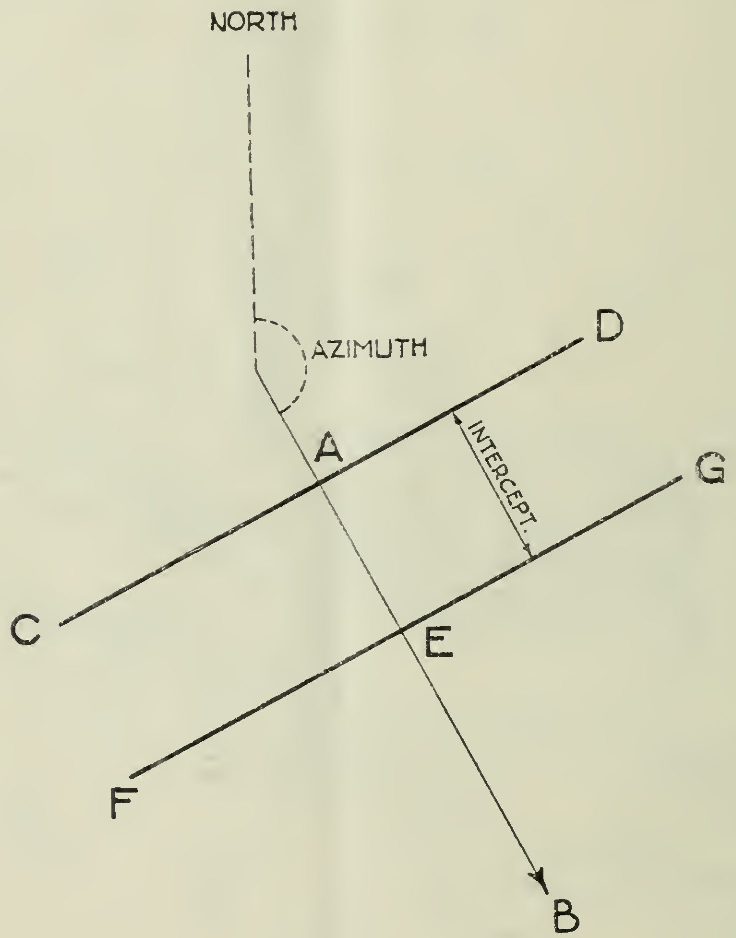


FIG. 4.

The position line is obtained in the following manner. The zenith distance and the azimuth of an observed heavenly body are calculated for the D.R. position of the aircraft, and the time noted. Let A-B (Fig. 4) represent the computed azimuth line drawn from A, the D.R. position. Then if the observed zenith distance is equal to the calculated distance, C-D is the position line. If, however, the observed zenith distance is greater or less than the calculated value, a line such as F-G is drawn parallel to C-D, the distance A-E representing in nautical miles the observed difference in minutes of arc. At least two position lines are necessary to determine the position of the observer. If it were possible to measure the azimuth as well as the altitude of any heavenly body, then a " fix " could be obtained by these two observations of the one body. In practice, however, it is necessary to make observations of the altitude of two heavenly bodies at the same time, or of one forenoon and one afternoon observation of the sun, estimating the run between the observations by D.R. methods. If, however, one directional wireless observation is possible, this allows one position line to be drawn and one

observation of the altitude of any heavenly body would then allow a "fix" to be obtained.

Observations of altitude are, of course, made with a sextant, and the design of a suitable model has been the subject of much patient research. The naval pattern sextant used for determining altitude at sea depends for its use upon the existence of a definite horizon, whether sea or haze. The air navigator cannot depend upon the existence of such an horizon, and even if one is visible, it is necessary to make a correction for "dip," depending upon the altitude of the observer. The value of this correction in minutes of arc is approximately equal to the square root of the observer's height in feet. If an observation is taken on a cloud horizon, it is necessary to know the height of the cloud, and this gives

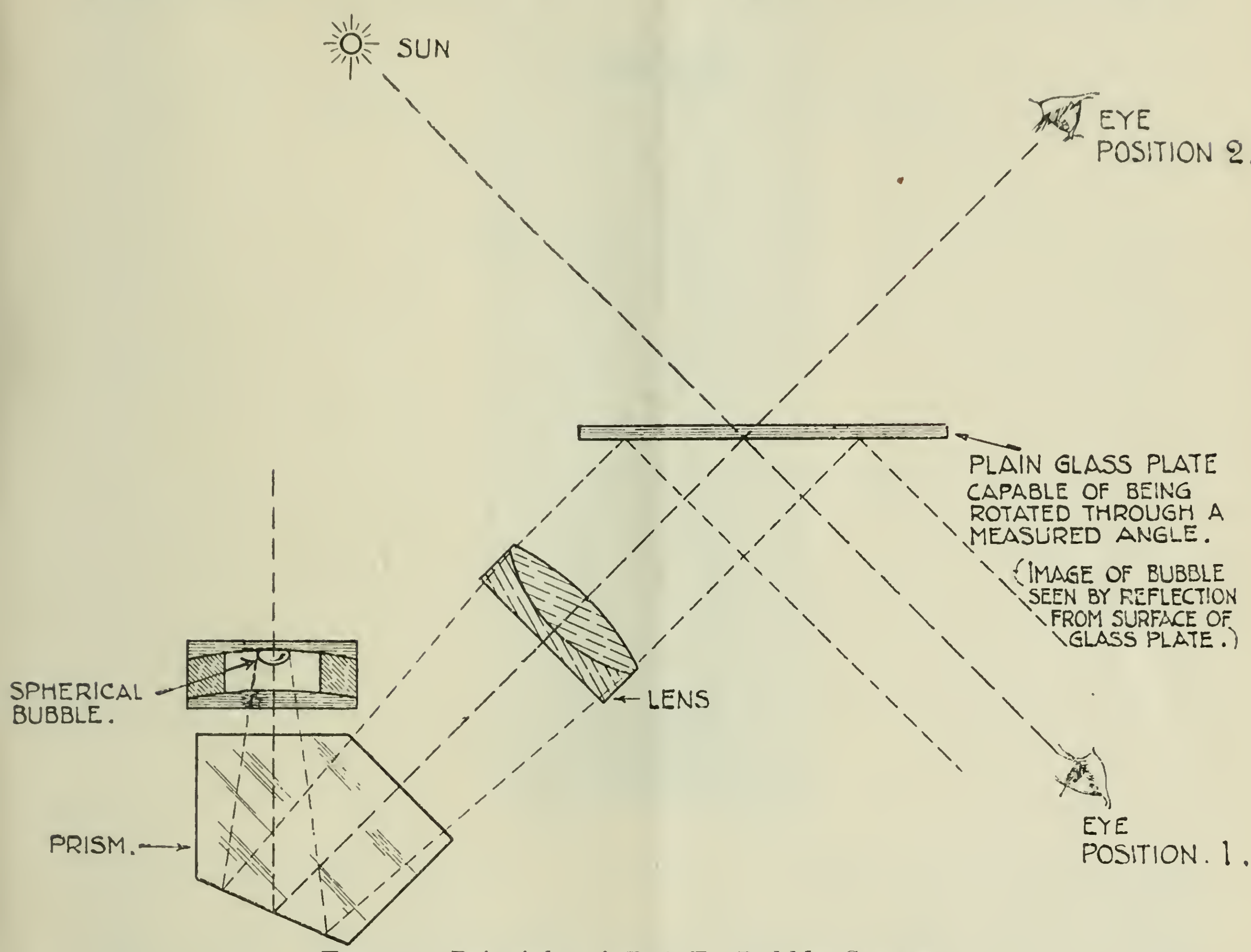


FIG. 5.—*Principle of R.A.E. Bubble Sextant.*

rise to errors. The Baker and Hughes cloud horizon sextant was an attempt to get over this difficulty. Images of two diametrically opposite portions of the horizon are projected into the field of view; then the true vertical should lie midway between the two images. An image of the sun is then made to appear midway between the two horizon images. It cannot, however, be assumed that the two positions of the horizon are at the same height, and error may well arise from this cause. Moreover, this sextant, like the naval pattern sextant, can only be used during the day, and has therefore a limited use. The R.A.E. bubble sextant, which we shall next consider, overcomes this difficulty by employing an artificial horizon. The principle of this instrument is shown in Fig. 5. The vertical is indicated by a vapour bubble constrained to move in contact with a spherical surface, the radius of curvature of which is equal to the focal length of the

collimating lens. If the instrument is rocked in the hand the bubble will appear to move with the astronomical body under observation. This is a great advantage, but errors due to small accelerations acting on the bubble are still existent. The spherical level is illuminated for and the instrument can therefore be used under all circumstances and is much in advance of other sextants for aircraft use.

It has been stated that it is necessary to compare the observed altitude with the altitude calculated from the D.R. position and the time of the observation. The methods employed at sea for this calculation are not suitable for use in the air, and moreover give a greater accuracy than necessary for our purpose. The reduction of observations in the air has to be performed quickly and with but little

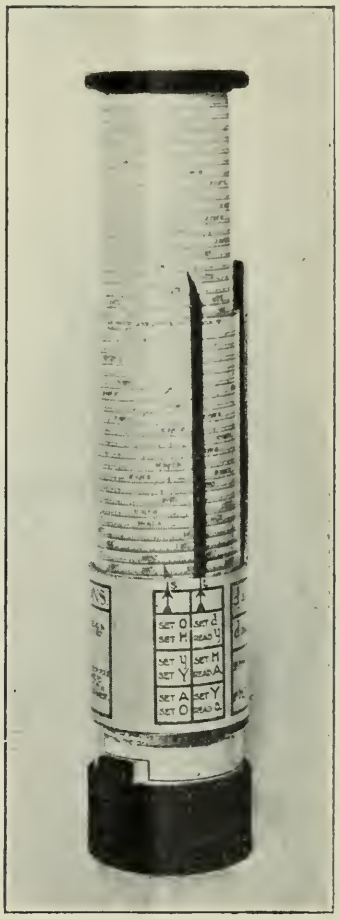


Fig. 6.

available space. The most convenient methods are either graphical—the most important of which is the Veater diagram—or by use of a slide rule. The Bygrave slide rule is shown in Fig. 6. The astronomical triangle is solved by dividing it into two right-angled spherical triangles. The instrument occupies but little space, is rapid in use and gives a degree of accuracy comparing favourably with other methods.

In addition, certain calculating machines have been devised, notably the Baker navigation machine, which is a combination of a mechanical and graphical method.

Directional Wireless

We have next to consider the navigation of aircraft by directional wireless. This method is comparativey recent and has not yet been satisfactorily developed. The basic principle is a simple one. If a coil A B C D (Fig. 7) is placed on the path

of wireless waves, such that the direction of propagation is parallel to B-C, then oscillating currents of different intensities will be set up in A-B and C-D. If now the coil is turned through 90 degrees, no current flows through it. Once, therefore, a search coil of this kind is mounted on a vertical axis it can be turned until the current received is either a maximum or a minimum and the direction of the wireless waves will be indicated. In practice it is general to observe the minimum current rather than the maximum current. With this simple form, of course, little accuracy can be expected, but the introduction of auxiliary electrical detecting apparatus enables the direction to be found to within a degree or two.

There are two alternative methods of use. In one, the aircraft carries the receiving apparatus, and notes the direction in which the wireless waves appear to come from a sending or beacon station, the actual position of which is known.

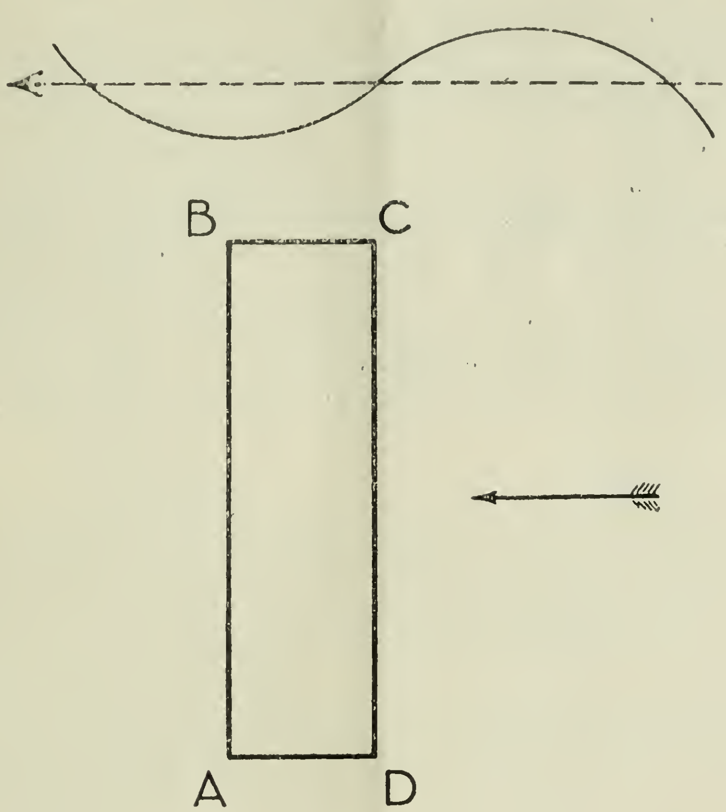


Fig. 7.

This method has the advantage of secrecy and, moreover, gives less work to the stations. The other method consists in calling up the beacon stations within range, and any two of these acting in collaboration can determine the position of the aircraft. Whichever method is adopted, there is a correction to be made when plotting a position line on a Mercator chart, owing to the fact that the wireless waves follow a great circle, and not the rhumb line on the chart. This correction may be made either by the Weir Azimuth diagram, or by the conversion angle method, the latter being sufficiently accurate for distances up to 1,000 miles.

The conversion angle is given by

(Half the difference of longitude of the sending and receiving station) × (Sine of mean latitude),

and is approximately the angle between the rhumb line and the great circle through the two stations.

Before concluding, I should like to call your attention to a problem intimately connected with ordinary navigation. I refer to the guiding and landing of the

aeroplane in a fog. This at present constitutes one of the chief dangers of civil aviation, and the climate of England, particularly Kent, being what it is, we cannot afford to ignore it. The gyro turn indicator, an instrument recording departure from the horizontal, is being developed and may be expected to form part of the standard equipment of an aeroplane in the near future. No system of landing in fogs has yet been perfected, however, and I will merely state that the problem is being attacked in two directions. One consists of locating a source of sound on the aerodrome, and the other, a French method, arranges for the guidance of aircraft by the electrical field produced by cables laid along the ground.

I hope I have shown that the science of navigation has progressed, at least as far, as other branches of aeronautics. When the time comes for the establishment of regular trans-Atlantic services, as come it will, I have no doubt that the navigation of the craft will be simply a matter of training observers to use their instruments intelligently. That time is not yet, but we, who probably represent the first generation to commence with the fixed object of making aeronautics our life's study, must prepare for it. To say that the Australian and Atlantic flights have been accomplished with an element of chance is not to depreciate the wonderful efforts of those responsible; rather is it to emphasise their courage. But the future of civil aviation cannot be expected to rest upon the courage of passengers. The business man of the future, travelling by air to America, has a right to ignore the risk exactly as he does to-day when considering a train journey. It is toward that goal that we must concentrate our endeavours.

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AVIATION IN SOUTH AMERICA, WITH AN INTRODUCTION ON THE SOUTH ATLANTIC FLIGHT.

A Paper read before the Royal Aeronautical Society (Scottish Branch) on 7th March, 1923,

BY G. F. LUKE, F.R.G.S., COMMISSIONER FOR GREAT BRITAIN OF THE WORLD'S BOARD OF AERONAUTICAL COMMISSIONERS, INC.

The object of this lecture is to try and show you what has been done, and what is still to do, in aviation on the South American continent, and I therefore propose to marshal my subject under the following headings :—

1. Existing air services.
2. New air routes projected.
3. Prospect of running aeroplane or seaplane services for mail or passenger traffic.
4. Missions from foreign countries.
5. Aviation transport companies.

Under these headings I will deal with the countries in rotation as I visited them, *i.e.* :—

Brazil,	Chili,
Uruguay,	Peru and
Argentine,	Colombia.

But, before dealing with the main subject, I think it would be fitting, rather than jump directly from this country to Brazil, to take you there by stages through the air and describe briefly that most wonderful flight made from Portugal to Brazil by those two great aviators of the Portuguese Navy, Comm. Sacaduro Cabral and Capt. Gago Coutinho.

These were not young enthusiasts that had set out to brave the wind and wave of the South Atlantic, but veterans in age as well as war, the sum of their ages aggregating more than a century.

If anything should go wrong, they could not count on a relay of warships to save them from disaster, as was the case with Read in 1919. They had to trust to their own skill and to luck. Coutinho had invented a modified type of sextant for taking observations in flight without recourse to the sea level horizon —a device entirely new in aeronautics, and one that proved to be a God-send to them in their navigation over those waste waters.

It was on March 30th, 1922, that these two intrepid men set out from Lisbon in their caravel of the air—a Fairey Series III. seaplane with 360 h.p. Rolls-Royce engine, carrying a useful load of 3,100 lbs., including fuel for 18 hours, having a speed with full load of 83 knots.

The route they intended to follow will be seen on the map to be from Lisbon to Las Palmas, Canary Islands, 710 miles; thence to Cape Verde Islands, 816 miles; thence to St. Paul's Rocks, 900 miles; thence to Fernando Noronha, 490 miles; and finally to Pernambuco, 275 miles, representing a total of 3,191 miles entirely over sea.

All went well on the first stretch to Las Palmas, which was accomplished in 7¼ hours, they having flown steadily at the rate of just under 100 miles per hour.

Waiting for a good moon, they launched into the unknown for Cape Verde Islands, and for more than half the way they were buffeted by high winds and

the vibration of the engine threw the compass out of order, so that they had to steer their course almost entirely by the sun. Only one steamer was seen—a mere smudge of smoke labouring far to the south on the bosom of the heaving waters. At last St. Vincent loomed ahead, where a good landing was made and a much-needed rest taken.

Rough weather delayed the start on the next stage, the most difficult of all, that from St. Vincent to those silent sentinels of the South Atlantic, the St. Paul's Rocks. The flight was made successfully, a distance of 900 miles, but on arrival misfortune overtook the gallant airmen, and they wrecked their machine in landing on a heavy sea. Another machine was cabled for from Portugal, and they took delivery of this machine at Fernando Noronha, and then started back to circle the St. Paul's Rocks and return to Fernando Noronha, so that the flight should be complete. Alas ! misfortune still dogged their efforts, for in this attempt they were blown by a gale far out of their course and at last had to come down in the rough sea. They were rescued by the British steamer "Paris."

Another machine was sent out, and finally, on June 5th, the final stage from Fernando Noronha to Pernambuco was accomplished. And so Commander Sacaduro Cabral arrived by air where just 422 years before another of his race and name, Pedro Alvarez Cabral, arrived by sea and founded an Empire for King Manoel and Portugal.

The navigating skill displayed on this flight may have been equalled, but certainly has never been surpassed, the powers of endurance and courage by these veterans, the one 49 and the other 54 years of age, being beyond all praise.

Any one of the first three non-stop flights was in itself a notable achievement, but the three together constitute a monument to man's conquest over the elements. That the machines and engines were all British adds another well-deserved tribute to the excellency of workmanship and material supplied by the British aircraft constructor.

On arrival in Rio de Janeiro, to which city the airmen continued their flight, they were presented with a cheque for £10,000 entirely raised by public subscription, and it is understood that this money is being spent by them in the purchase of a new machine, this time of Italian construction, with which to repeat their exploit early in the spring. Let us hope that they will be even more successful in this second attempt than they were in the first.

Brazil

And now, having brought you so far by air, I will turn to the main theme of my lecture.

Brazil, which is the largest of the South American Republics, has a population of over 30 millions and an area of 3,291,416 square miles, and is the fourth largest country in the world. Brazil has had a close connection with aviation since its earliest inception, for on November 12th, 1906, Santos Dumont, in France, astonished the world by making what was then an unparalleled duration record of 21 seconds. That was only sixteen years ago. To-day Brazil has a well-trained naval and military Air Force, each operated separately, amounting to some ten squadrons in all, the naval side being trained by the Americans and the military by the French. Naturally, the machines in use are either American or French, the Navy having mainly Curtiss flying boats, and the Army, Hanriot, Spad, Farman, Morane Saulnier, and some obsolete Nieuports and Caudrons. There are naval schools in Rio de Janeiro and Porto Alegre, and a military school at Sao Paulo, and others are to be established as soon as the finances of the country permit.

Of commercial services there are actually none, but two semi-commercial routes are projected from Rio to Porto Alegre, the one along the coast touching at

Santos, Paranaqua and S. Miguel, and the inland one calling at Sao Paulo, Curitiba and Camizas. These services at the present are intended primarily for use of the naval and military forces, and will be organised and controlled by the Ministry of Marine and Ministry of War respectively, but the facilities they afford will be available for civil and postal purposes. The Government is authorised to borrow up to 4,000 centos of milreis, or £200,000, to cover the cost of the two schemes.

A passenger and freight service between Porto Alegre and Montevideo, Uruguay, serving various centres between these two ports, was projected by a Senor Abreu Scareo, and he endeavoured to form a company for this purpose, but financial support was lacking, so the project fell through.

The prospects for running aeroplane or flying boat services for mail and passenger traffic are fairly good, inasmuch as there is extensive territory not served by railroads, and the coastal boat service between the smaller ports is uncomfortable and irregular, but drawbacks would be found in the difficulty of transporting material and petrol to outlying aerodromes, and in the unsuitability of the ground traversed for forced landings.

The only foreign missions are those already mentioned, *i.e.*, the United States Mission for training the Navy and Naval Air Service, and the French Mission for training the Army and Military Air Service. During the centenary celebrations, Capt. René Fonck and Lieut. Fronval came out to Rio on a special mission of aeronautical propaganda, chiefly in the interest of the French aircraft constructors. They had with them four aeroplanes—two of a commercial type—Breguets—and two fast Spads. I witnessed one of the finest exhibitions of acrobatic flying I have ever seen, and was only one of 200,000 people that saw it. It took me 2¾ hours in the car to do three miles back from the racecourse to my hotel—the traffic was so thick. Thus do the French Government help their aircraft industry.

Joy-riding is carried on to a small extent by Orten Hoover, an American, at Rio, and Mlle. Boland, of trans-Andes fame, runs a ferry service from Rio across the Bay to Nichteroy—a distance of 6½ miles.

Apart from the Military School of Guapira already mentioned, there are four other civil schools in the Sao Paulo district which train pilots for the Army under contract from the Ministry of War. They are :—

Escola da Forca Publica, using Curtiss Oriole machines.
Escola Campineira, using Caudron machines.
Escola Curtiss (run by orten Hoover), using Curtiss machines.
Escola Brazil, using Aviatic machines.

The foregoing represents the extent of aerial activities in Brazil as they are to-day, and it seems to me lamentable that Britain, who led the world in aviation during the war, should have no place in the training of, or in the supply of material to, Brazil, which undoubtedly will be in a very few years the greatest, richest and most powerful of all the South American Republics.

Uruguay

Uruguay, with a population of only a million and a half, and an area of 72,210 square miles, is the smallest of all the South American Republics. Actually there is no Air Service, either naval or military, that could be called as such, but the Army possesses a few obsolete British and French machines, mainly used for training purposes, the purchase of the former being the result of good pioneering work done by Major Scott, an ex-R.A.F. officer, on an Avro machine in 1919.

There is a military aviation school near Montevideo, and the head of this school has just returned from a mission to France, where he purchased six new Nieuports and a quantity of spare parts to the total value of half a million francs.

This may not seem a great deal, but as their standing Army only amounts to 10,000 officers and men, it is sufficient to form the nucleus of an efficient military Air Service.

An Air Convention has been signed between the Uruguayan and Argentine Governments regulating the traffic between the two capitals, and the negotiations between the postal authorities of the two countries have been brought to a satisfactory conclusion. On the first trip from Buenos Aires to Montevideo, after the completion of the negotiations, 1,270 letters were carried. I will deal, however, further with this cross-river service under the heading of the Argentine, to which it properly belongs, as the initative is all from that side.

The possibilities of successfully running aeroplane or seaplane services in Uruguay itself for mail or passenger traffic are not good. The railway and telegraph communications with the interior are admittedly bad, but on the other hand, the population in the country districts is scanty, for out of the total population of 1,500,000, a third of this is in Montevideo itself.

An international service from Montevideo to Asuncion, the capital of Uruguay, with halts at Colonia (Uruguay), Parana (Argentine), Corriantes (Argentine) and Formosa (Paraguay), might be a paying proposition for the future, though ambitious, I admit.

There have been no definite aviation missions to Uruguay, though in 1919 the French and Italian Missions to the Argentine gave exhibition flights. The Army, however, is trained on French lines, and the uniforms are almost identical with those seen in France in pre-war days, so that it can be taken that the predominating influence is French.

There are no aviation transport companies in existence yet, but tentative schemes for a service to Buenos Aires, and another to Rio Grande del Sul and Porto Alegre, have been mooted from time to time, though nothing has so far matured.

From the foregoing you will realise that there is not much being done in Uruguay in aviation, and the prospects for the future are somewhat obscure.

Argentine

The Argentine Republic is the second largest in South America, and has a population of 9,500,000, of which 2,275,000 are to be found in the province of Buenos Aires.

Great enthusiasm was shown during 1919 and 1920 in aviation generally, but in 1921 this dwindled somewhat, owing chiefly to the general crisis which swept the country, and also to the failure of the French and Italian Missions to weather the storm.

1922 saw a recrudescence of interest, due mainly to the initative of the Army aviation authorities, who put forward a scheme for the establishment of air routes throughout the Republic. The hub of these services was to be at the military aerodrome of El Palomar, the spokes leading to outlying stations to be created at Monte Caseres, Rosario, Salta, Cordoba, Mendoza, Nouquen and Callegos, besides which 22 secondary stations and 126 emergency landing grounds were projected. The negotiations between the Ministry of War, the Postal Department and the Ministry of Commerce in regard to this project were still in progress at the time of my departure.

Military aviation has made great progress, being well organised and equipped with modern craft. The training at El Palomar is most efficiently carried out, and real merit is to be found among the instructing officers and pilots. I would like to mention specially Captains Zanni and Parodi, two of Argentine's most capable pioneers, who, by their enthusiasm and example, have done much to vulgarise aviation in their country. They were among the first and few pilots

who successfully flew the Cordillera de los Andes to Chili and back, and this is no mean achievement.

At the recent manœuvres three squadrons—one bombing, one fighting, and one reconnaissance and spotting—took part, and it was considered by military attachés present that a degree of efficiency was shown equal to that to be found amongst many European countries to-day, and while the Brazilian Air Force is numerically stronger than that of Argentine, it is not, as yet, nearly so well trained.

The Naval Air Force is not so far advanced as that of the Army. Training is carried on at San Fernando, the seaplane station, but it would appear that the Army has a much greater pull on the purse strings of the nation than the Navy. The machines in use are nearly all Curtiss flying boats, but there are also a few French machines.

Buenos Aires reminds me very much of Pekin. The latter city is full of people trying to negotiate loans to the Chinese Government, while the former is full of people wanting to secure air concessions from the Government. The great similarity between the two is that neither succeed in their ambition. The reason for this negative result in both cases can be summed up in the two words "insufficient Bukshieh."

The British, French and Italians seem to be anxious to start freight, passenger and mail services to various parts of the country, provided that they are given the exclusive right to exploit their chosen route, but the Government does not appear willing to grant such rights.

Major Kingsley, an ex-R.A.F. officer, arrived in the country in 1919 and formed a company called the Sociedad Rio Platense de Aviacón. This company has operated for the last two years a successful cross-river service between Buenos Aires and Montevideo carrying mails and passengers. The service has only been worked during the summer months, the rest of the time being taken up with joy-rides and the training of pilots, and the company possesses an excellent 'drome at S. Isidro. Last season it was found that the distance from the town to the aerodrome, some 40 minutes by electric car or an hour and a quarter by motor car over what are termed roads, was often too much for a prospective passenger to face. The same time was lost at the Montevideo end.

To remedy this, Major Kingsley visited England during the winter (our summer) and purchased a number of seaplanes, so that in future the service will start from the North Basin, Buenos Aires, and finish in the Harbour at Montevideo, and *vice versa*. This means that the journey will be done in one hour and thirty minutes from hotel to hotel as against two hours and twenty-five minutes—a very considerable saving.

This new service should be of great commercial utility. The actual flying time should not exceed 65 minutes, and, always provided that the fares charged are not too high, the line will be patronised and should become a paying proposition.

Another source of revenue to this company is the private chartering of 'planes by rich estancieros to take them back from the capital to their estancias. By train such a journey often takes 24 hours, after which there may be a four or five hours' ride on horseback. By 'plane the whole distance can be done in four or five hours, and the question of a landing ground, as a rule, presents no difficulty owing to the general flatness of the country.

Altogether, the Rio Platense Co., since its foundation, has carried nearly 15,000 passengers and flown over 400,000 kilometres—a very creditable performance.

Another company started by the Italian constructor Caproni called the Italo-Argentine Aerobus Co. "Caproni" intends opening a service between Buenos

Aires and Montevideo. It is alleged that they are to use giant Capronis carrying 23 passengers and 3½ tons of cargo, but I think that this is doubtful.

The German-Spanish scheme for a Zeppelin line from Seville to Buenos Aires is beginning to take shape. The service is to be maintained by two dirigibles having 135,000 cubic metres' capacity, measuring 250 metres long and 34 metres in diameter. They are to have accommodation for 40 passengers and eleven tons of cargo. The duration of the voyage is to be 3½ days westwards and four days eastward, and the fare will be ptas. 6,000 or £200. Air ports are in course of contruction at the two termini and include hangars 300 metres long and 50 metres high. Both ports are well equipped with shops for undertaking repairs, and will also have wireless.

Capt. René Fonck, who paid a visit to the Argentine after Brazil, outlined a scheme for an aeroplane service from Paris to Buenos Aires. A committee of influential people was formed to study the best means of carrying it out, but that is as far as it has got, and probably as far as it ever will get. But the idea served the purpose for which it was intended excellently well, and that was to draw the attention of the Government and the people to the products of the French aircraft constructor.

I could tell you a great deal more about the Argentine, but time presses, and I think enough has been said to show that there is a great future there for aviation.

Before passing on to Chili, I should just like to show you what the Andes look like, so as to give you some idea what pilots had to face in attempting to fly over them.

Chili

Chili, with an area of 307,774 square miles and a population of some 4,500,000, has the distinction of having the only Air Force in South America which can be said to be modelled solely on British lines and using only British material.

The Naval Service, whose temporary bases are at Valparaiso and Quinteros, has been trained under Lieut.-Col. J. L. Travers, late R.A.F., with two British mechanics. The material consists of three Avros, four Shorts and two Sopwith seaplanes, also two Supermarine flying boats Channel type Mark II. fitted Siddeley " Puma " engine.

The Military Service has its headquarters at the aerodrome " Lo Espejo," near Santiago, while the Military Aviation School is at " El Bocque," which is next to " Lo Espejo." The school is housed in a fine large building, and is well equipped to teach the latest improvements invented for safe flying, as well as the usual military routine, including directional wireless and wireless telephony.

On the aerodrome are hangars built of reinforced concrete capable of holding ten or twelve machines of the Avro or Bristol type, and there is also an observatory and meteorological office.

The material consists of ten Avros, eight S.E.5A, eight Bristol monoplanes and twenty D.H.9 fitted with Siddeley " Pumas." They also have two Italian Svas, which, I understand, were gifted to the nation by Italians resident in the country. There is also a well-equipped repair shop employing some twenty-five mechanics.

All the afore-mentioned British machines were presented to Chili by Great Britain in part payment for the use during the war of the Chilian battleship " Latorre," named by us during the war the " Canada."

The very highest credit is due to Major Scott, late R.A.F., head of the British Mission to Chile 1920/21, who created and organised throughout the Chilian

Military Air Service, and this in spite of all kinds of political and diplomatic intrigue that was endeavouring to hinder the realisation of his task.

With thirteen Avros Major Scott trained 74 pilots and handed back thirteen Avros intact at the end of his contract. This, I think, is a wonderful performance, and one worthy of the highest traditions of the R.A.F.

The most outstanding flight made by a Chilian aviator was that of Capt. Aracena, the present Director of Aviation, who, accompanied by a British mechanic, flew on a D.H.9 with "Puma" engine from Santiago to Rio de Janeiro *via* Mendoza, Buenos Aires, Montevideo, Porto Alegre, etc.—a distance of 1,850 miles. This was in the spring (our autumn) of last year.

As regards commercial aviation, this does not exist at present. The principal difficulty in the way of establishing air routes in Chili is its configuration, which renders forced landings extremely hazardous, and might exclude the use of the aeroplane except for the Valparaiso-Santiago route.

A service between Valvidia on the 40th degree of latitude to Iquique on the 20th, with calls at Talcahuano, Valparaiso, Coquimbo and Antofagasta, has been mooted by people representing British and French interests using flying boats. The difficulty, however, is that, with the exception of Valparaiso and Talcahuano, the ports mentioned have no harbours, and the almost perpetual heavy swell experienced along the West Coast would make the landing at the different ports somewhat perilous.

Such a service, however, if it could be worked, would be an inestimable boon to the trade of the country, as at the present time the communications for mail and passengers are slow and erratic.

Peru

Peru, which has an area of 439,014 square miles and an estimated population of 5,000,000, possesses the nucleus of an efficient Air Force, under Comandante Juan Leguia, son of the President, who served with the British in the 7th Squadron R.N.A.S. during the Great War and won the D.F.C. The Naval Service has been trained by Capt. W. Simon of the U.S. Navy, and use mainly Curtiss flying boats.

The Military Service, under the control of Comandante O'Connor, a Peruvian of Irish descent, use British, French and Italian machines. It is not so efficient as the naval side, but in the summer of last year Comandante Leguia visited this country and made considerable purchases of material, securing also the services of Capt. Marsden, late R.A.F., to act as Chief Instructor, so that in the near future this service should become quite useful.

In 1919 the French sent out a mission, but in January, 1920, the head of this crashed and was killed, and this took away a great deal from their prestige. The Italian firm of Ansaldo had a mission here, and succeeded in getting two of their machines purchased by public subscription and presented to the nation.

The principal aerodromes are at Lima and Arequipa, and the repair shops are also in charge of a Britisher, Mr. Thurmer.

From the foregoing it will be seen that we have succeeded in ousting both the French and Italians, and have now got complete control of the Military Wing.

Turning now to civil aviation, there is one commercial aviation company, The Compania Nacional Aeronautica, Lima, which was formed in July, 1920, with American capital, and they are agents of the Curtiss Company. They started a service between Callao and Lima, but this had to be abandoned owing to lack of support, the reason being that there is an excellent tramway service between the two towns, taking only 25 minutes.

A mail service along the coast has been mooted, but nothing has so far materialised, due to lack of capital, and also on account of the heavy Pacific swell, already mentioned, making landing difficult.

In considering possible air routes, apart from the coastal one, the most important that immediately strikes one is that of linking up the Pacific with the inland port of Iquitos—2,175 miles up the Amazon from the Atlantic coast and 630 from the Pacific.

In 1918 Squadron Commander Dyott made the extremely hazardous journey from Pacasmayo to Iquitos to make an air route reconnaissance of the country from the coast to the latter town. The four possible routes were as follow :—

1. The Pichis trail from Lima *via* Oroya and thence to Iquitos.—Flying distance, 750 miles; present time, 32 days; flying time, $9\frac{1}{2}$ hours.

2. Trujillo *via* Soledad and Parcoy, thence by the Huallaga river to Iquitos.—Flying distance, 590 miles; present time, 40 days; flying time, $7\frac{1}{2}$ hours.

3. Pacasmayo, Chachapoyas, Balza Puerto and the Huallaga river to Iquitos.—Flying distance, 690 miles; present time, 64 days; flying time, $8\frac{1}{2}$ hours.

4. Paita or Chiclayo to Bellavista, thence through the Pongo de Manseriche to Iquitos.—Flying distance, 630 miles; present time, 92 days; flying time, 8 hours.

Of these, the fourth would appear the easiest on account of the lower height of the Andes, at this point some 8,000ft., and the possibility of utilising the Rio Maranon for landing in case of necessity.

In the latter part of last year an American aviator named Faussett practically accomplished this flight, using route No. 4 as suggested by Dyott.

He left Chiclayo and flew 436 miles non-stop to within 60 miles of his destination, being forced to land in a violent storm at the point where the Rio Tigre joins the Maranon. He accomplished this remarkable flight on a Curtiss-Oriole with an 80 h.p. Curtiss engine, taking only six hours.

The difficulty in establishing such a route would, of course, be the ground work. Supplies and landing places, whether on land or water, would require to be established about every 200 miles, those on the east of the Andes being supplied from the Atlantic coast, and those on the west from the Pacific. Amphibious machines would be required, and the capital necessary about £250,000, as against £9,000,000 for building a railway.

The benefits of such a service are obvious, as it would bring this important area within 12 hours of the Pacific coast instead of 32 days as at present.

Colombia

I did not visit Colombia, but in order to make this paper as complete as possible, with your permission I will give a few details of the aerial activities in that country.

Naval or military aviation cannot be said to exist as yet, the activities in this connection being confined to a school for training pilots with French machines and French instructors.

The lack of the third arm, however, is amply counterbalanced by the wonderful progress that has been made in commercial aviation, both for the carrying of mails and passengers.

No other country in South America, and few in Europe, possesses such a complete system of air mails as does Colombia, and this is mainly due to the lack

of competition from other means of transport and the eminent suitability of the configuration of the country for aerial communication. The advent of the aeroplane and flying boat has been instrumental in solving a problem which seems almost insoluble by other means.

Before aerial transport was established the sole artery of communication with the interior was the Magdalena River, which is navigable for some 600 miles, as far as La Dorada. The shifting sands and shallow draft, however, made the duration of such a journey a matter of conjecture, and mail matter from this country often took as much as five and six weeks to get to the capital, Santa Fé de Bogotá. With the introduction of the air mail a letter takes less than three weeks, so that it is possible to get a reply to a letter in the same time that it previously took that letter to arrive at its destination.

The mail services are as follow :—

Barranquilla to Girardot.—This is a bi-weekly service leaving Barranquilla every Tuesday and Friday and arriving at Girardot the same day, a distance of some 350 miles, with stops at Monpox, Barranca, Bermeja, Puerto Berrio and Honda for the delivery of mail. From Puerto Berrio, the port on the Magdalena for Medellin, the mail is taken by special messenger to the latter town, and the same is the case at Honda to Manizales, and at Girardot for Ibagué, Bogotá and Tunja.

Mail forwarded by air arrives at Medellin, Ibagué and Bogotá on Wednesdays and Saturdays, and at Manizales, Neiva and Tunja on Thursdays and Sundays. Letters addressed to other cities in the interior of the Republic are landed at the river port nearest to the National Post Office of the district in which the town is situated.

Girardot—Barranquilla.—This service is the same as the foregoing, only reversed, the departure being on Wednesdays and Saturdays. The Wednesday service makes connection with the Clyde Line steamers sailing on Fridays from Puerto Colombia to New York. Mail from Bogotá to New York takes fourteen days. It also connects with Elders and Fyffes' steamers leaving on Thursdays and Saturdays from Santa Maria for Bristol, mail taking by this route twenty days from Bogotá to London.

Barranquilla and Cartagena.—This service flies every Monday, returning to Barranquilla the same day in time to make connection with the Tuesday Barranquilla and Girardot service.

Barranquilla and Santa Maria.—This service is the link between the Barranquilla and Girardot line and Elders' and Fyffes' steamers. The 'planes leave Barranquilla every Thursday and return the same day to connect up with the Friday Barranquilla and Girardot service.

These are all the services actually working at the moment, but they represent some 1,250 miles, which makes our London-Paris seem rather a weak effort in comparison.

The charges for mail are rather high, being 1s. 2½d. per ½oz., but the saving in time, as I have tried to show, is enormous.

The unfortunate part of all these services is that they are run by a German company. The Colombian-German Aerial Transport Company has a contract with the Government to carry the mails for five years, being subsidised to the extent of $100 or £20 per trip from Barranquilla to ports on the Magdalena River. During the three months ending October, this Company made 307 flights, totalling 57,640 kilometres, carrying 303 passengers and 29,280 kilos of mails and freight. All the machines used are German, namely, Junker monoplanes.

For a country with only five millions of population this is a wonderful record, and rather makes one wonder why the people of this country, the hub of civilisation, do not patronise our continental services more.

In conclusion, I would say that great opportunities await the aircraft constructor, the pilot and the capitalist in South America. The continent is developing fast, and the means of transport essential to that development are slow. The people, unhandicapped by tradition, take to aviation with probably better grace than did our forefathers to the railway. The meteorological conditions are far better than those found in Europe.

With a return of general trade to something like its pre-war prosperity, South America should prove a large and steadily increasing buyer of aircraft, but it will want the very latest in design and construction, not surplus war stock.

REVIEWS

Etude Sur le Ballon Captif et les Aeronefs Marins

Par le Commandant Charles Lafon. Paris: Gauthier-Villars et Cie., Quai des Grands-Augustins, 55. 1922.

The subject matter of this book divides naturally into two parts. The first four chapters deal with the modern French kite-balloon and the remaining three are devoted to the mathematical study of aeronautical tactics with an application of it to surface ships. On neither subject has a great deal been written; indeed, in the case of the balloon, the author has practically a virgin field, as practically nothing of value exists outside the official manuals which have been written for the various national air services. More frequently than not the author of a new technical treatise needs to commence his book with a schedule of reasons, most of them more or less artificial, in justification of adding still further to the troubles of a world already grossly overloaded with treatises on the subject of his choice. Monsieur le Commandant Lafon does not do this; and rightly so, for any apology of this sort would be out of place, for his book, whilst not breaking a great deal of new ground, forms a very real and useful contribution to the literature of branches of aeronautics which much need written records. The development of the kite-balloon is the least spectacular of modern aeronautical developments, and the uses to which a type of aircraft which has no motive power of its own can be put are obviously limited. In its own sphere, however, the captive balloon did excellent service during the war at a cost in men and material comparing very favourably with that of other forms of aircraft. Although at the present time interest in balloons has dwindled to very small proportions, there is no reason for considering them abandoned permanently in favour of more recent developments. In particular, war requirements are likely to involve the use of captive balloons for many years to come for dealing with the problems which come within their particular sphere to the exclusion of other classes of craft, either lighter or heavier-than-air. The present book is to be welcomed, therefore, as putting on record the results of a good deal of research and experience on the subject. The combination in one work of two, at best, only loosely related subjects is due to the fact that the book is a presentation of the lectures given by the author before the commissioned and non-commissioned officers and cadets at the aviation stations at Corfu and Mudros during the war. In writing his book he has kept the subject matter within the limits of his lectures, whilst amplifying the detail of his treatment in various places, particularly in the mathematical treatment of various problems.

In Chapter I. a general account is given of the handling and working of naval kite-balloons, as practised at Corfu and Mudros. The details are essentially

of a practical nature. Descriptions are given of how to let up a balloon from the deck of a vessel, how to haul down under various conditions of weather, etc., the methods of transferring between ship and shore, arrangements of balloon stations, and so on. The main general features of meteorology as they affect the handling of naval balloons find a place here, as do also a description (which would be clearer if illustrated by a drawing) of the rigging of an R-type Caquot balloon and the balloon drill developed by the author and used by the French naval balloon units in the Near East. The chapter closes with a very condensed account of the silicol method of generating hydrogen and of the process employed for compressing the gas into tubes. A good deal of the matter in this chapter is of the sort which may be found in general instructional manuals, but some of it is in a more abridged form.

The next three chapters are concerned with dynamical problems related to the kite-balloon. Chapter II. deals very fully with the form of the curves taken up by the cable with and without a wind, equations giving the form of the cable and the relative tensions in it being arrived at by an extension of the well-known principles used in determining the equation to the catenary and other forms of suspension cable. The subject of dynamic lift is also treated in this chapter, and one cannot but remark on the apparent indifference to aerodynamic data which characterises the French treatment of balloon problems in general. Although it may legitimately be considered that the aerodynamic forces acting on a balloon form a relatively large proportion of the whole, especially when the balloon is being towed at high speeds, no reference is given in the book to any wind channel or other measurements of wind forces, but the author is content to state that the dynamic lift of a 150 cubic metre Caquot balloon may be given with sufficient approximation by the formula

$$0.5\ V^2 \sin i$$

i being the angle of pitch of the balloon and V the relative air speed (metric units). Some attention is devoted to the case of a balloon carrying an independent auxiliary cable, a matter which is of minor importance, at any rate in British practice, at the present day by reason of the development of telephone-cored cables, which render independent telephone cables unnecessary.

There appears to be a certain confusion of terms in the title to Chapter III., "Analytical Investigation of the Equilibrium of the Elongated Captive Balloon." The author actually discusses the questions of stability in pitch and yaw, his treatment of the relatively simple problems of equilibrium (as distinguished from stability) having been considered in the previous chapter. His treatment of stability differs from that developed in this country by Bairstow and others as an application of the Bryan-Bairstow method. The author considers separately the oscillations set up by small displacements in pitch and yaw, a treatment which is incomplete. Admitting the limitations of this method, there is something to be said in favour of its practical value. In attempting to apply the complete theory of stability we soon come up against factors which, at any rate in the present state of our knowledge, it is not possible to evaluate or even to hazard a close guess at their relative effects on the results. On the other hand, the more limited mathematical results arrived at by the analysis given in the book have enabled a number of practical deductions as to fin area, etc., to be arrived at, which have undoubtedly contributed largely to the evolution of a practicable balloon.

Chapter IV., a short one of five pages, deals with an important practical problem, namely, the determination of the stresses set up in the cable when letting up or hauling down a balloon in a wind. The analysis leads to the conclusion that under certain circumstances of hauling down rapidly a maximum tension approaching fairly close to double the static value may be set up in the cable, a matter of considerable importance when the low factors of safety that

have to be used in balloon work are taken into account. This chapter completes the author's notes on balloons.

A new line of thought is started in Chapter V. in which the author applies mathematical methods to some problems of aerial tactics. He deals almost exclusively with problems arising out of aircraft patrol work at sea. He begins by evolving equations to paths which an observing aircraft must follow in order to keep at a constant distance from a (slower) surface ship or squadron with which it is keeping in touch, and follows this up by a series of problems of a similar nature. The material of this chapter formed the subject of a communication to the French Academy of Sciences in 1922 by Professor Koenigs, of the Sorbonne. The author claims that the working out and plotting of such paths would be of advantage in enabling chasing craft to keep in closer touch with their quarry. For naval and air officers with sufficient capacity for and interest in mathematics to enable them to follow out the complete theory and apply it to the various cases that may arise in practice, it is probable that a study of the contents of this chapter would be of considerable theoretical advantage, but it seems scarcely that its practical value goes beyond this.

Chapter VI. is of a similar character to the preceding one, but deals with the kinetics of such aeroplane evolutions as spiralling, pancaking, looping, etc., and the author determines by mathematical reasoning the decelerations and changes of motion which take place in various cases. His results certainly bring out how and under what circumstances these evolutions can become dangerous. In the following chapter he applies the same principles to evolutions of ships at sea.

The book consists rather of a series of essays on problems of applied mechanics relating to naval aeronautics than of the chapters of a connected treatise. This applies particularly to the last part of the book. The first four chapters have a much closer connection of interest; but, excepting the first one, which deals with the practical field and sea work of handling and using kite-balloons, they deal mainly with a special phase of the subject only. Throughout the volume it is evident that the author's talent for applying mathematics to the problems met with in the practical work of his profession has been the main cause from which the treatise originated. And this gives the book its distinctive character, not as an aeronautical text-book, but rather as a collection of mathematical solutions of problems arising out of the use of kite-balloons and other aircraft at sea. Anyone turning to the book in the expectation of learning anything of the details of kite-balloon construction and design must inevitably be disappointed as it is quite devoid of such descriptive matter either written or in the form of diagrams. Whilst of very little use to anyone who has not some fair acquaintance with the subject, to the designer who wishes to look at his problems from a scientific standpoint and to eliminate rule-of-thumb from his work as far as possible, the book affords subject matter for much profitable thought. The chapters dealing with tactical kinetics and dynamical problems of aeroplane evolutions fall into a similar category; they are for the individual who has already a good general grasp of his subject and who can widen his knowledge and equip himself the better for considering new problems of a similar nature to those discussed in the book.

The Journal
OF THE
Royal Aeronautical Society

(Founded 1897 in succession to the Annual Reports)

Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

No. 154 OCTOBER, 1923 Vol. XXVII

NOTICES

Election of Members

The following members were elected at a meeting of the Council held on September 11th :—

Associate Fellows.—Captain A. B. Fanstone, A.F.C., E. C. Hubbard, J. D. Williams, F. T. Courtney.

Students.—R. Vasconcellos de Aboim, F. J. W. Digby, C. H. Pridham, W. T. Sandford.

Associate Members.—R. L. Preston, R. A. Tarleton.

Chairman

Mr. A. Ogilvie, C.B.E., Fellow, assumed office as Chairman of the Society on the first of this month. He has selected "Gliders and Light Planes" as the title of his inaugural address to be read at the Royal Society of Arts, John Street, Adelphi, on the 4th instant, at 5.30 p.m.

"Air Strategy"

It has been arranged with the Royal United Service Institution that Wing-Commander Edmonds's lecture on "Air Strategy" will take place before that Institution on Wednesday, December 12th, instead of before the members of this Society on Thursday, December 13th, as previously announced. Any members wishing to attend the lecture should apply either to the Secretary, or direct to the Secretary, Royal United Service Institution, Whitehall, S.W.1, for a ticket of admission.

Lantern Slides

The Council desire gratefully to acknowledge gifts of lantern slides to the Society's loan collection for the use of members by the following firms :—

The Supermarine Aviation Works Ltd.

Westland Aircraft Works (Branch of Petters Ltd.).

Library

The following books have been received and placed in the Library :—" Le Ballon, l'Avion," M. Larrouy ; " Flying Round the World," W. T. Blake ; " Regulations for R.A.F. Reserve," Air Council ; " Sur la Théorie des Surfaces Portantes," M. Roy ; " Engine Balance," Cormac ; " Properties of Engineering Materials," Popplewell & Carrington ; " Internal Combustion Engine," H. R. Ricardo ; " Motor Fuels," E. H. Leslie ; " Vom Gleitflug sum Segelflug," Gustav Lilienthal ; " Proceedings of the Third Air Conference," Air Ministry ; " Jane's All the World's Aircraft, 1923," C. G. Grey ; " A Book about Aircraft," E. Protheroe ; " Dictionary of Applied Physics—Aeronautics," edited by Sir Richard Glazebrook ; " Jaarboekje of the K.N.V.L.," Besluit ; " Anais do Club Militar Naval," Coutinho & Cabral ; " Photography as a Scientific Implement " ; " Inter-Imperial Communications Through Cable, Wireless and Air " (reprinted), Sir Charles Bright ; Report of the Aeronautical Research Committee, 1923 ; " Taschenbuch fur Flugtechniker und Luftschiffer," 31st Edition, Moedebeck ; " Etude sur le Ballon Captif et les Aeronefs Marins," Charles Lafon.

Council

In view of the Glider Competitions being held at Lympne by the Royal Aero Club from October 4th to 13th, the monthly meeting of the Council has been postponed to Tuesday, October 16th.

Arrangements for the Month

Thursday,	October	4th, 5.30 p.m.	Inaugural Meeting. Mr. A. Ogilvie, " Gliders and Light Planes."
Tuesday,	,,	16th, 5.30 p.m.	Council Meeting.
Thursday,	,,	18th, 5.30 p.m.	Squadron Leader R. M. Hill, " The Manœuvres of Inverted Flight."

W. LOCKWOOD MARSH, *Secretary*.

ROYAL AERONAUTICAL SOCIETY

A meeting of the Royal Aeronautical Society was held at the Royal Society of Arts, John Street, Adelphi, London, on Thursday, March 15th, 1923, Professor L. Bairstow in the chair.

The CHAIRMAN, in calling upon Professor Melvill Jones to read his Paper on "Control of Aeroplanes at Low Speeds," said that Professor Jones came before the meeting as a Fellow of the Royal Aeronautical Society primarily. He was also a member of the Aeronautical Research Committee, and was Chairman of the panel which dealt with control and stability. As a consequence of that, he was in a particularly good position to know how we stood in regard to that subject. The broad issue was very easily stated. It was within the knowledge of all that, when an aeroplane was stalled, control became exceedingly difficult, particularly lateral control; that was true of all heavier-than-air craft as we knew them at the moment. The point at issue, and the point which naturally came to the Aeronautical Research Committee for consideration was, "Must we always accept unsatisfactory control for stalling?" and he expected Professor Jones would deal with that point. He believed they would find his general outlook was that the problem was a very difficult one, but by no means hopeless, and that we might, in the course of a year or two, expect to so re-design the controls of an aircraft that the spin which was often precedent to a crash would be very much less common than to-day.

Professor MELVILL JONES then read his Paper.

CONTROL OF AEROPLANES AT LOW SPEEDS

I must begin by explaining how I come to be giving this lecture. The experimental work with which I shall deal has, for the most part, been done at the N.P.L. and the R.A.E., under the general direction of the Aeronautical Research Committee. The way in which I come to be connected with the work is that I am a member of this Committee and am Chairman of a small panel that was created, some three years ago, by the Committee, to deal with this and other work relating to control and stability. The experiments that I shall describe and the methods of dealing with the results that I shall employ are, therefore, the results of the combined work of a considerable number of people. I can thus claim no special ownership of any of the ideas that I shall use, except in so far as I belong to the panel that has been working upon them. On the other hand I am giving the lecture as a private person, so that any views I express are personal ones and in no sense official.

The policy of the A.R.C., in relation to the work under discussion, has been to issue experimental data as soon as it becomes available, but not to discuss methods of applying the data to practical problems until they are in a position to issue a comprehensive report on this aspect of the problem. The reason for adopting this course is to avoid annoying the public with half formed theories that may have to be withdrawn, whilst at the same time making the data available to anyone who is prepared to draw their own deductions from it. When, over a year ago, I agreed to give this lecture to-night, I hoped that some such comprehensive report, which I could have taken as my text, would have become available. The problem has, however, turned out to be very involved, and we are still no more than at the beginning, so that no such comprehensive report exists,

and I shall have to confine myself to a sort of interim rep on progress, given with the principal object of inviting discussion and suggests for future work. I shall to-night discuss only such data as has been issued, cs now sanctioned for publication, and I have to thank the Air Ministry and the separate authors for permission to use this latter data.

Safety in landing and getting off, which is the end toward which all this work is striving, depends, apart from engine reliability, upo two distinct factors :—

1. The minimum speed at which the aeroplane can fly steadily.
2. The control that the pilot has over its movements in th neighbourhood of this minimum speed.

The first factor depends on the shape of the wing and the loadin per unit area; the decision as to what this minimum speed will be is a matr of the greatest importance with which I shall not deal to-night. I gave my views on this subject at the Air Conference, and I still adhere to them, in spite of the fact that they called down the wrath of the high gods upon my head. One must, however, deal with one thing at a time, and I shall, to-night, assume that the minimum speed has been settled, either with or without the use of variable wing sections, and that we are merely concerned to give the pilot the maximum power of control over his machine when flying near this speed.

I propose to start this problem from the very beginning, asking the indulgence of those of you who already know something about it.

When an aeroplane slows up from one steady speed to another, the angle of incidence of the wings must, in general, be increased to compensate for the loss of speed, otherwise the air lift will no longer equal the weight and the aeroplane will fall. There is a limit to this process, beyond which further decrease of speed will inevitably produce a fall. The angle at which this limit occurs is called the critical angle, or sometimes the stalling angle, and if it is desired to fly above this critical angle the speed must be again increased. The minimum speed of steady flight which is associated with the critical angle is called the stalling speed. Steady flight is quite possible either above or below the critical angle, but with the difference that, below the critical angle, the resistance to motion of modern aeroplanes is low and their controllability good, whilst above this angle the resistance is inevitably very high and, in modern aeroplanes, the pilot's control is almost non-existent.

When, to-night, I shall speak of flying beyond the stalling angle, or, briefly, "stalled," I shall not refer to flying at steady speeds lower than the stalling speed, which is impossible, but merely to the fact that the angle of incidence is greater than the critical and that we have therefore passed from the first régime above mentioned into the second. This is a point upon which confusion has occasionally occurred.

With this short introduction I wish to turn for a moment to consider the history of flying. All the pioneers of the art experienced immense trouble with their controls, and many of them were killed for this reason. Later on these control troubles seemed almost to disappear. Why was this? It certainly was not because much more had been found out about control; it was because, having better engines, men were able to fly faster and thus avoid approaching or passing the critical angle, near which the control difficulties that killed the pioneers arise. The difficulty had thus been sidestepped, not overcome, but it never could be sidestepped entirely, because of the necessity of landing and getting off. In a forced landing particularly, it is often a matter of vital necessity to approach the landing ground with the minimum possible speed, and it is not a matte for wonder that pilots, when faced with the necessity for doing this or havin an obvious crash, occasionally overdo it and stall their aeroplane. When th

happens it is not unusual for control to be entirely lost and for a fatal accident to follow.

All down the progress of aeronautics there has been a steady series of fatal crashes due to this cause, and they still go on. In war we could afford to ignore these fatalities, but in peace we cannot, and I do not, myself, believe that there is any hope for a wide commercial development of aeronautics until this problem has been successfully tackled, so that the penalty of an error of judgment during the approach to a difficult landing is no longer more or less instant death.

How are fatal accidents from this cause to be reduced to a negligible quantity? In my own opinion it is no use whatever to take the line, "Pilots must not stall," "Engines must not fail." "Pilots should not stall," "Engines should not fail," are reasonable statements, but the fact remains that they do stall and fail respectively, and it is up to designers and scientists and such people to find out how the worst consequences of these errors can be avoided when they occur.

In my own view, and I think it is the view of the A.R.C., the only sound way to tackle this problem is to carry out a thorough investigation into the causes of loss of control, not only at angles of incidence at which the pilot may be expected to want to fly, that is at angles just below the critical, but at angles far above the critical which he may reach involuntarily. To me it seems unthinkable that we should leave uninvestigated a whole series of flying states into which it is possible to get an aeroplane through an error made in the ordinary course of flying; especially when the things that happen in these states are responsible for the majority of our flying accidents.

I lay stress on the words "thorough investigation." We are sometimes criticised on the lines that we are not getting along fast enough and that we ought to have tried this or that particular device that might have given us a controllable machine by now; in other words, that we fiddle about with experiments and theories instead of getting something done. Well, this brand of criticism leaves me, personally, quite unmoved. We are not aiming at short cuts to a success that we do not understand. What we want is to understand the problem thoroughly, so that we may be able to develop some broad basis upon which aeroplanes, under design, can be examined for controllability, much in the same way as they can now be examined for strength and performance. We have, as a matter of fact, made one or two freak alterations to aeroplanes and tried them in flight, but this was done more from the point of view of amassing information on which we could check theories and wind channel data than with the object of evolving the perfectly controllable aeroplane at this stage of the investigation.

Let me now define what I mean by the word control. Of course, no aeroplane is infinitely controllable, there are always limits to the rates at which they can be moved and rotated. A stalled aeroplane without engine, for instance, must descend at a rather high speed whatever else it may be doing, so that, in one sense, it is bound to be less under control than an unstalled aeroplane in which the inevitable rate of descent is much less. This is an inherent difference between the two states, and we cannot overcome it. We can, however, by altering the form of the controls, give the pilot more or less power over the orientation of his aeroplane, and in future, when I speak of control, I shall limit my meaning to control over the orientation of the aeroplane.

It is quite easy to give, in general terms, an explanation of the reasons for loss of control when stalled. When an aeroplane is rolling so that the starboard wing tip is falling, the local angle of incidence of that tip is increased, whilst that on the other tip is decreased. Below the critical angle increase of incidence increases the air reaction on the wing, hence in this case a rate of roll

to starboard results in a rolling moment tending to raise the starboard wing and thus to stop the roll. When flying above the critical angle, however, increase of angle of incidence decreases the air reaction, and a rate of roll to starboard generates a rolling moment to starboard, and hence the rate of roll tends to increase unless checked.

This phenomenon is called autorotation, and has been known for years; it can easily be observed by pivoting a model aeroplane in a wind channel about an axis parallel to the wind. Provided this model is stalled, a roll, once started, will continue indefinitely.

The above is not the only effect of rolling a stalled aeroplane, the downward velocity imparted, say, to the starboard tips not only reduces the reaction upon it, but increases the aerial drag upon it, the reverse being the case on the rising tip. In this way a yawing moment forcing back the falling wing tip is introduced. Now this yawing moment, unless checked, quickly starts the aeroplane turning to starboard. This speeds up the port wing relative to the starboard wing, and thus, by increasing the air reaction on the port wing and reducing that on the starboard wing, introduces an additional rolling moment tending still further to increase the rate of roll to starboard.

If these tendencies are not checked it is easy to see how the effects pile up rapidly and produce the familiar spin. Now why cannot the pilot check the tendencies as soon as they start? It is not that they are too rapid; they are rather rapid in developing, but the average pilot is more rapid. It is not, as we thought at first, that the ailerons will not give the necessary rolling control to neutralise and reverse the effect of the rate of roll. The reason is as follows:—When the ailerons are used to check an undesirable rate of roll they generate a yawing moment of the same sign as that which is already being generated by the rate of roll, and the two combined will, as a rule, overpower any rudder of conventional size that is supplied with modern aeroplanes. Although, therefore, the direct action of the ailerons is of the kind desired, their indirect action upon the rate of yaw undoes all the good they have done, and the last state of that aeroplane is worse than the first.

Two obvious cures for this ill present themselves. One is to provide such a big rudder that the pilot can hold up against any yawing moment that may occur, and thus use his ailerons with impunity; and the other is to provide freak ailerons that will give the opposite yawing moment to that given by the conventional type, for if this could be done a simple movement of the ailerons would neutralise both the effects of rolling a stalled machine and leave very little to be done by the rudder.

Another cause that contributes to the loss of control after stalling is that the centre of pressure of the wings moves backwards as the critical angle is passed, so that the aeroplane tries to dive downwards. In many aeroplanes, especially those that are stable in normal flight, the elevators are not powerful enough to counteract this tendency, and an uncontrolled dive results. To cure this trouble it is obviously necessary to make the longitudinal control system more powerful. This may not necessarily mean larger controls, but merely more movement in the trimming arrangements for the fixed part of the tail. This question has not yet been fully investigated.

Here then you have, in a nutshell, the whole situation, together with the directions in which a cure is to be sought, but do not be misled into the belief that because we can formulate the problem clearly in this way the trouble is over. It is when we come to consider quantities and ways and means that the troubles really begin. When we came to tackle the quantitative side of this problem, some three years ago, we were met with a complete lack of data, for the reason that almost all wind channel experiments on controls had hitherto been confined

to angles below the critical, and nearly all free flight experiments had been confined to flying below this angle and to spins. Spins are the last stages of lack of control, and therefore are not very interesting from our point of view, which is to find out how to prevent the loss of control occurring in the early stages. If I may use a simile from mountaineering, the rock climber is more interested in finding out how to avoid slipping from the rocks than in studying the precise nature of the bump with which he will hit the ground far below.

May I digress a moment on to the vexed question of the use of models, for the situation in which we found ourselves bears strongly upon this problem. It is not until you have got no model data and, therefore, as things are, no detailed data at all, that you realise how difficult it is to think quantitatively when you have no quantities to think about. In this case we knew that we were working in the region, above all others, in which models are likely to give misleading results, but the alternative to using them was to sit and think about nothing. The plan that we have adopted to get over this difficulty is to use models to give us something to think about, trusting that the general character of the data will be correct, but firmly refusing to trust any conclusions until they have been thoroughly checked on the full scale.

When one comes to deal with these problems of control from a quantitative point of view, one is immediately rebuffed by the appalling complication involved in any attempt to calculate the motion of the aeroplane under the action of the controls. The classical stability calculations of Bryan and others are complicated enough, although they only deal with infinitesimal disturbances from steady straight flight. If we were to attempt to attack our problem in the same way, we should be involved in hundreds of times this complication before we could make a complete statement of the more involved motions.

From the practical point of view, however, it is not, in general, necessary to determine exactly how the aeroplane will behave under some fixed setting of the controls, it is usually sufficient, and indeed more interesting, to know whether the pilot has the power to apply any turning moment that he may desire to the aeroplane, when the latter is moving in any particular way. All experience goes to show that if a pilot *can* influence the aeroplane in any way he likes he may safely be left to find out *how* to do it for himself. There appears to be no need to carry out elaborate calculations in order to tell him what to do.

Acting on these principles we attack the problem quantitatively by trying to find out under what conditions the pilot will be able to apply moments in any way that he may desire, and under what conditions he will not be able to do so. There is a very simple way of distinguishing broadly between these two sets of conditions. If a pilot can produce, about any particular axis, a moment in either direction, he can also produce zero moment about that axis. It follows from this that in all those cases where he can produce any moment he likes about any axis he likes he can also bring the aeroplane into a state of complete balance, such that no moments are acting about any axis whatever passing through the C.G.

The power to produce a state of complete balance under any particular set of conditions is, therefore, a first necessity of control, although the possession of such a power does not necessarily imply that there is sufficient control for practical purposes, since moments of some definite magnitude will be required to provide the necessary angular accelerations involved in the manœuvre in question. We may therefore say that the power to produce balance is a *necessary* but not *sufficient* criterion of controllability.

The plan that we have adopted to study control quantitatively is to concentrate attention first upon finding out whether it is, or is not, possible to reach a state of balance under any set of conditions. If it is not, then the aeroplane

is obviously not under control under those conditions; if it is possible, then the aeroplane may or may not be under *adequate* control, depending on the amount of free control moment that can be applied after balance has been reached.

As an example, we have already seen roughly why an aeroplane becomes uncontrollable when stalled, and we observed that the reason was that under certain conditions (*i.e.*, when rolling more than a certain rate) it was no longer possible to maintain balance, owing to insufficient rudder power.

Of course the consequences of not being able to reach the balanced state will depend a good deal upon whether the moment that we cannot neutralise tends to make things worse or better. Thus we cannot roll an unstalled aeroplane at more than a certain rate, but if it does roll more rapidly the unbalanced moment tends to stop the roll and, unless the roll is necessary to the manœuvre, no harm is done. In the stalled state the reverse is the case, and the lack of control has very serious consequences.

Let us bear these facts in mind for future reference and proceed to the study of the data available.

The main variables concerned are as follows:—The angular settings of the three controls, the angle of incidence, the rates of roll, pitch and yaw, and the rate of side-slip. A complete investigation would involve a determination of the three forces and three moments that act on the aeroplane for every combination of these eight variables. Such a complete experimental investigation is, of course, out of the question and some simplification is essential. The first simplification that we make is to neglect the effect of rate of pitch on rolling and yawing moment, and of rate of roll and sideslip on pitching moment. It is not rigidly correct to do this, and we can see cases in which we shall have to take such effects into consideration; still, we must begin by simplifying somewhere, and this seems, on the whole, the best way to start.

Let us deal with what are called the asymmetric or lateral group, that is with roll yaw and side-slip. For a complete investigation, even of this restricted group, we would have to find the effect upon the air reactions of every combination of ailerons and rudder setting, at every combination of rates of roll, yaw and side-slip, with the aeroplane flying at every angle of incidence. This is still too big a job to be practical, even if we could do it experimentally, so we make the further simplification that we will only study the effects of pure rates of roll, yaw and side-slip; each in the absence of the others.

We have no guarantee that the effects of a combined yaw and roll can be obtained from the addition of the effects of each taken separately, but there are reasonable grounds for supposing that two aeroplanes that behave similarly when subject to pure rates of roll, yaw and side-slip, will also behave similarly when subject to combined movements. If this should prove to be the case we could get all we want in practice by a study of the simplified data for pure rolls, etc.; for what we generally require is to be able to predict whether some particular aeroplane, under design, will be better or worse than some other one of a different shape, upon the control of which we have experience in actual flight.

Now how can we get at the effects of pure rates of roll, yaw and side-slip upon the air reactions. Well, we can make a shot at them by calculation, based on strip theories similar to those used in the design of airscrews. But if we are prepared to use models we can also determine them experimentally.

The effect of side-slip can be determined by merely turning the model in the wind channel until the wind blows on it from the side, and then measuring all the forces upon it; the measurements being repeated, of course, for various angles of incidence and for various amounts of side-slip. Apparatus for doing this has been designed at the National Physical Laboratory, and is used in one of their 7ft. channels.

The effect of a steady rate of roll can be examined by means of a special apparatus designed by the Royal Aircraft Establishment and used in one of their 7ft. channels. In this instrument the model is rotated steadily about an axis parallel to the direction of the wind. Such rotation does not alter the attitude of the model to the oncoming air, so that steady forces and moments are set up that can be measured.

The effect of a rate of yaw cannot be studied in the wind channel, but could be studied on a whirling arm, such as is used for testing airscrews. These experiments have not yet been done, but it is hoped to revive the small whirling arm at the N.P.L., for this and other reasons. For the purposes of this lecture I have, however, made a rough calculation of the effects of yaw, and, though this is based on admittedly shaky assumptions, it probably gives results of the right order, sufficiently accurate for the somewhat rough arguments that I shall be using.

Fig. 1 shows some typical data of the type we have discussed. The figures

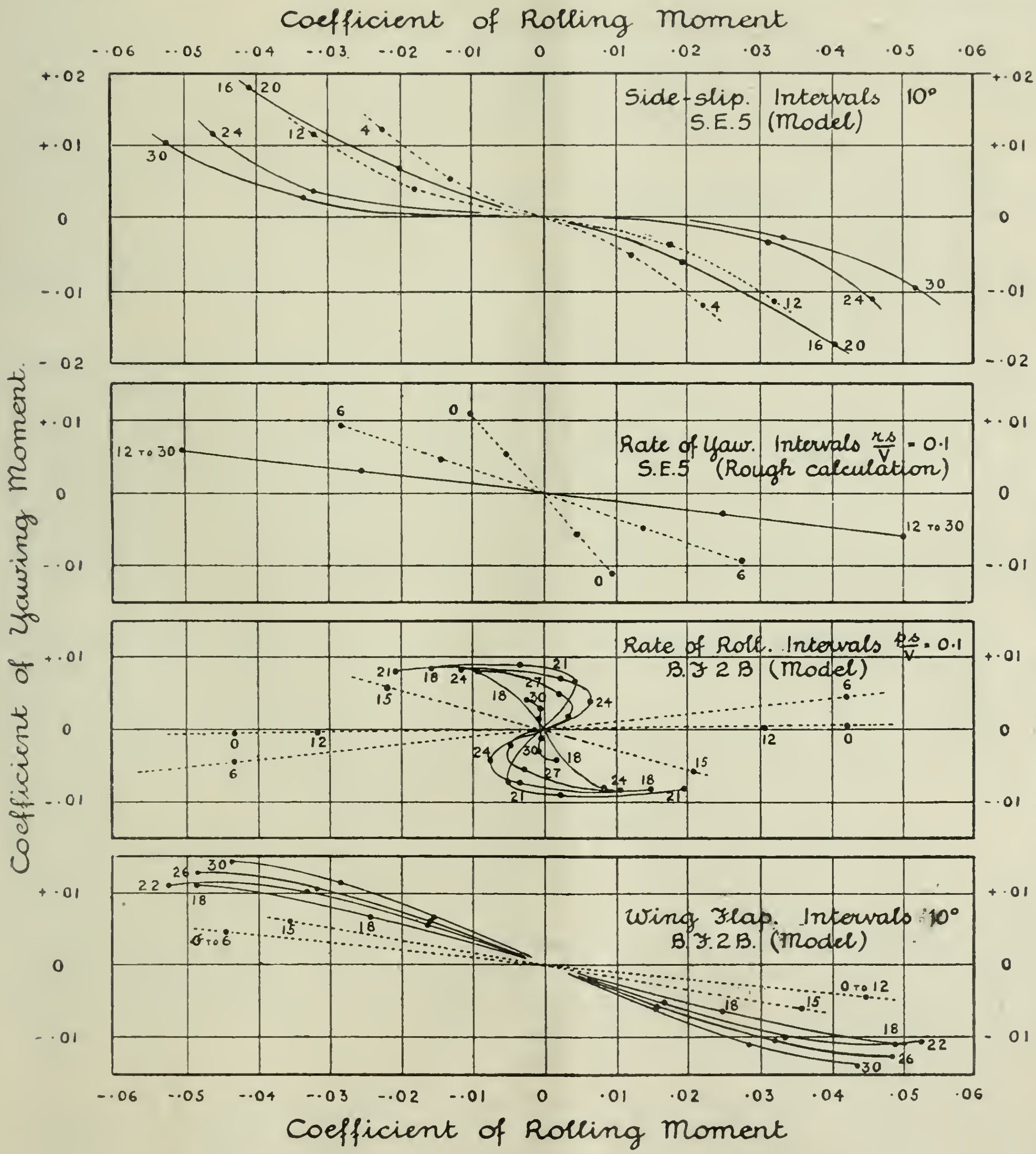

FIG. 1.

refer to the rolling and yawing moments produced by various rates of side-slip, yaw and roll and by the ailerons in straight steady flight. Rolling moments are plotted as abscissæ and yawing moments as ordinates to the same scale, but it should be noted that the abscissæ represent rolling moment, not about the direction of motion, but about an axis in the body inclined at about an angle of — 3 deg. to the chord of the wings. It will be remembered that the rates of roll and yaw were measured about axes that were parallel and perpendicular to the direction of motion. The reason for adopting this somewhat peculiar procedure is that it very greatly simplifies the representation of the results and the physical conceptions involved. It will be noticed that the line joining the origin to any point on the diagram is a vector representing in magnitude and direction the resultant moment acting on the aeroplane.

The units in which these results are represented are of a non-dimensional nature similar to that used for representing lift and drag. They may be considered to have been obtained by dividing the actual measured moment by the quantity $\rho S^2 Vs$

where	ρ is the air density
	S is the area of the plane
	V is the air speed
and	s is the semi-span.

For those who are not used to these non-dimensional systems the figures can be considered as being proportional, for a given aeroplane at a given height, to the actual moment divided by the square of the air speed.

The separate curves in the figures represent results for different angles of incidence of the centre part of the wing, those for the unstalled régime being shown dotted and for the stalled régime in full.

Let us take the figures in order. The consequative dots on the first group of curves represent the effects of side-slipping 10 and 20 deg. respectively. It will be observed that side-slipping produces both yawing and rolling moments, *i.e.*, a positive side-slip, to starboard, produces a negative rolling moment, lifting the starboard wing, and a positive yawing moment, turning the nose to starboard. The effect of side-slipping increases as the angle of incidence increases until about 24 degrees incidence, when there is a rapid fall in the yawing moment, except at high angles of yaw. This is no doubt due to the body shielding the fin and rudder at these high incidences.

The next diagram, showing effect of rate of yaw, is only constructed from very rough calculations. It shows that, after stalling, the effect of rate of yaw is nearly independent of angle of incidence and that the rolling moments generated are much greater than the yawing moments. The consequative dots in this and the next diagram represent rates of rotation, such that the velocity of the wing tip due to the rotation alone is 0.1, 0.2, 0.3, etc., of the speed of the aeroplane.

The diagram showing the effect of a rate of roll about the direction of motion is the most interesting of the lot. It shows that, below the stall, the effect of rolling is to introduce an immense rolling moment tending to stop the roll, whereas above the stall the result is a relatively small rolling moment, in the direction to increase the roll, accompanied by a yawing moment retarding the falling wing. This is, of course, the phenomenon that we have already discussed qualitatively. It will be noticed that the effect practically ceases at 30 degrees incidence, where rolling produces practically no moments at all. The dots on the curves have the same meaning as in the previous diagram.

Turning now to the action of the ailerons in straight flight, we see that the effect of these is to produce rolling and yawing moments of opposite sign, as already explained; but we see now that, after the stall, the desirable

rolling moment is much decreased, whilst the undesirable yawing moment is increased.

If we assume that the effects of the ailerons are the same whether one is rolling or not, we can see, from the last two diagrams, that it is impossible to neutralise the effects of a rate of roll of, say, 0.1 at 24 degrees incidence with the ailerons alone; for, if we used them to destroy the rolling moment, we should be left with a large unbalanced yawing moment, and an inspection of the diagram shows that this unbalanced moment would amount to about 0.008. The Bristol Fighter rudder at this incidence only provides about 0.003 yawing moment; hence we see where the difficulty lies.

One cannot, however, assume lightly that the ailerons will behave in the same way when flying straight as when rotating in roll or yaw, but there is no difficulty in extending the experiments to find out how they act when any of these simple motions are occurring. These experiments have been made in the case of side-slip and rate of roll, and they lead, of course, to a whole series of observations at different aileron settings for each spot upon the three upper diagrams of Fig. 1. It would be useless to represent here every one of these experiments, they can be found in the official reports. I shall content myself with using one particular instance to explain how we deal with this data to find out the conditions that will bring about a state of balance in any particular case.

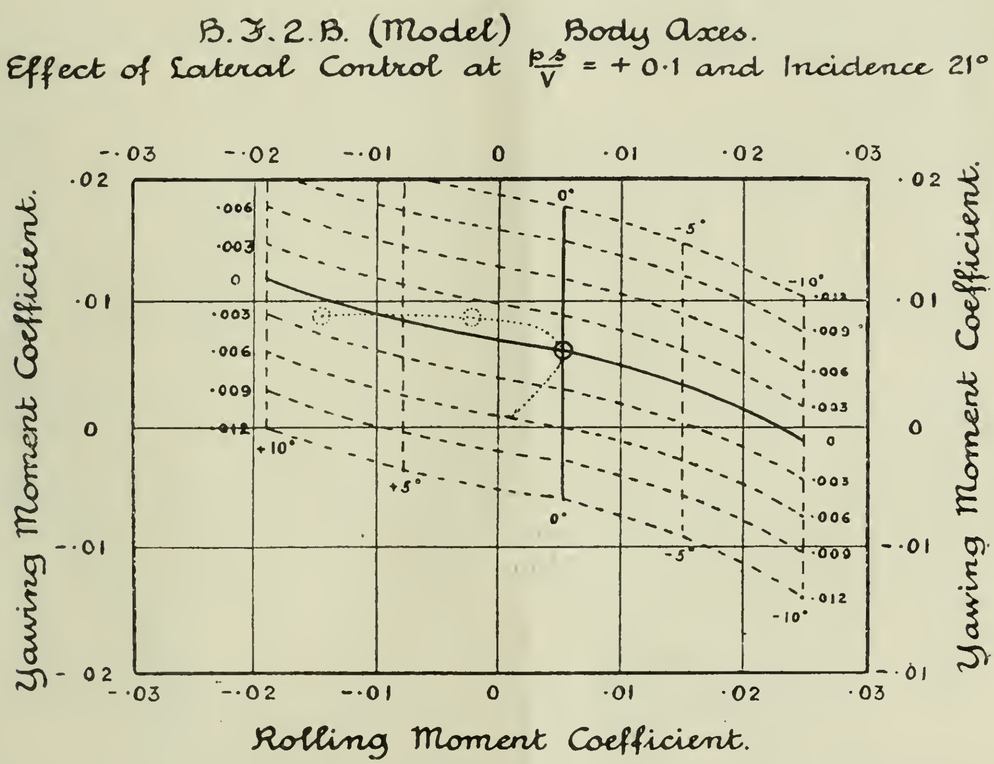

FIG. 2.

Fig. 2 represents the effect of the controls when the angle of incidence is 21 degrees and the rate of roll 0.10. The rectangular co-ordinates represent moments, as in Fig. 1, and the thick oblique line represents the effect of the ailerons under these conditions. The symmetrical position of the ailerons leads, in this case, to definite yawing and rolling moments owing to the presence of the rate of roll. The rudder applies a pure yawing moment; hence the effect of moving the rudder is represented by vertical movements in the diagram. It will be apparent that the settings of the ailerons and rudder that are required to bring about any particular moment upon the aeroplane can be instantly read off upon the distorted co-ordinates—shown dotted. We can see, for example, that to bring about

a state of balance, or zero moment, it is necessary to set the ailerons at 2 degrees and for the rudder to provide a moment of 0.007.

When we have treated all the information in this way and extracted the aileron and rudder settings necessary to bring about balance in each case, we get a mass of data that is represented in the six diagrams in Fig. 3. These

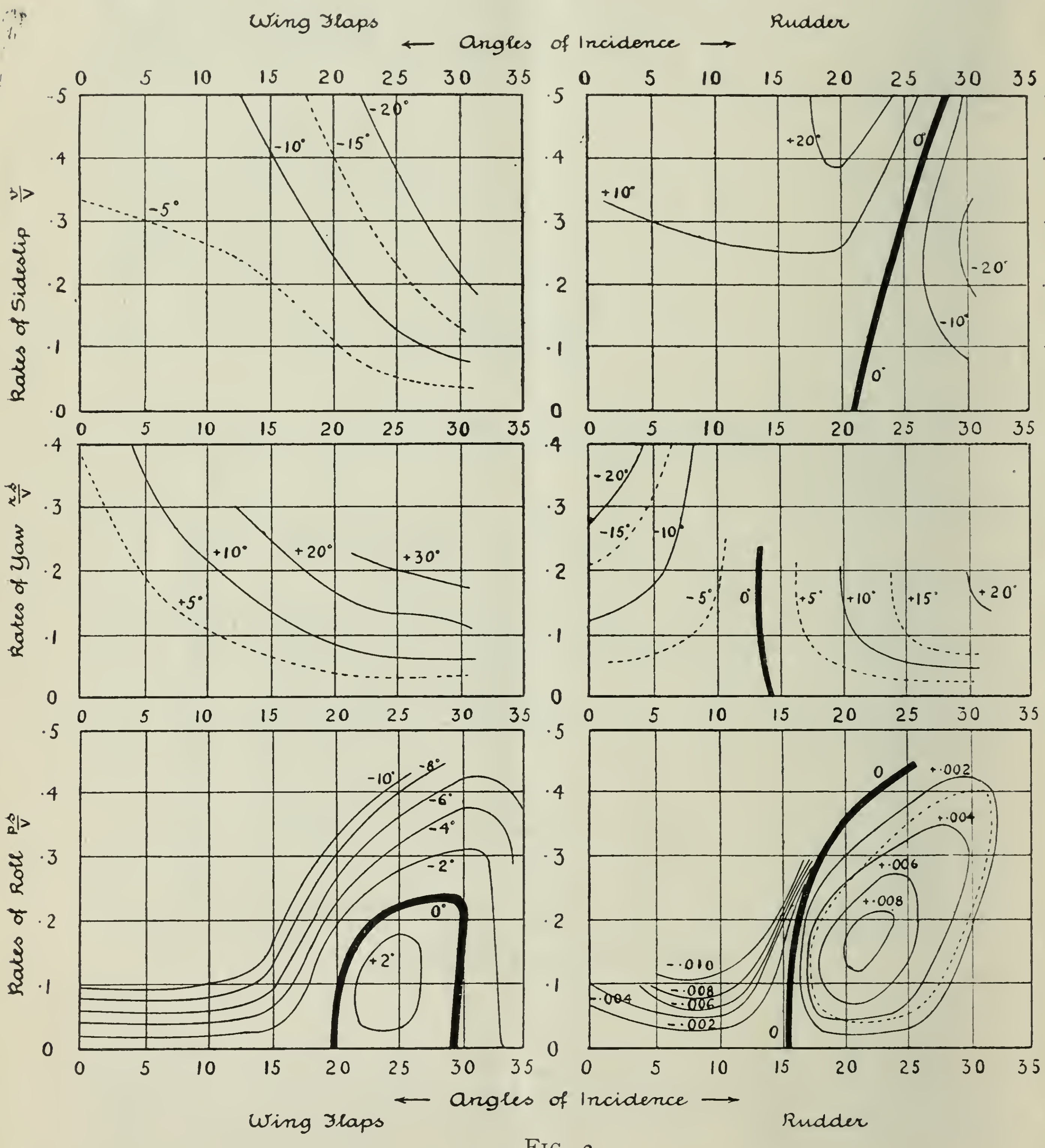

FIG. 3.

show the rates of roll, yaw and side-slip at which balance can be obtained with given settings of the controls. For example, when side-slipping at 0.25 of the forward speed, with incidence 20 degrees, balance could be obtained with — 10 degrees aileron and + 10 degrees rudder.

It is these curves that we hope will be characteristic of the controllability of the aeroplane; that is to say, we hope that aeroplanes for which these six curves are similar will in practice behave similarly. It remains for experience to decide whether this will be the case.

The curves of this diagram do not represent any one particular aeroplane. The side-slip curves are from a model S.E.5, the roll curves from a model Bristol Fighter, and the yaw curves from rough calculations of my own, which may be taken roughly to represent the S.E.5. The curves should, however, be sufficiently typical of most modern aeroplanes to allow of our discussing broadly their general features.

Let us see what information we could obtain from these curves concerning any aeroplane of which they are representative. Positive control angles mean stick to the left and left rudder. Positive side-slip is to starboard. Imagine a side-slip to starboard of 0.2 times the forward speed. The curves show that, in the unstalled régime, this requires right stick and left rudder of quite reasonable amounts, but that, as the angle of incidence increases, the necessary aileron angle increases rapidly whilst the rudder changes sign.

Now for the effect of rate of yaw. A positive rate of yaw means turning to starboard. A flat turn to starboard without side-slip, if such a thing can be imagined, involves, in the unstalled state, left stick and right rudder, and not much of either. After stalling, the amount of stick moment necessary is greatly increased and the rudder crosses over against the turn, very large angles being needed to get any rapid turn at the higher incidences. The reason for this apparent paradox is easy to understand. The effect of the turn to starboard is to raise the port wing, and to counteract this, such large aileron angles are required that they turn the aeroplane by their secondary effect on yawing moment, and this effect has to be counteracted by the rudder.

Now consider the effect of rate of roll. A pure rate of roll to starboard requires right stick and rudder in the unstalled state, and large control moments are required to get rapid rolls. After stalling, however, left stick and rudder are necessary; that is, the roll must be prevented from increasing. This, as we have seen already, is one of the primary reasons for the difficulty of stalled flying.

It will be noticed that, in the rudder-roll diagram, angles are not given, but instead the moments required from the rudder. It is convenient to do this because in this case the action of the rudder is not seriously affected by the rate of roll, so that the diagram can be applied to any rudder of known power. The peculiar interest in this diagram lies in the fact that there is one particular spot on it at about 22 degrees incidence and 0.18 rate of roll where the rudder moment required for balance has a maximum value. If the rudder power exceeds this value it will be possible to keep the aeroplane in balance over a very wide range of rates of roll and angles of incidence; but, if it falls much short of this value, quite a small rate of roll will send the aeroplane out of control.

The power of the Bristol Fighter rudder, for instance, at about 22 degrees incidence is about 0.003 on this scale, so that, if its controllability is represented by these diagrams, it should go out of control, when stalled, at a rate of roll of only 0.025 or thereabouts. This works out to about 60 degrees in 12 seconds, which is quite a slow motion.

Imagine an aeroplane that has a maximum rudder power just equal to the maximum rudder power here required. Under the worst conditions it will just be possible to reach a state of balance when a pure rate of roll is taking place, but there will be no margin over, neither to maintain balance when side-slips and rates of yaw are superimposed, nor to provide angular accelerations. Such a machine, though not hopelessly uncontrollable, will not be under adequate control.

The suggestion now made is that the reserve yawing moment available, over

and above that required to cope with a pure roll under the worst conditions, should be considered as a criterion of the controllability of the aeroplane when stalled. A single number of this nature cannot, of course, be a sufficient and only criterion of controllability. The six diagrams of Fig. 3, at the very least, will be necessary to specify controllability in detail, and we have said nothing about the stability of the balanced states, which will certainly have an important influence. The contention that I now make is, however, that this number will, more than any other single number, be representative of controllability in the stalled state.

Let us see how this criterion can be applied to existing data. The following figures are taken from wind channel experiments:—

Aeroplane.	Approximate value of the criterion.
Bristol Fighter	— 0.005
Standard Avro	0.000
Avro Large Rudder	+ 0.010

In practice we know that the Bristol Fighter is notoriously bad on the rudder at slow speeds, although it can, with great care, be flown stalled. I am unable at present to say definitely whether it is, or is not, possible to stop the initial stages of a spin with the rudder in this machine, but I think not. The standard Avro can just be controlled with full rudder at any stage in the development of a spin, whilst with the big rudder there appears to be ample rudder power under all conditions.

So far, therefore, the simple criterion suggested appears to represent the known facts approximately, but the full scale data is not yet nearly complete enough to make any definite statement on this point. The pilots are quite clear that the control of the Avro, even with the big rudder, is not as good as they would like, although they do not complain of lack of rudder power, as they do with all other stalled aeroplanes. It would appear, therefore, that other considerations, besides this simple criterion, must be taken into account in specifying an aeroplane that shall be adequately controllable when stalled, but it remains for further experience to show what these will be.

I have not discussed the question of the stability of these balanced states because we do not understand it yet. It must not be inferred that differences of stability will have as little effect on problems of control as they do in the unstalled state. In unstalled flying the types of instability that occur in conventional aeroplanes develop so slowly that they are checked by the pilot with the greatest ease. In the stalled state, however, they develop much more rapidly, and although they are still not too fast to be checked in time by an alert pilot, they develop fast enough to affect the ease of control quite appreciably.

That is about as far as we have got at present. Let us now consider future work. The pilots of the R.A.F. have developed a whole art of stalled flying, being able to fly steadily enough to take measurements up to angles of incidence of 40 degrees, but they have never yet had a machine on which they can do this comfortably, owing to the fact that we have not yet been able to alter an Avro so as to have big enough aileron and tail moments. This is being done, but the process of altering standard aeroplanes in the radical manner required is laborious and unsatisfactory, and we hope shortly to have a special type of aeroplane designed in which all the alterations that are required for research work on this subject can be easily made. If and when we get this machine, progress should go on with a jerk, provided that economy has left anyone to carry on the experiments.

The direction in which we hope to proceed is, first, to get an aeroplane that can be satisfactorily controlled when stalled—we have not quite done this yet—and, next, to get this effect with the least ugliness in design, that is to say, with reasonable sized rudders, etc. We hope to do this by finding forms of

aileron that will give the necessary rolling moment combined with yawing moments of the opposite sign to those given by ailerons now in general use. There seems to be every hope of doing this by using Handley Page slots on the wing tips, in conjunction with the ordinary ailerons; we are experimenting on this problem at the moment.

The prospect of eliminating the particularly fatal kind of accident that follows an involuntary stall near the ground in present-day aeroplanes is, in itself, sufficient inducement to carry on this work as vigorously as possible, but we have a faint hope that we may be able to go even further than this and deliberately use the stalled state to assist in making difficult landings. One of the principal difficulties of landing an aeroplane in the ordinary way is that it is impossible to alter the angle of glide without stalling on the one hand, or gathering speed on the other. The pilot who finds himself too high has therefore to resort to some terrifying stunts in order to lose height, and terrifying stunts at low speeds near the ground are not pleasant things. The peculiarity of stalled flying, on the other hand, is that you can come down at any angle you like without appreciably altering speed, and this is exactly what you want when landing.

We are still a long way off being able deliberately to use stalled flying to assist a landing, but some of us have hopes of getting there in time, and if we can do so, I, personally, believe that it will work a revolution in the safety of flying.

Discussion

Colonel Ogilvie, after thanking Professor Jones for his interesting lecture, said he had one small criticism to make. Professor Jones had said that in the early days the number of people who killed themselves became less as the power of the engines became greater. He himself believed it was the other way about. It was when people began to fly easily that the caution exercised by the earlier people was neglected, and the number of accidents increased. He should think that one of the main reasons why stalling decreased in this country was the introduction of means of seeing how fast one was going.

Another point he would like to put to Professor Jones was that, when thinking of this problem of stalling, why were we always so interested in trying to raise the lower wing? Having got that wing down and on the point of stalling, why should we not pull the other wing down to it? There might be a line of thought in that direction which would prove useful.

Another matter which he would like to bring to the attention of the meeting and one which was much more important, was that there was a movement among some of the rather influential circles in this country to give up endeavours towards safety and push towards performance. That was really a serious danger. There had been several indications of it, and unless we in the Society stood out against it and prevented it happening we should be worse off than we are now.

Mr. Handley Page said that it seemed to him that sufficient emphasis had not been laid on the fact that the stalling point should be delayed as far as possible. It was granted by everybody that the critical period of control was that which occurred round about the stalling point, and therefore the longer this was delayed the easier became the problem of control. In his opinion it was much better to fly unstalled at a large angle of incidence than to have to adopt special means for control under stalling conditions.

Both of these questions, namely, being able to fly unstalled up to, say, 45 deg., or flying steadily in the stalled condition at the same amount, required a great deal more research, and he looked forward with great interest to the further experiments which Professor Jones had outlined, and the combination of the use of the slot in front of the wings combined with the aileron.

He hoped that the research that was necessary to elucidate a good many difficult problems would be continued.

Squadron Leader R. M. Hill said he felt that, after what Professor Jones had said, there was little he could add. In any case, this subject was a very difficult one to discuss at the present stage, and, considering the difficulty, he thought that Professor Jones had dealt with it in a wonderful way, inasmuch as he had presented a vivid picture of the experiments carried out, of the difficulties surrounding them, and of what results they might be expected to yield.

What was the commonest and most serious kind of flying accident? It was the class of accident ensuing on an involuntary stall. However they might blink the fact, what had hindered flying from becoming popular in the sense that people were not apprehensive of it, was that an aeroplane, when it got into the stalled state, became temporarily uncontrollable. It was not as if this were a rare or unlikely occurrence. There was, indeed, a variety of reasons that led to involuntary stalling, the results of which were so frequently fatal. It was useless, as Profesor Jones had pointed out, to attempt arbitrarily to prevent a pilot stalling; the alternative was therefore to study the phenomenon of stalling and find means of mitigating its effects. This would become possible only when the pilot was given the power to exert control over the aeroplane, not only just below the stalling incidence, but well above it. The changes in an aeroplane's behaviour at stalling must be made less critical if the aeroplane was to be maintained in a state of steady flight at and above its stalling incidence.

How could the problem be solved? Professor Jones had suggested lines of thought which it was hoped would lead to a solution. From the flying point of view this solution would mean that the pilot could go up in his aeroplane, and, if the engine failed near the ground, he could pull back the control stick and keep it back without being deprived of the power of orienting the aeroplane at will. The worst that could then happen would be that it would strike the ground at a downward velocity of not more than 25ft. per second, a forward speed little in excess of its stalling speed, and with the fuselage practically horizontal. That was the sort of crash that might shake, but it would not kill. If it were necessary to make a pointed comparison, a description of the sequence of events which led up to the present day crash consequent on stalling would serve; they were, however, only too well known. Squadron Leader Hill added that if the results hoped for were achieved, and the experiments so carefully tended by Professor Jones bore fruit, flying would be revolutionised.

Professor Melvill Jones, in reply, said: Squadron Leader Hill's speech puts the case for continued work on the problems associated with the control of a stalled aeroplane very clearly; it requires no answer from me, and I will merely state that I am in agreement with everything that he has said.

In answer to the points made by Colonel Ogilvie. When I implied that a higher proportion of the early pilots met with disaster than is the case with modern pilots, I had in mind the very early pioneers in the period before and just after the Wrights appeared on the scene. I do not think that there is any doubt that these people did experience great trouble with their controls and that many of them were killed in consequence. It was, in fact, the great skill shown by the Wrights in grappling with these difficulties of control that differentiated their work from that of their predecessors. With regard to the idea of lowering the upper wing rather than attempting to raise the lower one, both methods of tackling the problem are being investigated experimentally.

I was glad to hear Colonel Ogilvie raise the question of the relative importance of safety and performance. I am strongly of the opinion that provided a cruising speed of about 100 miles per hour can be obtained it is infinitely more important, in the commercial sense, to reduce all dangers of a fatal crash to a vanishing point than to obtain still greater speeds, and I am in direct disagreement with the

opinion, expressed freely at the last Air Conference, that it will be practicable to attain this end by the elimination of engine failure. By all means let us reduce engine failures to the lowest possible limit, but, on top of that, let us arrange, if we can, that a failure when it does occur seldom if ever results in a fatal accident. Both these achievements are in my opinion necessary before the ordinary man going about his ordinary business will sit up and take notice of aerial transport.

In answer to Mr. Handley Page, I would say that there are two distinct problems to be solved—they are to fly slowly and to have control when doing so. I stated at the beginning of my paper that I would confine myself to the latter. This does not mean that I am not awake to the importance of obtaining the slow speed, whether by means of low loading or by the use of high-lift coefficients. It merely means that the two subjects, taken together, are too big to be dealt with in one lecture. Possibly Mr. Handley Page considers that if the stall can be postponed to a large angle, the necessity for studying the control after stalling will not arise; if this is his view, I differ from it, because there is no limit to the slowness with which a pilot would like to fly if he could do so safely; the result of putting off the stall and thus increasing the lift coefficient will, therefore, almost certainly be that the aeroplane will be treated as having a lower landing speed than would otherwise be the case and there will be the same danger of overstepping the safe condition as at present. Our object up to the present has been to reach an understanding of the problems of control after stalling, and we have considered that this is more easily done on conventional aeroplanes, to which we are all accustomed, than by mixing the two problems of controlling an aeroplane and of obtaining specially high-lift coefficients. There is, however, no doubt that the two problems will eventually have to be studied together.

A hearty vote of thanks to Professor Melvill Jones for his exceedingly interesting and inspiring lecture concluded the proceedings.

Contributed Notes on Prof. Melvill Jones' Paper by Major T. M. Barlow.

I should like at first to congratulate Professor Jones on the definite stand he has taken up against those short-sighted critics who wish to push along with such complicated research problems as outlined in this paper, on the "hit or miss" principle. From my own experience in the testing of new types it is quite evident that as yet we are only on the fringe of definite information as to the most efficient (in all senses of the term) form of controls. At Martlesham Heath we have for some time now, as a matter of routine, carried out slowest speed trials, using the Camera Obscura method, and these tests have emphasised the difficulties which designers, through lack of information, have had to contend with to produce adequate control throughout the large speed ranges now ruling in high-powered aircraft. I hope, therefore, the work initiated by Professor Jones will be extended to controls over the whole speed range.

At Martlesham also we have been obtaining records of longitudinal stability of most aircraft from slow to top speeds by the Pitching Camera. These all provide a certain amount of useful data. It seems to me, however, that it should be possible for the Advisory Committee Panel to draft out some form of flight tests which, with suitable apparatus, could be carried out on all types, and thus a collection of reliable technical data would be obtained. I am in agreement that a special machine would be of immense help to deal with the more intricate problems, but progress will undoubtedly be hastened if information could be collected systematically from firms' tests pilots and Service experimental stations. There would be difficulties probably in the matter of suitable instruments and apparatus, but not insurmountable.

Stalled flying with full control will have to come, and this I think will be attained by following to a certain extent nature and using in flight variable areas and cambers with perhaps special devices such as the Handley Page slotted wing.

ON THE VORTEX PAIR QUICKLY FORMED BY SOME AEROFOILS

BY N. A. V. PIERCY, D.SC.

It was suggested by Lanchester* some years ago that an aerofoil immersed in a moving fluid gave rise to two trailing vortices of opposite hand, one situated near each of the extremities of span. The existence of the vortex pair has since been demonstrated in a variety of ways, of which perhaps the most striking is that due to Caldwell and Fales,† who used the condensation of water vapour in a high-speed wind channel to obtain photographs. The vortex pair constitutes so important a feature in the stream behind most aerofoils that measurements capable of numerical analysis seemed desirable, and an account is given in the present Paper of some work to this end which appears capable of straightforward interpretation.

It may be that the residual motion sufficiently far behind any aerofoil is a vortex pair. Similar experiments to those about to be described, however, which deal with the motion only for a few chords back, have been made with several other aerofoils of a variety of shapes having long, graded wing tips, and with some of these no distinct vortex was found. With others apparently more than one pair could exist in certain circumstances, but opportunity has not occurred to probe deeply into these interesting cases. Attention is confined to two aerofoils representative of considerable variation in a class having uniform section and angle of incidence with blunt, or square-ended, wing tips. The general nature of the results is common even with aerofoils outside this class, but the foregoing remarks will serve to indicate that exceptions are known to exist.

In the course of the investigation an attempt is made to throw some light on the more general question of the structure of a vortex well on into turbulent flow. If the eddies are to be usefully employed in aerofoil theory—or indeed, since they are so commonly to be found in real flow past other bodies, in any synthetic account of a fluid motion problem which avoids a solution of the viscous equations—information on this score is hardly less important than knowledge relating to position and strength.

The experiments were carried out in the 4ft. wind channel of the aeronautical laboratory of East London College. The channel was driven from the battery of the electrical engineering side (by kind permission of Prof. Macgregor-Morris), so that the velocity was kept exceptionally constant. The apparatus for measuring the angle of the stream was as follows. The angle head was of similar form to that for some time used at the N.P.L., and consisted of two fine tubes, inclined at 45 deg. to the stream, with their open mouths touching (see sketch in Fig. 1). This was mounted on a cranked arm turned by a micrometer wheel outside the channel, so that it could be rotated without displacement of the mouths of the tubes. The turning gear was mounted on the tail-stock of the lathe, which in turn was carried on the lathe bed—fixed outside the channel at right-angles to the wall. Thus traversing of the instrument along a line parallel to the trailing edge of the aerofoil was arranged for, as well as orientation about that line as axis. The aerofoil could be traversed without change of incidence in the direction of its lift. The two tubes were

* Lanchester, "Aerodynamics," page 171 (1909 edition).
† Caldwell and Fales, American Advisory Committee for Aeronautics, Report 83 (1920).

connected across a Chattock gauge and, in use, were oriented until the pressures in them were equal. Errors in the manufacture of the instrument, warp of the lathe bed, and angle variations in the channel not due to the model were allowed for by a preliminary exploration with the model removed. Speed variation with position, not caused by the aerofoil, was also determined in a similar manner.

Character and Strength of the Vortex

Aerofoil *A* was rectangular in plan-form, 24in. span by 4in. chord, with a thick section somewhat resembling that known as R.A.F. 19. At 8deg. incidence and a speed of 31.3ft./sec., the distribution of downwash angle along a line parallel to the trailing edge, near its level and two chords behind it, is given in column two of Table I., and plotted in Fig. 1. The exact z level of the experimental line was so chosen as to give maximum upwash at the point H, corresponding nearly enough with maximum slope of the line JH. The nature of the readings clearly indicates the existence in some form of Lanchester's vortex with its centre at the point J and with a core—0.6in. diameter—of fluid having approximately uniform angular velocity.

TABLE I.

MODEL A.

(Span = 24in., Chord = 4in., Speed = 31.3ft./sec.)

Distance outside wing tip (inches).		Angle of downwash (degrees).
—12.0	...	6.1
—10.0	...	5.9
—8.0	...	6.3
—4.0	...	8.1
—2.6	...	10.6
—1.8	...	14.3
—1.4	...	19.9
—1.2	...	25.7
—1.0	...	34.4
—0.95	...	35.2
—0.9	...	34.0
—0.8	...	26.4
—0.7	...	22.2
—0.65	...	0.0
—0.6	...	—5.0
—0.5	...	—20.7
—0.4	...	—34.0
—0.35	...	—35.0
—0.3	...	—33.0
—0.2	...	—28.7
—0.1	...	—24.5
0.0	...	—21.3
0.4	...	—13.9
1.0	...	—9.4
2.0	...	—6.3
3.0	...	—4.5
4.0	...	—3.4
5.0	...	—2.5
6.0	...	—1.8

It may be remarked that the form of such a curve—apart from the magnitudes of the readings—may be insufficient to decide on the existence of a columnar

vortex. Thus, immediately outside the wing tip of some rectangular aerofoils, a crosswash curve of the form in Fig. 1 may be associated with a continuous upwash curve, showing uniform shear without rotation in the sense of eddying. In the present case, a rough exploration of crosswash along a line at right-angles through J checked the deduction to be derived from a quantitative consideration of the readings.

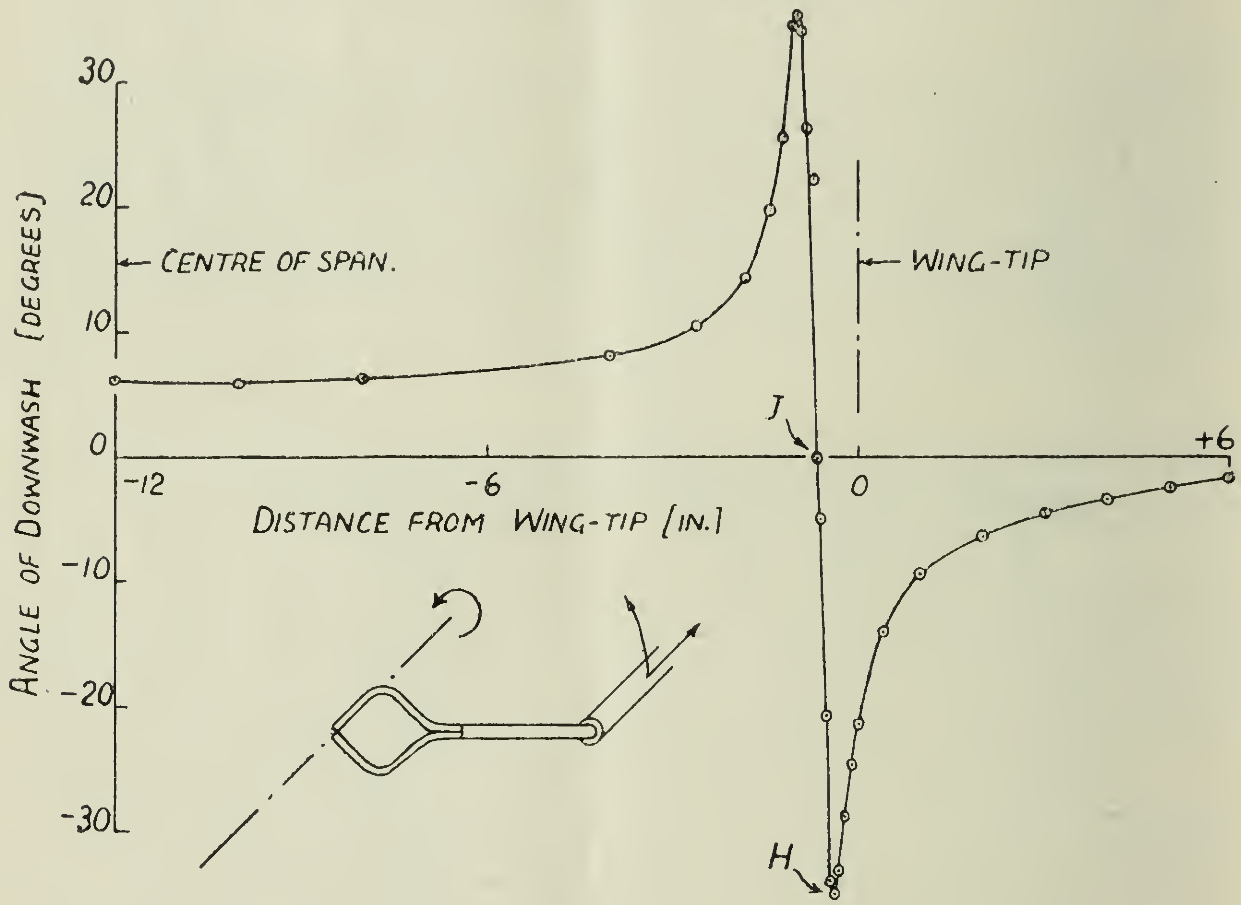

FIG. 1.
Downwash angles, Aerofoil A.

To examine the details of the vortex, it transpires that the simplest point of view is probably sufficient. More theoretical difficulties exist, however, than may appear at first sight, and a preliminary discussion is needed.

In the case of non-turbulence, Lamb teaches from Stokes that the spinning about its axis of a long circular cylinder in a viscous fluid initially at rest sets up a circulatory motion, the angular velocity of which, when steady, is inversely proportional to the square of the radius. Stresses now exist which impose a couple on the cylinder, but the form of the motion is the same as with an infinite rectilinear vortex in an inviscid fluid, except as to effects possibly arising from the question of the stability of a fluid core. A hint may thus be conveyed as to the manner in which viscosity, together with the aerofoil, generates the eddy, but whether, far past the critical speed, the average form of the eddy is still similar to that of an inviscid vortex remains an interesting question. Moreover, the superposition of the general stream, causing the particles to move on the average in helical paths, changes the direction and magnitude of the stresses. Added to this, the circulation depends in part on the history of the vortex pair and of other rotations, which may exist elsewhere without giving rise to distinct cored vortices.

Looking at the problem from the standpoint adopted by Levy,* complete information regarding the distribution of vorticity at any instant would enable the instantaneous values of the upwash readings to be calculated without reference to viscosity, since the experimental points are far removed from a boundary. The same might be more or less true of average values, and under very simple conditions the process would be reversible. Nothing like the experimental knowledge theoretically required is available, of course. On the other hand, a glance at the numerical values in Table I. shows that the eddies at the wing tips are of outstanding importance in determining the motion adjacent to them.

There are thus some theoretical grounds for applying inviscid methods to the present problem, though the process clearly remains to be justified or condemned by experiment. The first assumption involved is that modifications of the vortices in the x direction (parallel to the undisturbed stream) due to viscosity are negligible in their effect on the upwash readings. Other assumptions require to be made regarding the configuration of the vortex in three dimensions, and it is necessary to choose between these. For this purpose, the principle has been adopted that the circulation due to the vortex round its core shall be independent of the path. Only the outer part of the vortex is examined so as to minimise as far as possible the superposed velocities arising from imperfectly known causes. The x component u of the velocity q is assumed constant in this region and equal to its undisturbed value V.

The simplest assumption as to configuration is to suppose that the effects of undetermined factors balance out in the experimental region, so that the whole velocity of upwash (w) is due to the trailing vortex pair. Neglecting also the limitation of length upstream, the problem becomes two-dimensional, and w outside the core is given by

$$w = (K/2\pi)\,[1/y - 1/(l + y)] \quad . \quad . \quad . \quad . \qquad (1)$$

where K is the strength of the vortex (equals twice the angular velocity of the core, or the circulation round it), l is the distance apart of the vortices, and y is measured outwards from the centre of the core.

To allow for the walls of the channel, it is theoretically necessary to introduce a complicated image system arranged in doubly-infinite columns and rows. A great simplification may be obtained, however, if we accept the notion that the substitution of a circular channel for the square one actually used would not affect the upwash observed by much more than the experimental errors. In this case two images are sufficient, each distant $c^2/\frac{1}{2}l$ from the centre of the channel, of which the radius is c. The formula (1) is then amplified to

$$w = (K/2\pi)\,[1/y - 1/(l + y) + 1/(2c^2/l + l/2 + y) + 1/(2c^2/l - l/2 - y)] \qquad (2)$$

and we may choose the arbitrary value 2.1 (ft.) for c so as to give reasonable coincidence over a fair space to the supposed and actual walls.

Now suppose the vortices only extend upstream as far as the mid-point of the chord, and are there attached to the surface. To calculate the velocity we have to go back to the general formula

$$\delta q = (K/4\pi)\,(\sin\beta\delta s/r^2) \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

where r is the distance of a small length δs of a vortex from the point at which q is required, and β is the angle between r and δs. A suitable application of this gives

$$w = (K/4\pi)\,\{\,(1/y)\,(1 + x/\sqrt{\{x^2 + y^2\}}\,) - [1/(y + l)]\,(1 + x/\sqrt{[x^2 + (y + l)^2]})\,\} \qquad (4)$$

x being the distance of the yz plane of the measurements behind the mid-point of the chord. As before, an approximate allowance for the walls of the channel may readily be added.

* Levy, Aeronautical Journal, Vol. XXIII., page 335 (1919).

As a final alternative, following Prandtl, let us assume the aerofoil to be replaced by a transverse vortex joining the upstream ends of the trailing vortex pair. We get, from (3)

$$w = (K/4\pi)\,[\,(1/y)\,(1 + x/\surd\{x^2+y^2\}) - \{1/(y+l)\}\,(1 + x/\surd\{x^2+(y+l)^2\}) \\ - (1/x)\,\{(y+l)/\surd x^2+(y+l)^2 - y/\surd(x^2+y^2)\}\,] \quad . \quad . \quad . \quad (5)$$

but correction for the channel is more cumbersome. The simple nature of the substitution for the aerofoil requires x to be sufficiently great, and this we shall have to assume to be satisfied by the experiments, for the time being. Another assumption of (5) is that K is not only constant along the trailing members of the filament (which may, if desired, be considered re-entrant by supposing a second transverse member at infinity), but has also the same value along the transverse member. This question will be referred to again.

The first three columns of Table II. give values of $K/2\pi$ as calculated by formulæ (1), (4), and (5) from some of the upwash readings of Table I. Neglecting the walls of the channel leads to an apparent increase of K with y in every case. All the methods give approximate agreement close to the core, and little difference results, over the present range of y, in taking into account the limited extent upstream of the vortices. The substitution for the aerofoil, however, doubles the variation and there appears little hope of obtaining a constant value for K by the use of such formula as (5). Returning, therefore, to the method of (1) and (4), but allowing for the walls of the channel, we obtain the last two columns of the table showing that the image corrections secure fair constancy for K.

TABLE II.

Application of the Formulæ, Model A.

y (chords) from centre of core.	$K/2\pi$ from formulæ. (1)	(4)	(5)	(2)	(4) modified.
0.112	0.66	0.66	0.67	0.65	0.65
1.41	0.81	0.80	0.96	0.63	0.69

Comparing the last two columns, the small advantage suggested in favour of (2) was verified in another example of greater y range taken from Table IV. below. This showed that the two-dimensional assumption gave practically a unique value for K. Table V. below clears up the question raised in choosing the points for Table II. Formula (2) is therefore used in the following work to reduce the readings. The failure of (5) is of course no comment on Prandtl's aerofoil theory, for the formula may be incomplete as to important details. For instance, a number of similar but narrower and weaker vortex filaments might lie between the two trailing vortices, so arranged as not to give rise to cores. These might build up the transverse vortex towards the centre of span and yet, on the whole, yield an upwash in the experimental region sufficient to balance—or partly so—the downwash from the wing.

In applying (2) to the figures obtained from Model A it has been chosen to fit best the points adjacent to the core. The calculated curve of upwash so obtained passes fairly through the distant points but somewhat misses intermediate values, as will be seen by comparing columns 3 and 4 of Table III. The value obtained for K in this way is 4.08 (ft./sec. units). On the whole the agreement is not bad and, in view of the greater success obtained later, the discrepancies may fairly be regarded as experimental errors, until further work is available for comparison.

The next step in the work was to examine the extent to which the assumption of constant u is valid. This was carried out by measurements of resultant

velocities in the following manner. A small T-shaped arrangement of two fine hypodermic tubes was used, one pointing upstream to measure the dynamic head and the other pointing downstream immediately behind it. The instrument was first calibrated for speed and inclination to the wind and then drawn through the centre of the vortex. The readings obtained, corrected for previously observed angle variation, are given in column 5 of Table III., where they may be compared with values obtained from the calculated values of w (column 6). The agreement, which is satisfactory to within a short distance of the core, may alternatively be inspected from the values of u as deduced from the measured angles and resultant velocities (column 7).

TABLE III.

Vortex of Model A.

1. y in.	2. Expertl. angle. deg.	3. Expertl. w ft./sec.	4. Calcd. w	5. Expertl. q	6. Calcd. q	7. Expertl. u	8. Expertl. p lb./sq. ft.	9. Calcd. p
0.0	0.0	0.0	0.0	36.0	31.3	36.1	1.57	1.60
0.05	5.0	2.75	4.33	—	—	—	1.51	1.58
0.15	20.7	11.8	13.0	—	—	—	1.45	1.40
0.25	34.0	21.1	21.7	36.8	38.1	30.5	0.94	1.045
0.30	35.0	21.9	26.0	37.5	40.7	30.7	0.73	0.80
0.35	33.0	20.3	22.2	36.7	38.4	30.8	0.55	0.59
0.45	28.7	17.1	17.3	35.5	35.8	31.1	0.36	0.355
0.55	24.5	14.3	14.2	34.4	34.4	31.3	0.265	0.24
0.65	21.3	12.2	12.0	33.6	33.5	31.3	0.25	0.17
1.05	13.9	7.75	7.4	32.0	32.2	31.0	0.10	0.065
1.65	9.4	5.2	4.7	31.5	31.65	31.1	0.025	0.026
2.65	6.3	3.45	2.95	31.5	31.45	31.3	0.015	0.010
3.65	4.5	2.45	2.15	31.4	31.38	31.3	0.005	0.005
4.65	3.4	1.85	1.7	31.3	31.35	31.3	—	—
5.65	2.6	1.4	1.4	—	—	—	—	—
6.65	1.9	1.05	1.2	—	—	—	—	—

Another check and point of interest is provided by an examination of the pressures measured separately in the backward-turned tube of the T-shaped instrument. After correction for angle and speed in accordance with a separate calibration, these give the static pressure distribution through the vortex. The corrected experimental pressures are tabulated in column 8 of the same table, while in the last column are given the corresponding values calculated from K in accordance with the well known formulæ for an inviscid fluid, viz.,

$$p = \rho K^2/8\pi^2 r^2 \text{ and } p = (\rho K^2/4\pi^2 a^2)(1 - r^2/2a^2) \qquad . \qquad . \qquad (6)$$

for outside and inside the core, respectively. In (6) p is the pressure drop relative to the pressure of the undisturbed stream, at a radius r (equals y) and a is the radius of the core. In view of the special need in the application of (6) for accurate estimations of K and a, the agreement with the experimental results is better than might well be anticipated on experimental grounds.

The curves (a), (b), and (c) of Fig. 2 are plotted from columns 4, 6 and 9 of Table III., and are therefore theoretical, referring to w, q and p respectively. The observations are shown as points. The values most distant from the core are omitted.

The measured velocity at the centre of the core has a peculiar interest. It has closely the value which would be produced from rest (at the pressure of the

undisturbed stream) by the observed pressure reduction. If the central portion of the core could be traced upstream to an originating point such that the vortex motion had not there been formed, the mechanical energy at that point would thus be $\frac{1}{2}\rho u^2$ less than in the undisturbed stream.

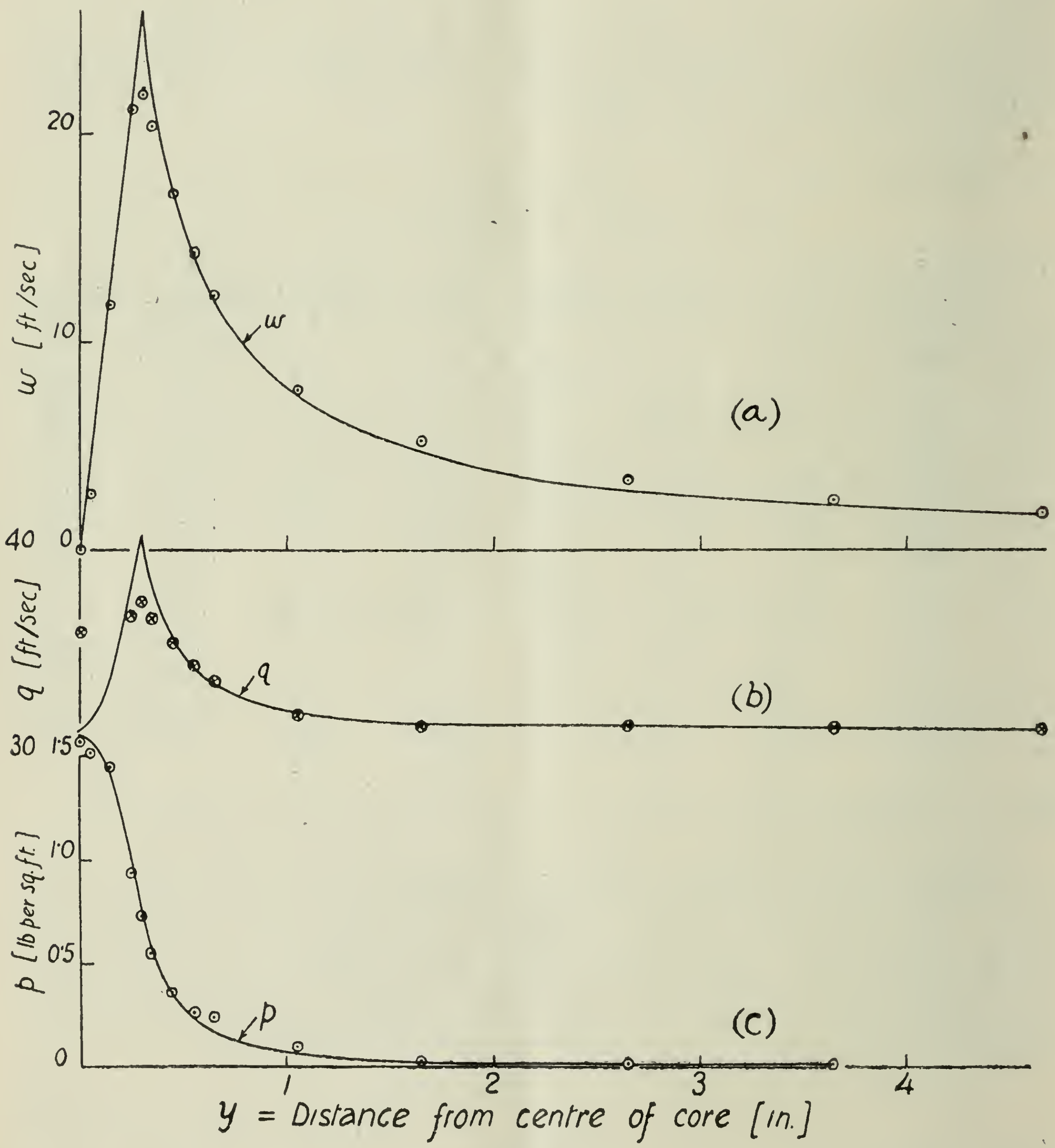

FIG. 2.

Details of vortex of Model A.

Variation of Vortex Strength

The results so far obtained encourage the belief that the present method of analysis is sufficient and that a viscous vortex exhibits to perhaps a remarkable degree the properties developed by Helmholtz. Further work is justifiable, and proceeding on the same lines, the increase of the strength of the vortices with incidence has been determined for a square-ended aerofoil B of R.A.F.15 section, measuring 18in. span by 3in. chord. The wind speed was 40ft./sec.

The observed downwash angles are given in Table IV., y being measured from the wing tip, for six angles of incidence. Values of w are compared in Table V. with those calculated from the best value of K in every case except the greatest angle. Agreement will be observed to be good. With the greatest angle (20deg.), the points were found to lie closely on a theoretical curve obtained by assuming a displacement of the centre of the vortex from the measured position. The calculated values given were derived from this curve. The close agreement suggests that the advancing measuring apparatus, on approaching the core closely, sufficed to push it inwards at this angle of unstable flow. Readings for 0 deg. were taken but are not included on account of known experimental inaccuracies. A core was formed, but there seemed reason to believe that either the external motion was not a vortex at this distance behind the aerofoil or a change of method was required for analysis. The readings were such as to suggest the possible applicability of equation (5).

TABLE IV.

MODEL B.

(Span = 18in., Chord = 3in.)

Angles of downwash in degrees. Speed : 40ft. per sec.

Distance outside wing tip (in.)	Angle of incidence (degrees).					
	3.1	7.1	10.2	13.2	17.0	20.0
— 9.0	2.8	3.8	4.7	4.4	2.8	—
— 7.5	2.4	3.65	4.4	4.4	2.3	—
— 6.0	2.4	3.75	4.7	4.8	4.8	—
— 4.5	2.5	4.0	5.2	5.9	7.6	5.0
— 3.0	2.85	4.6	5.8	6.8	9.8	12.3
— 1.5	3.5	6.15	8.6	10.4	13.6	18.6
— 1.2	4.4	7.9	11.1	13.3	17.2	23.7
— 0.9	6.9	12.1	17.3	21.0	24.5	31.8
— 0.75	10.0	16.4	22.2	28.5	31	33
— 0.65	—	19.6	26.5	31	32.5	27
— 0.6	13	21.1	27.5	31	32	25
— 0.55	10.6	21	26	30.5	29	15.5
— 0.5	8.6	17	19	24.5	27	4
— 0.45	— 4	7.5	3	17	24.7	— 11
— 0.35	— 11.5	— 10.5	— 16.5	— 9	— 6	— 22.8
— 0.3	— 12.4	— 18	— 22	— 22.5	— 20	— 25.3
— 0.25	—	— 20.8	— 25	— 26.5	— 24.2	— 25.6
— 0.2	—	— 20.4	— 24.3	— 29	— 27.8	— 25.5
— 0.15	— 10.2	— 19.0	— 23.3	— 29.5	— 29.7	— 25.0
— 0.1	—	— 17.2	— 21.1	—	— 29.4	— 23.9
0.0	— 8.4	— 13.6	— 17.3	— 23.5	— 25.4	— 21.1
0.15	— 6.6	— 12.1	— 14.5	— 17.9	— 20.0	— 18.2
0.3	— 5.5	— 10.0	— 13.2	— 15.4	— 16.6	— 15.4
0.6	— 3.9	— 7.3	— 9.6	— 11.3	— 12.5	— 11.9
0.9	— 3.1	— 5.8	— 7.3	— 8.7	— 9.4	— 9.2
1.2	— 2.5	— 4.4	— 5.8	— 7.0	— 7.6	— 7.3
1.5	— 2.1	— 3.8	— 4.8	— 5.7	— 6.1	— 5.9
3.0	— 1.2	— 2.2	— 2.7	— 3.2	— 3.4	— 3.2
4.5	— 0.8	— 1.6	— 1.8	— 2.2	— 2.3	— 2.2
6.0	— 0.6	— 1.2	— 1.4	— 1.6	— 1.6	— 1.6
7.5	— 0.5	— 1.0	— 1.1	— 1.3	— 1.4	— 1.3

The six values of K are given in Table VI. together with those of the lift and drag coefficients. Dimensional theory states that, for geometrically similar models, K will vary as the product VL, subject to a scale correction. The quantity K/VL has therefore been tentatively denoted as the "Vortex coefficient" and is also tabulated. In Fig. 3, K is plotted against α and K/VL

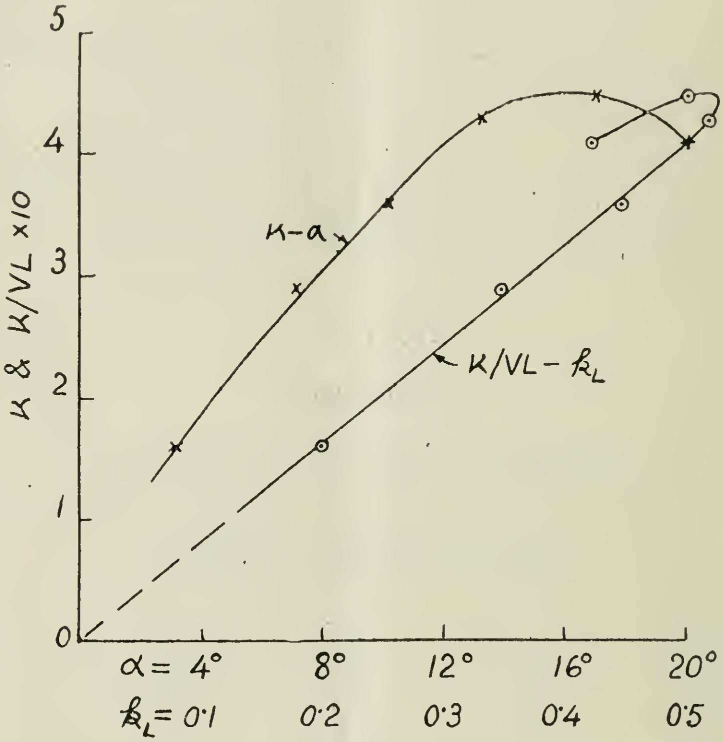

FIG. 3.

Increase of strength of vortex of Model B with incidence.

against k_L. It will be seen that K is linearly related—or nearly so—to the lift over a wide range of flying angles, and reaches a maximum value at a somewhat greater angle than that of maximum lift.

The observations of downwash behind the centre of span at the larger angles (Table IV.) serve to indicate the well-known early failure of the middle portion of the aerofoil. They are not, however, a direct guide to loss of lift, since the downwash curve in the z direction is found in these circumstances to have a double peak instead of the single one characteristic of smaller angles. Thus at 20 deg. and 1.5 chords from the symmetrical plane the minimum downwash was observed to be -0.4 deg. at a little distance below the level of the trailing edge of the aerofoil, while the two turning values were about $+7.0$ deg. and 2.6 deg., at 0.5 chords above and below this level respectively.

The whole of the foregoing work was carried out at 2.0 chords behind the wing. The results obtained both numerically and in form may be expected to depend on this distance for reasons which have already been set out and also for the time required by the wing to produce the vortex. A remarkable feature, however, is that this variation has been found to be small over a considerable range. Some measurements on this question are given in Table VII. Even so close as 0.5 chord behind the trailing edge it was unexpectedly found that formula (2) was still the best for the readings and agreed with them well, giving the vortex to be almost in full strength. The small mutual approach—or bending inwards of the cores—may also be gathered from the table. A rather rough test has been made at 12 chords behind an aerofoil without revealing any great modification. It seems then that with this class of aerofoil the specification of down-

stream distance is unnecessary, within limits. For a few chords behind the aerofoil, but away from its immediate neighbourhood, the cores are just within the wing tips, their centres being separated by 0.95 of the span. This ratio is approximately independent of the angle of incidence, and is about the same for the two models. Prandtl's work gives 0.79 for this number at infinity.

TABLE V.

VELOCITIES DUE TO VORTICES OF MODEL B.

(w deduced from angle of stream, w^1 calculated from K.)

Angle of incidence (degrees).

y^1	3.1		7.1		10.2		13.2		17.0		20.0	
(in.)	w	w^1	w	w^1	w	w^1	w	w^1	w	w^1	w	w^1
0.3	3.85	3.96	7.06	7.54	9.40	9.70	11.02	11.19	11.9	12.32	11.0	12.9
0.6	2.73	2.82	5.13	5.28	6.77	6.72	8.0	7.84	8.87	8.50	8.44	8.53
0.9	2.17	2.18	4.07	4.07	5.12	5.12	6.12	6.01	6.63	6.47	6.48	6.36
1.2	1.75	1.77	3.08	3.28	4.08	4.14	4.92	4.86	5.34	6.21	5.12	5.05
1.5	1.47	1.49	2.66	2.75	3.36	3.48	4.00	4.07	4.28	4.37	4.12	4.18
3.0	0.84	0.82	1.54	1.51	1.88	1.88	2.24	2.23	2.38	2.36	2.24	2.22
4.5	0.56	0.56	1.12	1.03	1.26	1.29	1.54	1.53	1.61	1.61	1.52	1.52
6.0	0.42	0.43	0.84	0.79	0.98	0.98	1.12	1.16	1.12	1.22	1.12	1.14
7.5	0.35	0.35	0.70	0.64	0.77	0.79	0.91	0.95	0.98	0.99	0.92	0.92

(y^1 = distance outside wing tip, velocities in ft. per sec.)

TABLE VI.

COEFFICIENTS OF VORTICITY, LIFT AND DRAG. MODEL B.

Angle of incidence (degrees)	K	K/VL	k_L	k_D
3.1	1.6	0.16	0.200	0.0125
7.1	2.9	0.29	0.348	0.024
10.2	3.6	0.36	0.445	0.0375
13.2	4.3	0.43	0.515	0.054
17.0	4.5	0.45	0.500	0
20.0	4.1	0.41	0.420	

(L = chord in ft.)

TABLE VII.

MODEL B.

Angle of incidence = 13.7°.

Distance behind trailing edge (chords)	K (ft./sec. units)	l
0.5	3.85	1.46
2.0	4.25	1.43
4.0	4.35	1.41

Applying dimensional theory we observe that both l and a should vary with the scale, subject to a viscosity correction. The observed core diameter for Model A suggests the possibility of a large core being present with the full-scale machine. The core is much smaller, however, than has been advanced on theoretical grounds. Estimation of the size of the core which may be derived from Table IV. for Model B should be treated as only of an approximate nature.

In contrast with angles well outside the core, those within it in the case of this aerofoil appeared to be very unsteady and repeat readings were wide apart. Plotting Table IV. showed the estimated averages to be a little incoherent, so a static pressure check was made. From this it was calculated that the diameter of the core at 5 deg. should be nearly 0.5in., instead of the considerably smaller value indicated by the downwash readings. Owing to the unsteadiness the analysis of the readings has not been carried quite so close to the core as with aerofoil A—where the readings were steady with the same damping. The estimations for K may be slightly on the high side on this account, but it is surmised that the apparatus may have been too clumsy for the smaller model and have caused the cores to behave in the manner of a spring on passing the measuring head through them, leading to an underestimate of size.

On the whole, the core diameter appears to increase somewhat with increase of incidence, though no clear interpretation is possible. Some interesting conceptions may be roughly formed from the vortex readings at say 17 deg. At a radius of 0.1in. from the centre of the core, the mean rotational component of speed is over 17ft./sec. This makes w about 2,000, so that the centre of the core is rotating at over 300 revs. per sec. The drop in pressure at the centre should exceed pu^2 and the velocity there should be some 50 per cent. in excess of the general speed, applying the results obtained from Model A.

Impulse and Energy Loss

The equations of impulsive motion of a real fluid being identical with those of inviscid fluid, we may apply the theory of impulse of the latter case to seek a partial balance between the force on the aerofoil and the rate of change of momentum imparted to the stream. The rate of change of impulse required to generate continuously the vortex pair is (neglecting the cores) $K\rho Vl$. Equating this to a part of the force, say Z,

$$Z = K\rho Vl \qquad (7)$$

Alternatively (7) might have been inferred from dimensional theory in the form

$$Z = K\rho Vlf\,(Vl/v,\ K/v) \qquad (8)$$

The same formulæ are obtained for the lift due to cyclic flow about a solid transverse core, if of length l, in the stream.

Using (7), Z may readily be calculated from the experimental results, yielding row 2 of Table VIII. Row 1 gives the angles of incidence, row 3 the lift in lb. of the aerofoils as determined on the wind channel balances, and row 4 the ratio Z/lift. The last column refers to the Model A, the others to the second aerofoil B.

TABLE VIII.

	Aerofoil A.						Aerofoil B.
α (degrees) ...	3.1	7.8	10.2	13.2	17.0	20.0	8.0
$K\rho Vl$ $(=Z)$ ...	0.216	0.394	0.496	0.584	0.613	0.569	0.58
Lift	0.285	0.495	0.633	0.732	0.711	0.598	0.97
Z/lift	0.76	0.80	0.78	0.80	0.86	0.95	0.60

According to the Prandtl aerofoil theory, Z is inclined backwards a little from the direction of the lift, and the present experiments do not throw any light on this question. Considered numerically, however, the results in the last row of the table would evidently be little changed by a slight inclination of Z. In the case of the aerofoil B the impulse per sec. required for the vortex pair amounts to about 80 per cent. of the lift at the smaller angles, while at the greatest angle almost all the lift may be traced in the stream in this manner. The proportion is not constant for different aerofoils.

If Z is at an angle to the lift, what has been called the "induced drag" arises from the generation of the vortices. Entirely apart from this question, there are two causes by which the vortices contribute to the drag of the aerofoil. The first is directly traceable to the presence of cores, while the second follows from the difference of the external motion, specially near the cores, from the inviscid form.

Separating the effects of the vortex from other components of the flow, the difference due to the vortex in the rate at which mechanical energy crosses the yz plane is given by (in accordance with the principles discussed elsewhere)*

$$E = \int u . D . 2\pi r dr \qquad . \quad . \quad . \quad . \quad . \qquad (9)$$

where u is the *disturbed* velocity component, and D the pitot head difference caused by the vortex. The assumption : u = its undisturbed value outside the core leads at once, on substituting from (6) to $E = 0$ for that region. In fact, the first equation of (6) is obtained by assuming the equivalent of $E = 0$ outside the core.

Inside the core, however, $q = \sqrt{(u^2 + w^2 r^2)}$ or for the velocity to be continuous at $r = a$,

$$q = \sqrt{(u^2 + K^2 r^2 / 4\pi^2 a^4)} \qquad . \quad . \quad . \quad . \qquad (10)$$

Hence (9) gives $E = \rho u K^2 / 8\pi$, an expression independent of the radius of the core. Equating this to the rate at which work is done by the fluid against the corresponding portion R of the resistance of the aerofoil, we get for the two vortices (neglecting mutual effects)

$$2R = \rho K^2 / 4\pi \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

For the Model B and for angles from 7 deg. to 13 deg., (7) gives resistances amounting to about 5 per cent. of the whole aerofoil.

TABLE IX.

ENERGY LOSS. MODEL A.

y (in.)	$lq(D)$	$lq(D)$ (u const.)
0.0	42.8	50.0
0.25	15.1	15.3
0.3	6.9	0.0
0.35	3.5	0.0
0.45	0.9	0.0
0.55	0.7	0.0
0.65	2.3	0.0
1.05	1.45	0.0
1.65	0.35	0.0
2.65	0.03	0.0
3.65	—0.06	0.0

Referring back to Model A, some account may be taken of the actual variation of u with radius. Table IX. gives experimental values of $u.D$ along the experimental line. Comparison with the corresponding values for u constant (column 3) shows the energy differences involved. On multiplying the element of column 2 by $2\pi r$ and integrating, the result comes out as 85 per cent. of $\rho K^2/8\pi$, the speeding up of the stream at the centre somewhat decreasing the loss. The energy loss is not confined to the core in the actual case. If the

* I regret that the paper dealing with this question is not yet published.

external loss is assumed symmetrical and as given along the experimental line, the whole works out to 0.0037 for a single vortex, including the core, or more than twice the value given by (11). It appears reasonable to estimate that about 10 per cent. of the whole drag of an aerofoil at usual angles of incidence is directly due to the flow in and around the cores of the vortex pair.

It is hoped that the work described has shown that the wing tip vortices are amenable to accurate investigation. Only the fringe of the subject has been touched and further experiments, particularly on the largest scale feasible, should be productive of results of importance in relation to aerofoil theory. The relation of the present results to Prandtl's theory is many-sided and has scarcely been approached from any direction, efforts being rather directed to sketching the observed motion as clearly as possible. A single aspect forms the subject of a separate note.

There is an evident practical advantage of some importance in ridding a wing of cored vortices, where this may be effected without loss of lift. While, once formed, their strength is intimately connected with lift, the details of the relationship are obscure, and, as may be gathered from some remarks at the beginning of this paper, recent attempts to obviate them have met with success. Difficulties may be encountered, however, in carrying out this policy. It would be surprising if cored vortices were not common with full-scale aircraft. Their general presence recognised, the knowledge should be of utility. On the one hand they may have an occasional effect on design and construction,* and in applied channel testing, on the other an effort might well be made towards recovering a proportion of the power wasted.

The author has pleasure in acknowledging the financial provision of the Department of Scientific and Industrial Research, which allowed more time to be spent in completing the work and preparing the account of it than would otherwise have been possible. He has also had the great advantage of being able to consult with Prof. L. Bairstow.

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* The writer has come across an example of this in practice.

NOTE ON THE EXPERIMENTAL ASPECT OF ONE OF THE ASSUMPTIONS OF PRANDTL'S AEROFOIL THEORY

BY N. A. V. PIERCY, D.SC.

Mr. Glauert's paper "Theoretical Relationships for the Lift and Drag of an Aerofoil Structure" strikes a bold note in asking us to discuss the fundamental principles of Prandtl's aerofoil theory rather than its detailed application. His suggestion is a particularly good one, however difficult the task. Justification of the general truth of the suggestions brought forward by Lanchester, and so ably developed by Prandtl and his colleagues and others, have frequently been afforded by the application of the theory, and similar support only awaits the seeking. But, to achieve its greatest utility, the theory should enable us to extrapolate experimental knowledge with confidence; an example is provided by the question as to what limit, if any, exists to the camber of wings on economic grounds and apart from burble considerations. An approximate tally—rough, in some cases—of the theory with observation over a limited range is inadequate to establish it sufficiently for such a purpose. The theory may indicate with truth the existence of, say, a limit to a certain development, but its numerical calculation of the limit may yet be widely wrong.

The examination of the fundamental assumptions of the theory is a first step towards the inclusion of terms, if this be necessary and feasible, which would make the theory account accurately for well-established observation, and towards framing such terms on a rational basis so that generality may not be lost. It may be that this process would yield results more quickly than the preferable though more difficult onslaught of the viscous equations.

The present note is confined to the assumption whereby vorticity in a strictly limited region is in effect substituted for part of the general action of viscosity. The assumption is especially interesting, for in certain circumstances the form of a vortex in a real fluid is very similar to the familiar theoretical form in one devoid of viscosity. In a recent number of the Journal, Dr. Levy worked out for us a fascinating example of this. The vortex pair formed quickly by some aerofoils provides another example. The success of such work depends on knowledge of the situation or distribution of the vorticity. Prandtl's assumption in this connection is that the vortices are confined to a thin sheath containing the aerofoil and uniting at the trailing edge into a trailing vortex sheet which remains thin behind most of the span, but rolls up into the trailing vortex pair. That part of the action of viscosity which the vortex system replaces is also therefore confined to a thin sheet. I have stated the assumption in this way because, in expressions given for the drag of an aerofoil, the theory provides a term for a certain "profile" drag which is not susceptible to calculation by vortex methods. In Mr. Glauert's paper, this term is attributed entirely to viscous forces. It is not to be associated with skin friction, for it is so large as to include a considerable part of the normal pressure integration. The notion that the theory deals with only a part of the system of flow is important and needs emphasis.

The presence of an additional viscous system, which in the usual type of aerofoil is of major importance so far as the drag is concerned, places a difficulty in the way of examining the fundamentals of the theory in an experimental light. With this reservation, however, it may readily be shown firstly that a limited region indeed exists where viscosity produces striking effects, but that this sheath is not of the evanescent thickness contemplated, and in fact is better described as a "bag." Secondly, it appears that viscosity also leaves, though with a

lighter hand, a widespread stamp on the more distant motion. Experimental evidence is brought forward in support of these statements and may be examined below.

In view of the far-reaching effects of viscosity that we actually find, the feasibility of dividing the flow into independent viscous and vortical components, and not concerning ourselves with details of the former, may be questioned. Consider for instance what takes place near the critical angle. It has recently been shown that the strength of the trailing vortex pair of some aerofoils attains a maximum value at a substantially greater angle than that of maximum lift. The burbling at the central part of the aerofoil associated with loss of lift is apparently to be traced to viscous action since it finds no place in the vortex theory; in fact, the vortices appear to be prepared to continue growth with incidence at the angle at which failure first occurs. We may seek an explanation in several ways. We may infer that at this angle the two wing-tip vortices are not joined together by transverse members along the aerofoil—in which case it is not necessary for them to be so joined at a smaller angle—or that they may be joined in such a manner as not to give cyclic lift. As a third alternative, we may suppose that cyclic lift may be destroyed to a considerable extent by viscous effects, when it seems reasonable to conclude that cyclic lift is not immune from viscosity at smaller angles.

To such an argument the objection may be raised—it has been raised in the past—that the cyclic flow theory has nothing to do with an aerofoil at an angle of burble. But that is beside the point. The question is whether we can afford to neglect at 8deg. incidence, say, a factor so powerful as to be able to overthrow the vortex system at, say, 16deg. Should we not rather conclude that at any angle viscosity is playing an essential and important rôle in the whole system of flow?

With these introductory remarks I invite the attention of the Society to some experimental information relating to the nature of the flow over the upper surface of an aerofoil near its centre of span. Some measurements in three dimensions of the size and shape of the special viscous "bag" are added. Details of the motion within the bag have been studied, and it is hoped to describe these shortly in a separate paper.

On the Motion Above the Aerofoil in the Symmetrical Plane

The aerofoil A had 14 per cent. and 6 per cent. camber for the upper and under surfaces respectively. It measured 1.5in. chord and 13.4in. from tip to tip, and had long graded wing tips. Set at $\alpha = 9.5°$ in a stream of $V = 41$ft./sec. in a 4ft. channel, it was not quite at, though very near to, its critical angle. The object in employing so large an angle was to obtain measurements inside the specially viscous layer. The observations recorded in Table I. (a, b) were obtained along two lines drawn upwards, parallel to the lift, at distances of about one-third and two-third chords from the leading edge. Fine pitot and static tubes were arranged at short equal distances on either side of the centre of span. These were oriented so as to lie approximately along the streamlines, and the relative distance from the model was varied by traversing the latter. In the table the first column gives the distance of the centres of the tubes from the local surface (z); the second, the loss of pitot head (D); the third, the reduction of static pressure; the fourth, the variation of velocity. Some of the observations are plotted in Fig. 1. The points surrounded by broken circles for $z = 0$ were found by assuming that on the surface the velocity was zero and the static pressure the same as at a short distance away. The extension of the curves in this way to $z = 0$ seems reasonably consistent with the readings.

TABLE I.

FLOW OVER UPPER SURFACE OF AEROFOIL A.
(Chord = 1.5 in., Speed = 41 ft./sec.)
(*a*) 0.33 *chord behind leading edge.*

Distance above local surface. in.	chords.	Pitot loss. $\div \rho V^2$	Pressure drop. $\div \rho V^2$	Velocity factor.	Energy flow. $\div \rho V^3$
0.07	0.047	0.008	0.468	1.385	—
0.15	0.10	0.008	0.442	1.365	—
0.20	0.133	0.008	0.433	1.36	—
0.32	0.213	0.008	0.370	1.315	—
0.37	0.247	0.008	0.327	1.28	—
0.53	0.353	0.006	0.261	1.23	—
0.85	0.567	0.008	0.180	1.16	—
1.05	0.70	0.008	0.148	1.13	—
1.20	0.80	0.006	0.134	1.12	—
1.70	1.133	0.006	0.093	1.085	—
2.2	1.467	0.006	0.072	1.065	—
2.7	1.80	0.006	0.055	1.048	—
3.7	2.47	0.003	0.033	1.030	—
7.0	4.67	0.006	0.014	1.008	—
		(*b*) 0.67 *chord behind leading edge.*			
0.00	0.00	—	—	—	0.50
0.07	0.047	0.68	0.310	0.51	0.592
0.19	0.127	0.128	0.308	1.165	0.066
0.26	0.173	0.088	0.312	1.205	—0.013
0.43	0.287	0.008	0.275	1.24	—0.110
0.60	0.40	0.004	0.244	1.215	—0.104
0.78	0.52	0.005	0.207	1.185	—0.087
0.95	0.633	0.009	0.176	1.155	—0.067
1.45	0.967	0.006	0.117	1.105	—0.046
1.95	1.30	0.005	0.083	1.075	—0.031
2.45	1.63	0.005	0.066	1.06	—0.025
3.45	2.30	0.005	0.036	1.03	—0.010
4.45	2.97	0.004	0.024	1.02	—0.006
7.0	4.67	0.003	0.011	1.008	—0.001

Several noteworthy features may be immediately noticed. The pitot head shows a small but measurable loss extending probably to seven chords or so from the aerofoil. As the model is approached, this increases only slightly until a point is reached where the head begins to drop at a great rate. At one-third C (chord) from the leading edge, this point is less than 0.05 C from the surface, but farther back it is about 0.2 C. The point lies on a fluid surface enveloping the aerofoil and (as will be shown) trailing behind it, which may easily be mapped out owing to the sharpness with which the drop begins.* It is proposed to call this fluid surface the "Pitot boundary," with the caution that it is not a boundary in the hydrodynamical sense, since fluid crosses it. Outside it, Bernoulli's theorem closely, though not exactly, applies; inside it, the practically constant pressure in the z direction shows the loss of energy in approximately that direction to be almost purely kinetic over the range of the observations. It would be sufficiently close for practical purposes to calculate the maximum velocity over a

* Mr. Harris Booth first suggested to me the existence and importance of the Pitot boundary. He also added the note that is given above as a caution and which differentiates it from, say, the slip-stream boundary used in airscrew theory.

position on the contour of a wing by applying Bernoulli to the normal pressure. Alternatively, a fair idea of the normal pressure at a position on the after-part of the upper surface of a wing could be obtained without drilling and coring a model by the more ready expedient of laying a fine static tube on the surface.

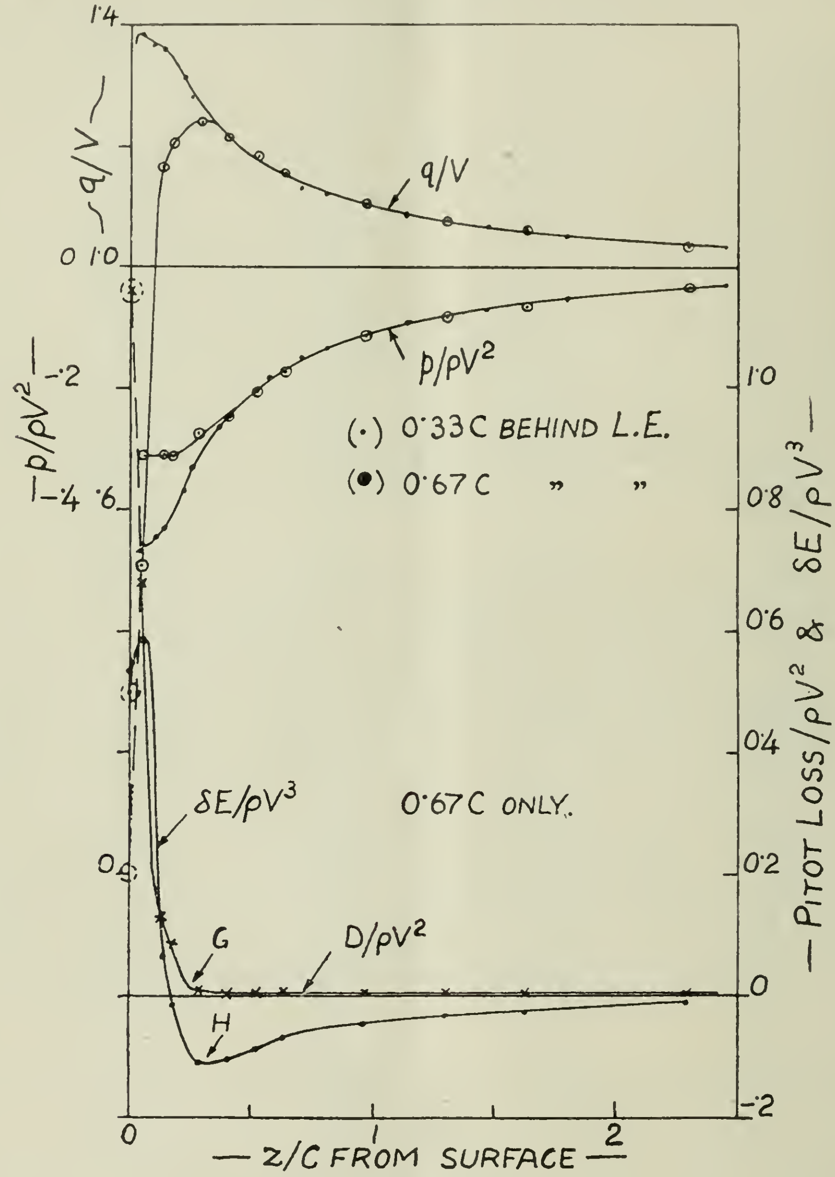

Fig. 1.

Flow above centre of span, Model A.

The pitot head gives the whole mechanical energy of the stream provided we neglect the possibility of an increment due to finely-divided rotations. The loss of head at distant points, where the rotation must be small, is a sufficient indication that in the velocity rearrangements caused by the wing, some of the mechanical energy is converted into heat before the experimental line is crossed. This loss must be traced to viscosity, and is seen to be widespread.

The change in the flow of energy across the line Cz is, of course, not given by the pitot head; but, if we call it E, by the expression :—

$$E = \int \{ lq \cdot D + \tfrac{1}{2}\rho V^2 (lq - V) \} \, dz \quad . \quad . \quad . \quad (1)$$

where lq is the x component of the changed velocity, initially V. Values of δE are given in the last column of Table I. (b), from which it will be noticed that the energy-flow loss increases from 0.5 ρV^3 on the surface (assuming the velocity

there to be zero) to 0.6 ρV^3 at a little distance from it, whence it decreases to a considerable negative value which only again approximates to zero about 5C away. The initial increase of δE *may* be associated with the production of spin, making the expression (1) incomplete. The value of E within the experimental limits is negative, the increase of fluid crossing the line per unit time being out of all proportion to the pitot drop. This of course is explained by the slowing up of the stream beneath the model as demanded by cyclic theory. The figures just considered are also plotted in Fig. 1 and illustrate that the cusp value of the local energy-flow loss, occurring at the point H, is displaced from the trace of the pitot boundary G; δE becomes negative well within the latter. It is important to recognise* that (1) may only be applied to investigate local changes in energy-flow produced by a wing, as will be seen when details of the motion within the bag come to be considered.

The velocity curves (and hence approximately the static pressure curves) practically overlie at a distance from the aerofoil. The extent to which this is true may be gauged from Table II. (a, b), where the observed increments of velocity, $q - V$, are in each case compared with values calculated from the single formula: $q - V = 6.1/(z + 0.233) - 1$, where z is in chords. The thickening of the viscous layer appears to cut short the building up of the velocity above the aerofoil, and only that pressure drop which has been generated up to this point of intervention is transmitted to the aerofoil surface to assist in lift. If the viscous layer were of uniform thickness, the pressure at both points of the surface would be about the same.†

TABLE II.

VELOCITY OVER AEROFOIL A.

(Details as in Table I.)

(a) 0.33 C *behind L.E.*			(b) 0.67 C *behind L.E.*		
z.	(ft./sec.)		z.	(ft./sec.)	
(in.)	Observed.	From Formula.	(in.)	Observed.	From Formula.
0.07	15.8	20.8			
0.15	15.0	17.4			
0.20	14.8	15.7			
0.32	12.9	12.7			
0.37	11.5	11.7	0.43	9.9	10.7
0.53	9.4	9.4	0.60	8.8	8.6
0.85	6.6	6.6	0.78	7.6	7.1
1.05	5.3	5.5	0.95	6.3	6.0
1.20	4.9	4.9	1.45	4.3	4.1
1.70	3.4	3.5	1.95	3.1	3.0
2.2	2.7	2.6	2.45	2.5	2.3
2.7	2.0	2.0	3.45	1.2	1.4
3.7	1.2	1.25	4.45	0.8	0.9
7.0	0.3	0.25	7.0	0.3	0.25

It may be noticed from the nature of the formula given above that if we assumed a redistribution of speed in the channel following the introduction of the model, such that, apart from the cyclic flow variation, the velocity was increased everywhere along the experimental line by approximately 2 per cent. of the undisturbed speed, the speed points would lie on a hyperbola. It may be

* I regret that the paper dealing with this question is not yet published.

† It is therefore a little difficult to see that the pressure diagram along the chord is determined by the methods developed by Joukowski, and is not rather controlled by viscosity.

as well, therefore, to note that certain experimental errors were carefully avoided. Four gauges were used—one connected the exploring pitot with a distant pitot, another the exploring static with a distant static, a third the exploring pitot and static, while the fourth was used for the general speed. A certain degree of checking was therefore possible and was satisfactory, and further, the data for the free stream at the one point where all the measurements were taken were obtained by removing the model from the channel.

In general, the speed formula :—

$$(q - V)/V = (S/\sqrt{[z^2 + S^2]})\{A/(z + B) - C\} \qquad . \quad . \quad (3)$$

where S is a fraction of the half-span, and A, B, C are constants for a particular aerofoil at one angle and scale, has been found to fit fairly several cases that have been tested. It is proposed to consider these next, and they will illustrate the use of the first factor of the R.H.S. and show that some such factor is necessary. It may immediately be seen, however, that the form chosen for the factor is consistent with vortex-wing theory. The factor also makes the velocity increment vanish at infinity.

Model B was a rectangular aerofoil, 2in. chord, aspect ratio = 6, of the section shown in Fig. 2. Model C had the same section and chord as B, but

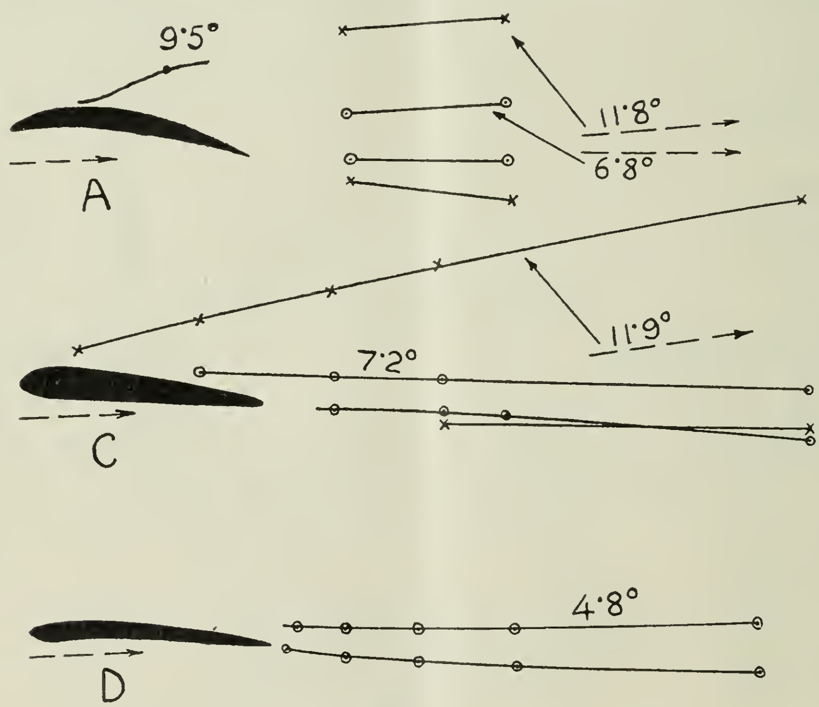

Fig. 2.

Pitot boundaries of three aerofoils behind centre of span.

the plan form shown in Fig. 3. Model D was a 3in. x 18in. rectangular aerofoil of R.A.F.15 section. All were tested in a 4ft. channel at an angle of incidence of about 4.8deg. and a general speed of 41ft./sec. Measurements of velocity were made along the line Oz in the symmetrical plane drawn vertically upwards from the uppermost point of the contour of the model as set in the channel. By this means the best effort possible was made to obtain the velocity distribution along a line orthogonal to the streamlines. The measurements are collected in Table III.

Taking S as half the span in the case of the rectangular aerofoils, the factor inside the brackets of (2) (xV) comes out for Model B as :—$3.9/(z+0.24) - 1.0$, z being in chords. The "fit" of the formula is indicated with slide-rule accuracy below, the observations being labelled (M) and the calculations (F).

TABLE III.

Velocity Distribution above Three Aerofoils at 4.8°.

(V = 41 ft./sec.)

z/C	q/V Model B.	Model C.	Model D.
0.01	1.01	—	1.015
0.015	1.06	—	—
0.02	1.32	1.32	—
0.025	1.33	—	—
0.03	1.325	—	—
0.04	1.32	—	—
0.05	1.31	—	—
0.06	1.295	1.30	1.295
0.11	1.25	1.26	1.25
0.16	1.215	1.215	1.215
0.21	1.19	1.18	1.185
0.26	1.16	1.15	1.16
0.38	1.13	1.10	1.125
0.51	1.095	1.082	1.09
0.76	1.062	—	—
1.51	1.027	1.015	1.030
2.51	1.012	1.004	1.014

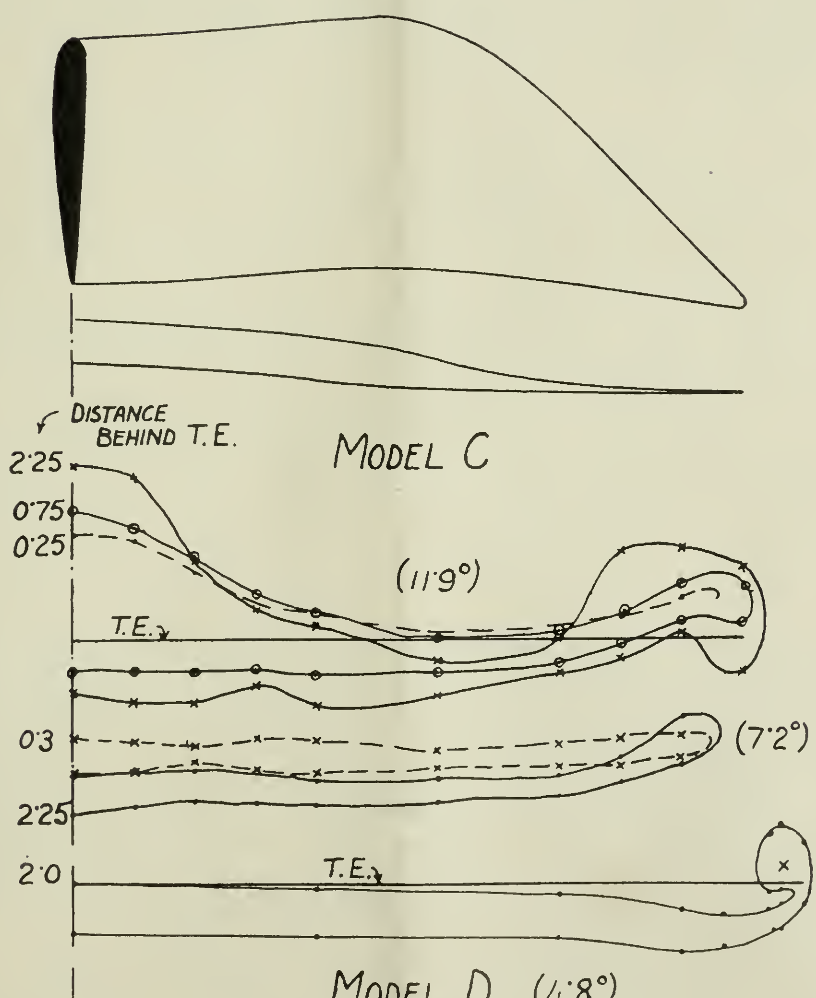

FIG. 3.

Sections through pitot boundaries of two aerofoils.

MODEL B. VELOCITY INCREMENTS IN FT./SEC.

z/C ...	0.02	0.025	0.03	0.04	0.05	0.06	0.11	0.16	0.21	0.26	0.38	0.51	0.76	1.51	2.51
$q-V$ (M)	13.0	13.5	13.4	13.1	12.8	12.1	10.3	8.9	7.7	6.6	5.2	4.0	2.6	1.1	0.5
$q-V$ (F)	14.0	13.7	13.4	12.9	12.4	12.0	10.1	8.7	7.7	6.8	5.3	4.1	2.8	1.1	0.3

It will be noticed that the formula is successful quite close to the aerofoil. If we took a shorter length for S, as would appear to be suggested by Prandtl's theory, the calculation for $z=2.51$ would be thrown somewhat further out, but the difference would not be great. The constants chosen for the formula are open to a certain variation depending on which part of the curve it is desired to fit most closely, but the value for the constant B requires to lie between 0.2 and 0.24, and the larger value appears to have advantages.

Proceeding to consider Model C, an essential difference is observable following the inclusion of wing tips in the span; the additive velocity—scarcely changed near the aerofoil—decreases more rapidly with distance. This illustrates the necessity for the first factor of (3). Taking S as the distance to the shoulder of the wing tip, or to the end of the central portion of constant section ($1.5C$), a change of the factor in the brackets (xV) to $2.9/(z+0.15)-0.8$ reflects the alteration. The substantially lower value for the constant B suggests that well-designed wing tips may enable a given pressure drop on the surface to be built up with less effect on the flow at a large distance, or that with the same effect at a distance a greater pressure drop will result at the aerofoil. The measurements are again compared with the values calculable and agreement is seen to be fair until the two points nearest to the aerofoil are reached.

MODEL C. VELOCITY INCREMENTS IN FT./SEC.

z/C	0.02	0.06	0.11	0.16	0.21	0.26	0.38	0.51	1.51	2.51
$q-V$ (M) ...	13.2	12.2	10.6	8.9	7.4	6.1	4.1	3.4	0.6	0.15
$q-V$ (F) ...	16.2	13.0	10.3	8.6	7.3	6.3	4.6	3.4	0.7	0.15

Finally, $3.3/(z+0.2)-0.6$ seems best to suit the results for Model D.

MODEL D. VELOCITY INCREMENTS IN FT./SEC.

z/C	0.06	0.11	0.16	0.21	0.26	0.38	0.51	1.51	2.51
$q-V$ (M) ...	12.1	10.2	8.9	7.5	6.5	5.1	3.7	1.2	0.6
$q-V$ (F) ...	12.1	10.0	8.6	7.5	6.5	5.1	4.0	1.2	0.5

Comparing all the results that have been noticed, it appears that very little further work should suffice to build up experimentally a reliable formula for the speed variation above the centre of span of aerofoils. Assuming the form of (3) to be sufficient, A would evidently best be a function of the lift coefficient or angle of incidence, while B and C might be arranged to take some account of scale and plan form. The presence of the constant C is probably consistent with the assumption of cyclic flow due to a distribution of vortices along the chord. The testing of Prandtl's theory by means of the first factor of the R.H.S. of (3) is, as will be seen, too difficult at low speeds since the factor only takes effect at considerable distances where the added velocity is small.

In conclusion of this discussion, the speed curves for three positions along the chord of an aerofoil similar to Model C and at the same angle are given in Table IV. The well-established curve over the leading edge and the high value of the maximum velocity measured there will be noticed, but it is to be borne in

mind that these curves do not give the variation along lines which cut the stream-lines at right angles.

TABLE IV.

Velocity (ft./sec.) above aerofoil similar to Model C. $\alpha = 4.8°$.

z/C (above L.E.).	$(q/V)^2$ Above L.E.	Above Mid Chord.	Above T.E.
0.1	1.88	1.32	1.05
0.25	1.355	1.265	1.08
0.4	1.235	1.21	1.05
0.55	1.18	1.17	—
0.7	1.16	1.145	1.05
0.95	1.095	1.11	1.07
1.2	1.07	1.085	1.065
1.7	1.05	1.06	1.045
2.2	1.032	1.048	1.035
2.7	1.030	1.040	1.028

TABLE V.

TRACES OF PITOT BOUNDARY ON SYMMETRICAL PLANE.

Model A ($1\frac{1}{2}$in. chord) and Model D (3in. chord). $V = 40$ ft./sec.

(See notes to Table VI.)

Angle =		6.8°				11.8°			
		Distance behind L.E. in chords.							
Aerofoil.	yC	1.4		2.07		1.4		2.07	
		z	z^1	z	z^1	z	z^1	z	z^1
A	0	5.8	0.1	6.1	1.5	17.5	3.5	18.3	7.2
		0.05		0.3		0.6		1.0	
D	0	5.7	1.5	4.5	4.5	2.8	6.9	1.5	10.3

On the Form of the Pitot Boundary Behind the Trailing Edge

Going now beyond the trailing edge, we may notice from Fig. 2 the distance apart of the traces of the pitot boundary on the symmetrical plane for the three aerofoils *A*, *C* and *D* at a number of angles. The depth of the "bag"—or the thickness of the specially viscous layer, if we prefer to call it so—increases with angle and distance aft. The actual measurements for these sketches may be found in Tables V., VI. and VII. Model A stalled at a somewhat greater angle

TABLE VI.

SECTIONS THROUGH PITOT BOUNDARY, MODEL C (2IN. CHORD, 11IN. SPAN).

(V = 40 ft./sec.)

z = distance of upper trace above local level of trailing edge in m/m.

z' = ,, lower ,, below ,, ,, ,, ,,

a means distance less than about 0.3 m/m., — means no measurement attempted.

Angle =	2.8°			7.2°								11.9°						
Distance behind L.E. (in chords).	0.75	1.3		0.25	0.75	1.3		1.75		3.25		0.25	0.75	1.3	1.75		3.25	
y/C from centre-line of span.	z	z	z'	z	z	z	z'	z	z'	z	z'	z	z	z	z	z'	z	z'
0	1.0	0	3.7	1.7	3.5	5.3	1.7	2.9	2.9	—2.4	13.0	3.3	14.1	22.8	27.4	6.0	36.8	10.2
0.25	1.3	—.4	3.9	—	5.6	4.5	1.2	3.1	2.1	—0.9	11.5	2.5	11.5	21.0	23.8	6.0	34.3	12.5
0.5	1.6	—.6	2.6	2.6	4.3	3.3	—0.3	3.5	1.3	—2.7	10.2	3.5	9.4	14.6	17.8	6.5	17.0	12.6
0.75	1.5	—.2	3.2	—	6.2	5.3	1.2	4.3	1.7	—1.0	10.7	3.3	8.1	7.8	9.8	5.2	6.6	9.0
1.0	1.3	.3	4.1	3.5	5.6	4.5	1.6	3.3	1.8	—3.3	11.5	2.8	5.8	6.0	6.8	7.2	3.3	13.4
1.5	0.3	.9	3.3	a	3.1	2.8	0.1	2.1	0.7	—2.7	7.9	3.3	3.3	1.8	0.8	5.8	—4.5	10.7
2.0	a	1.3	0.2	a	5.8	4.3	0.1	2.0	0.2	—2.2	6.9	1.8	3.8	3.3	2.2	4.4	—0.4	5.9
2.25	a	1.4	—1.0	—	4.0	5.3	0.2	4.8	0.2	1.6	3.7	—	—	—	—	—	18.6	3.5
2.5	a	2.5	—2.4	a	2.5	5.9	—1.3	8.0	—0.5	10.1	0.0	a	0.6	9.5	12.3	—0.1	19.3	—2.1
2.75	—	a	a	—	a	a	a	a	a	shallow		—	—	—	8.0	—4.7	15.3	6.1
3.0	—	—	—	—	—	—	—	—	—	—	—	a	a	a	a	a	—	—

than that at which it was set for the two readings in front of the trailing edge, but the presence of the pitot tubes may have hastened the critical angle. Looking at the curves, however, it may be estimated that the pitot boundary will recede, say, 0.25 *C* from the trailing edge before the critical angle is reached. Model C stalled at between 10deg. and 11deg.; the lift curve was afterwards very flat, the lift at 12deg. being 0.94 of the maximum. It will be seen that the specially viscous layer can only be regarded as very thin over the under surface and the front part of the upper surface.

Measurements for outside the symmetrical plane with Model C are included in Table VI., and some of these are plotted in Fig. 3. Downwash experiments showed that this model was devoid of cored vortices at flying angles, and there is an interest in comparing its boundary at 7.2deg. with that of an aerofoil where vortices were present. This may be done by inspecting the lower part of the figure, where a section through the viscous layer of the rectangular aerofoil of R.A.F.15 section at 4.8deg. is shown (Table VII.) recording the measurements. The circular region near the wing tip of the latter aerofoil encloses very marked pitot loss; the region is nearly isolated from the rest of the layer, for the link connecting the two is a shallow depression. The position of the centre of the vortex core with the circular region is marked with a cross. This feature is absent from the former model, and 10 per cent. or so of drag is thereby directly saved. The substantially thinner viscous layer behind *C*, except at the centre of span, will also be noticed.

TABLE VII.

Section through Pitot Boundary of Model D at 4.8°, 2C behind T.E.

(3in. chord, $V = 40$ ft./sec.)

(See notes to Table VI.)

$y/C=$	0.0	1.0	2.0	2.5	2.66	2.86	2.9	3.0	3.15
$z=$	0.0	—1.5	—3.0	—7.6	—8.3	$\left.\begin{matrix}+16.0\\-7.6\end{matrix}\right\}$	$\left.\begin{matrix}+19.0\\-6.5\end{matrix}\right\}$	+13.0	—
$z^1=$	15.3	16.5	16.8	21.3	19.1	$\left.\begin{matrix}2.3\\15.2\end{matrix}\right\}$	$\left.\begin{matrix}1.5\\14.0\end{matrix}\right\}$	6.5	—

The change of the boundary of *C* behind the extremity of span caused by the stall shows the beginning of the development of a vortex. The layer has also thickened, though not unduly so, behind the shoulder of the wing tip, but the effect increases rapidly towards the centre of span and may be associated with the early loss of lift there.

I conclude by repeating that such measurements as the present make a case for the remark that viscosity as such is of fundamental importance in determining the system of flow.

I have great pleasure in mentioning that most of the readings to which reference has been made were obtained initially for the Commercial Aeroplane Wing Syndicate, and in acknowledging their permission for publication. The Department of Scientific and Industrial Research provided for the completion of the work.

THEORETICAL RELATIONSHIPS FOR THE LIFT AND DRAG OF AN AEROFOIL STRUCTURE

BY H. GLAUERT, M.A., A.F.R.AE.S.

1 *Introduction*

It is a fact of common experience that a body in motion relative to a gas or liquid is subject to a resultant force, and it is customary to resolve this force into two components, the drag opposing the relative motion and the lift at right-angles to the direction of this motion. In general the drag is the predominant component, but the class of bodies known as aerofoils and used for the construction of aeroplane wings, is such that the lift is considerably in excess of the drag. The present discussion relates solely to this class of bodies whose essential characteristic is the production of a large lift correlated with a relatively small drag. It is a matter of very considerable importance to develop a theory which will explain the origin of the forces experienced by an aerofoil, and will provide a method of calculating the characteristics of any aerofoil structure from a knowledge of its geometrical form and of the physical properties of the fluid through which it moves. It is proposed to discuss the behaviour of aerofoils in two and in three dimensions, and in particular to discuss the vortex theory of lift and induced drag. The general nature of the flow pattern on which the theory is based has been described by Lanchester, but the mathematical development of the theory is due to Prandtl and his colleagues.

The solution of a physical problem can be analysed into three parts, each of which involves its own special difficulties. In the first place it is necessary to formulate the physical assumptions as to the nature of the bodies and fluid involved in the problem. It is necessary to come to a decision as to the legitimacy of neglecting gravitational and other effects, of regarding the bodies as rigid and the fluid as incompressible and non-viscous. It is only on the rarest occasion that it is deemed necessary to retain the full complexity of the physical nature of the bodies and fluid, but the value of the solution obtained will always depend on the validity of the initial physical assumptions. The second part of the solution consists of the mathematical statement of the problem, usually in the form of differential equations. This statement will be a full and accurate representation of the physical assumptions and will comprise both the equations of motion and the initial and boundary conditions. In a certain sense the problem can now be regarded as solved, since the third step consists only of the mathematical manipulation of the equations into a form more suitable for deducing numerical results which may be compared with experience or used in design. This third step, however, often presents almost insuperable difficulties, and it may become necessary to confine our attention to certain special types of problem, to restrict the motion to small deviations from a known motion, or to simplify the original physical assumptions by neglecting those terms which the mathematical analysis indicates to be of small importance. The value of the final solution decreases with the simplification of the physical assumptions, but it is important to remember that in no case are these assumptions absolutely rigid, and that in all cases the extent of justifiable approximation depends on the accuracy of the results deduced when compared with experience.

2 *Aerodynamic Problems*

Progress in solving aerodynamic problems has been delayed by the difficulty of formulating simple but accurate assumptions as to the nature of the fluid. The

old corpuscular theory was a failure and the kinetic theory of gases is too complex. Classical hydrodynamics assumes a continuous compressible viscous medium, but owing to the difficulties of mathematical treatment, the majority of the solutions refer to a perfect fluid which possesses neither viscosity nor compressibility. In addition the motion is usually assumed to be irrotational, by reason of the established theorem that rotation or vorticity can be neither created nor destroyed by a conservative system of forces.

The solutions for the steady irrotational motion of a perfect fluid fall into three classes. In the first place solutions have been obtained for the steady acyclic flow past different bodies, but it was at once evident that the initial assumptions had destroyed all reality in the final solutions. The analysis showed that the body would experience no resultant force, and in consequence the theory failed to account for the observed phenomena. An attempt to overcome this difficulty was made on the lines suggested by Helmholtz, involving the existence of surfaces of discontinuity of velocity springing from the surface of the body. In this way a resultant force is obtained on the body, but the agreement with experimental values is poor. Further, it has been shown that these surfaces of discontinuity are unstable and break up into vortices. This method, however, does suggest an analogy with the actual behaviour of blunt bodies which shed a series of vortices. The third type of solution is that of cyclic flow, when there is a circulation round the body. The theory of a perfect fluid suggests no method by which this circulation might arise, but if the circulation is present the analysis leads to the conclusion that in two-dimensional motion there is a resultant force acting on the body at right angles to the direction of motion, and of magnitude

$$L = \rho V K l$$

where ρ is the density of the fluid, V the relative velocity, K the circulation, and l the length of the body. Apart from the criticisms already mentioned, all these solutions suffer from the defect that the fluid has a finite tangential velocity at the surface of the body, whereas experiments have shown that in all cases the fluid immediately adjacent to the surface has no velocity relative to the body. A further criticism can be made in the case of bodies which have sharp edges or regions of small radius of curvature. At such a point the theories indicate excessively high velocities and very low pressures. On account of the low pressure the fluid can no longer be regarded as incompressible, and on account of the high velocity gradient viscous forces become relatively more important, and it is certain that even if the theory were found to be satisfactory in general, it would require modification near these particular regions.

In view of this discussion it appears that no satisfactory solution of an aerodynamic problem is to be expected when the effects of compressibility and viscosity are neglected, and it becomes necessary to consider the effect of these two factors. Assuming the general mass of the fluid to be at rest, compressibility only becomes of importance when the velocity of the fluid in any small region approaches to the velocity of sound. In the case of projectiles or high-speed airscrews, the velocity of the body is such that compressibility effects are of prime importance, but it is proposed to limit the present discussion to moderate velocities for which the compressibility of the fluid can be ignored. There remains, however, the possibility that the velocity of the fluid near a sharp edge of the body may reach so high a value as to introduce compressibility effects.

Turning next to the question of viscosity, there is the experimental fact that there is no slip at the surface of the body. Also as the viscous force is proportional to the rate of change of velocity, it is evident that the viscosity will be of importance in the immediate neighbourhood of the body, but may be negligible at large distances from it. The equations for viscous flow are known, but present considerable difficulties of solution, and the question arises to what extent approxi-

mation to these equations is legitimate. It is known that the solution obtained by ignoring the viscosity is unsatisfactory, but it is by no means obvious that the limiting solution obtained as the viscosity tends to zero is the same as the solution for zero viscosity. In particular, in the case of a body with a sharp edge, there is a region where the velocity gradient tends to infinity, and where the viscous forces will be of the same order as the dynamic forces, however small the viscosity. On the other hand, the layer round the body in which viscosity is of importance can be conceived as of zero thickness in the limit, and this conception is equivalent to allowing slip on the surface of the body. It appears, therefore, that the non-viscous equations will be the same as the limit of the viscous equations, except in the region of sharp edges.

One more general point is worthy of attention. The solutions considered above have all been assumed to be steady motions, but it is by no means certain that the solution of any aerodynamic problem is ever a steady motion. On general grounds it seems certain that a steady motion solution always exists in theory, but this solution will correspond to actual conditions only if it be a stable motion. In the case of the flow past a cylinder, the flow is known to be periodic, and the same is true for the flow past an aerofoil at large angles of incidence. It is possible that the flow past an aerofoil is always periodic, but that at small angles of incidence the amplitude of the oscillations may be negligibly small. As a further example the case of the flow along a flat plate may be mentioned, where the steady viscous flow is known but applies only to very small velocities, the flow becoming periodic for higher velocities. Even when a steady flow is obtained, it appears that the possibility of periodic flow may exert an important influence in deciding which of several possible steady motions will actually occur.

3 *Cyclic Flow*

Prandtl's aerofoil theory is based on the belief that a cyclic motion is essential for the production of the lift of an aerofoil, and it is necessary, therefore, to examine the evidence for the existence of this circulation, the method of its origin and the relationship of the solution to the complete equations of viscous flow.

If an aerofoil experiences a lift force the average pressure above the aerofoil must be lower than that below it, and if we consider points at some distance from the wing where the effects of viscosity are negligible, this implies that the average velocity is higher above the aerofoil than below it, and so indicates a circulation round the wing. Alternatively by considering the pressure distribution across the span of a wing, we can see that the streamlines must be converging above the wing and diverging below it, leading to the conclusion that a sheet of velocity discontinuity or vorticity trails behind the wing, and it is evident from fundamental principles that this vortex system must be completed by circulation round the wing. This conclusion is confirmed by all the experimental results which are available. The wing tip vortices have been observed on several occasions, and several comparisons of the flow pattern round aerofoil structures have shown good agreement between the observed flow and that calculated from Prandtl's theory. In fact the association of the lift of an aerofoil with cyclic motion can be regarded as definitely established.

The method in which this cyclic motion originates is somewhat obscure in detail, but the general lines of its development are not difficult to visualise. The irrotational acyclic flow of a perfect fluid past an aerofoil involves the existence of a stagnation point on the upper surface near the trailing edge, and the occurrence of high velocities near this relatively sharp edge. This solution is clearly at fault, but appears to represent the general type of flow at very low velocities. As the motion proceeds a succession of transverse vortices will be shed from the aerofoil, and this process will continue until conditions are reached which no longer give rise to the formation of vortices. In so far as the theory

of a perfect fluid is valid, this implies that the rear stagnation point must move to the immediate neighbourhood of the trailing edge, and this change in its turn implies that a cyclic motion round the aerofoil has developed, the strength of the circulation being equal in magnitude and opposite in sign to the total strength of the transverse vortices which have passed down stream. It is possible that steady conditions are never reached, and that alternate vortices of opposite sign continue to leave the aerofoil, but this would merely imply the oscillation of the circulation round a mean value. The conditions which give rise to the formation of vortices are not well known, but the experimental fact is confirmed by observations of the flow past many different types of body.

Finally we must consider the relationship of Prandtl's analysis to the complete equations of viscous flow. Prandtl's solution is a first approximation to the accurate equations, and as such it is legitimate to neglect the effects of viscosity except in those regions where the velocity gradient is large, *i.e.*, near the surface of the aerofoil or in the cores of any vortices which may exist. In other regions it is legitimate to use the equations of a perfect fluid. This method receives confirmation from G. I. Taylor's experiments with rotating fluids. The non-viscous solution for the case of a sphere moving along the axis of rotation of a fluid shows no slip at the boundary, and in this case the non-viscous solution agrees well with experiments. Since to the first order the vortices in the fluid can be regarded as line vortices, the region where viscosity retains its importance is confined to a narrow sheath round the aerofoil. Thus the approximations used by Prandtl are equivalent to the use of the equations of a perfect fluid, modified by the condition that vortices may arise at points of the body where the streams passing above and below the body re-unite. The only doubtful point of this approximation appears to be in the assumption that the vortex wake behind the aerofoil can be regarded as of zero thickness, and it is on account of this limitation that the theory can only be applied to small angles of incidence, and gives no account of the stalling of the aerofoil at its critical angle.

The real test of the validity of Prandtl's approximations lies in the comparison of the results deduced from this theory with actual experimental results, and on this basis the approximations appear to be fully justified. The theory has now been tested in a variety of ways, by examination of the flow past aerofoil structures, by comparison of the effect of aspect ratio and of the effect of complex multiplane structure, and in almost every case the agreement between theory and experiment has been far better than would be expected from the nature of the approximations. The application of the theory is, however, limited to aerofoils of good shape at small angles of incidence, and excludes the region in the immediate proximity of the surface of the aerofoil. These limitations, however, which are essential to the nature of the approximations made, do not detract in any way from the validity of the solution in the region to which it applies. It must also be emphasised that Prandtl's theory is based on the association of the lift of an aerofoil with cyclic motion, but does not give any indication of the relationship between the magnitude of this circulation and the geometrical form of an aerofoil. This problem, which has been attacked by Kutta, Joukowski and other writers, is quite distinct from the work of Prandtl, and the validity of Prandtl's theory does not in any way depend on the validity of Joukowski's hypotheses.

4 *The Two-Dimensional Theory*

The flow round an aerofoil in two dimensions is represented by the equations of a perfect fluid on the assumption that the vortex layer surrounding the aerofoil is of evanescent thickness. The assumption will clearly break down if the solution involves excessively large velocity at any point, and in all cases it can only be regarded as an approximation to the actual flow. The only difficulty in

obtaining a solution on these lines is the uncertainty of the value which shall be assigned to the circulation, and no solution of the general case has been obtained. Joukowski has, however, suggested the hypothesis that for the class of aerofoils which have a sharp trailing edge, the circulation will be such that the rear stagnation point is situated on the trailing edge. The justification of this hypothesis is that the solution is consistent with the assumption of a vortex layer of evanescent thickness, while any other position of the stagnation point would violate this assumption. The hypothesis can only be applied to a limited class of aerofoils, but the results obtained are very hopeful. In particular the slope of the lift curve is found to be slightly larger than π per radian in all cases, and this agrees excellently with values deduced from experimental results by applying Prandtl's correction for aspect ratio.

It is not proposed to enter into the method of solution or its many applications in any detail, as the subject is a large one, worthy of separate attention. The method is based on the known cyclic flow round a circle, and by means of a suitable conformal transformation the flow round an aerofoil is obtained, the value of the circulation being chosen in conformity with Joukowski's hypothesis.

The Joukowski series of aerofoils is obtained by the conformal transformation

$$Z_1 = Z + c^2/Z$$

which transforms a circle in the Z plane to an aerofoil in the Z_1 plane. The origin of the circle is arbitrary, but the circle must pass through the point $Z = -c$ and enclose the point $Z = c$. There is therefore a doubly infinite series of Joukowski aerofoils which all have the common characteristic of a cusp at the trailing edge.

An extension of this method is obtained by use of the transformation

$$(Z_1 - nc)/(Z_1 + nc) = (Z - c)^n/(Z + c)^n$$

applied to the same set of circles as the previous transformation. This leads to a class of aerofoils which have a sharp trailing edge, but whose surfaces at this point meet at an angle $(2 - n)\pi$. Thus an aerofoil of conventional type is obtained by choosing n slightly less than 2.

The most general transformation can be written in the form

$$Z_1 = Z + a_1/Z + a_2/Z_2 + \ . \ . \ . \ .$$

where the coefficients $a_1\ a_2\ .\ .\ .$ may be complex. The transformation is applied to a circle which has one of the zeroes of dZ_1/dZ on its boundary and encloses the other zeroes. If a be the radius of the circle and if $a_1 = c^2\ e^{2i\gamma}$, it can be shown that the lift of the aerofoil is

$$L = 4\pi a \rho V^2 \sin(\alpha + \beta)$$

and the moment round the centre of the circle

$$M = 2\pi c^2 \rho V^2 \sin 2(\alpha + \gamma)$$

where α is the angle of incidence and β is the angle between the x axis and the line joining the centre of the circle to the zero of dZ_1/dZ on its boundary.

5 *The Three-Dimensional Problem*

Prandtl has developed the theory of an aerofoil in three dimensions on the lines of the flow patterns suggested by Lanchester and on the basis of a definite association between lift and circulation. The analysis, however, does not claim to establish from first principles the relationship between circulation and geometrical form, but provides a method of deducing the characteristics of one aerofoil structure from those of another structure with the same aerofoil section. The theory is, therefore, quite independent of the accuracy of Joukowski's hypothesis, and is not limited to the class of aerofoils with sharp trailing edges.

In general the solution is a first order approximation only, but there is no reason why the solution should not be carried to a higher order of accuracy at the expense of greater complexity, if this course should be considered necessary or desirable. The excellence of the results obtaind by the first order solution is the justification of the approximations made.

The lift of an aerofoil is associated with a definite circulation, and to complete the system, vortices spring from the trailing edge. To the first order these may be regarded as line vortices in the direction of the general stream. A consideration of the velocity field of the vortex system shows that there is a normal velocity in the region of the aerofoil by virtue of which the effective angle of incidence is reduced and the resultant force has a drag component, to which the name of induced drag has been given. The mathematical statement of these effects presents no difficulties, but the detailed analysis is rather complex except in a few special cases.

It has been shown that the minimum induced drag is obtained when the normal induced velocity due to the trailing vortex system has the same value at all the lifting elements. In the case of a single monoplane aerofoil this will occur when the lift is distributed elliptically across the span of the wing. The formulæ obtained then are

$$\alpha = \alpha_0 + Sk_L/2\pi s^2$$
$$k_D = k_{D0} + Sk_L{}^2/2\pi s^2.$$

In these equations α, k_L, k_D are the charasteristics of the aerofoil of area S and semispan s, while α_0, k_L, k_{D0} are the characteristics of the same aerofoil section in two-dimensional flow. The drag represented by k_{D0} is known as the profile drag, and is due entirely to viscous forces.

These formulæ refer to the condition of minimum induced drag, but also give quite good approximations to all conventional shapes of aerofoil. The case of a rectangular wing has been worked out in detail, and it has been found that the induced drag is about 5 per cent. higher than the value indicated by this minimum formula.

In the case of a multiplane structure, an aerofoil element is subject to induced velocity due to its own trailing vortices, and also to the vortex systems of the other aerofoils. The analysis becomes increasingly difficult, but has been fully justified by comparison with experiments, even in the case of a multiplane structure consisting of five aerofoils at a large negative angle of stagger. The analysis of a multiplane structure is simplified by an interesting theorem as to the effect of stagger. The induced drag of the system can be measured by the kinetic energy of the field of the vortex system far behind the aerofoils, and so it appears that the induced drag will not be affected by moving an aerofoil element in the direction of motion, provided the incidence is adjusted to maintain the same lift on all the aerofoil elements. By this means a staggered structure can be replaced by an equivalent unstaggered structure as regards total induced drag.

6 *Conclusion*

The object of this Paper has been to attempt a discussion of the fundamental principles of the modern aerofoil theory, rather than to give a detailed account of its varied applications or of the many tests which have been applied to test its accuracy. References are, however, given below to the principal reports which deal with these other aspects of the aerofoil problem.

The two-dimensional problem provides an interesting field of work, involving conformal transformation, but has scarcely reached the point of direct practical utility as yet. Further work is required to examine the question of the viscous drag of an aerofoil, and the method of conformal transformation must be developed so as to be capable of dealing with the aerofoil sections actually in use.

The three-dimensional analysis has already provided results of considerable value, and has been repeatedly confirmed by comparison with experiments. It provides a satisfactory method of predicting the characteristics of an aerofoil structure, however complex, from the known characteristics of a monoplane aerofoil of the same section. Further interesting applications have been the interference experienced by an aerofoil tested in a wind channel due to the constraint of the walls, and the development of an airscrew theory on lines suggested by the vortex theory of aerofoils.

References

Lanchester.—Aerodynamics (1907).

Prandtl.—Tragflügeltheorie (Göttingen Nachrichten, 1918 and 1919). Applications of Modern Hydrodynamics to Aeronautics. (National Advisory Committee for Aeronautics, Report No. 116.)

Mises.—Zur Theorie des Tragflächenauftriebs (Zeitschrift fur Flugtechnik u Motorluftschiffahrt, 1917 and 1920).

Trefftz.—Prandtlsche Tragflächen-und Propeller-Theorie (Zeitschrift fur angewandte Mathematik u Mechanik, 1921).

Glauert.—Aerofoil Theory (A.R.C. R. and M. 723). Applications of the Vortex Theory of Aerofoils (A.R.C. R. and M. 752). An Aerodynamic Theory of the Airscrew (A.R.C. R. and M. 786).

CORRESPONDENCE

Audit House,
Victoria Embankment, E.C.4,
17th July, 1923.

To the Editor of the JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY.

SIR,—Professor Orr, of the Royal College of Science, Dublin, has been kind enough to send me some criticism in connection with my paper, "On the Stability of Aero Engines," which appeared in the Journal for April, 1923.

I do not propose to deal yet with the more general criticism, but as a sequel to Professor Orr's remarks the formulæ given in my paper abbreviates to a remarkable degree and the elegant results are free from the reproach of being "complicated mathematical formulæ."

As Professor Orr naively writes, "What labour you might have saved yourself!"

Yours faithfully,
J. MORRIS.

CASE I.

Direct Drive

All throws and intermediate journals equal (Fig. 1).

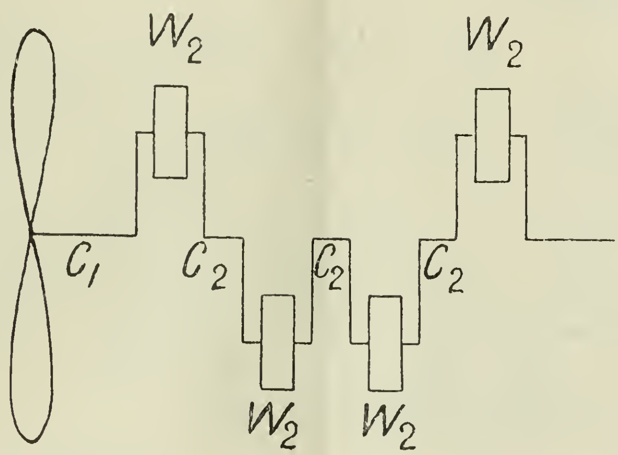

FIG. 1.

Let

I_0 = moment of inertia of one throw (with crankpin but without rotating load) about the crankshaft axis.

W_2 = rotating load at crankpin.

l_2 = the length of a throw from crankshaft centre to crankpin centre.

c_2 = torsional stiffness of a journal (*i.e.*, its CI/L where C is in lbs./in.^{2}, I in ins.^{4} and L in ins.).

The polar moment of inertia of the airscrews is considered to be very large.

Then the frequency of *one throw complete* about a *single journal* is

$$f_2 = (1/2\pi)\sqrt{(c_2/p_2)} \qquad . \quad . \quad . \quad . \quad . \qquad (1)$$

where

$$p_2 = (I_0 + W_2 l_2^2)/g = (W_0 + W_2)\, l_2^2/g \quad \text{(say)}$$

g being in ins./sec.^{2}.

Let c_1 = torsional stiffness of the shaft between the airscrew and the first throw and let

$$c_1 = rc_2$$

Then the frequencies of the system in torsional oscillation are given by the formula

$$f = 2 f_0 \sin a/2 \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where a is given by

$$\tan \tfrac{1}{2}a \tan na = r/(2 - r) \quad . \quad . \quad . \quad . \quad . \quad (3)$$

n being the number of throws.

Example

Take a four-throw crankshaft driving an airscrew direct. Let the dimensions of the journal be $1\frac{7}{8}'' \times 1''$ (dia.) and those of the shaft between the airscrew and the first throw $6'' \times 2''$ (dia.).

We find

$$c_2 = 10^6 \pi/5$$

and

$$c_1 = \pi \times 10^6$$

$$c_1 = 5\, c_2$$

By formula (3)

$$\tan \tfrac{1}{2}a \tan 4a = - 5/3$$

From the tables or otherwise we find that the lowest value of $a/2$ to satisfy is

$$a/2 = 12^0\ 10^1 \text{ approx.}$$

$$(\tan 12^0\ 10^1 \tan 97^0\ 20^1 = - 1.68).$$

So that by formula (2)

$$f = 2 f_2 \sin 12^0\ 10^1$$

$$= .422 f_2$$

Let now

$$W_0 + W_2 = 8 \text{ lbs.}$$

and

$$l_2 = 3 \text{ ins.}$$

Then

$$p_2 = 8 \times 9/384 = 3/16$$

Hence the fundamental frequency of the system is

$$(.422/2\pi)\sqrt{(\pi \times 10^6 \times 16)/15}$$

or approximately 122 double vibrations per second.

(*Cf.* this with the solution of the same problem on page 213 in the JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY for April, 1923.)

CASE II.

Geared Drive (Fig. 2)

Otherwise as in Case I. The inertia of the gear wheels is supposed small.

Let ρ_1 be the gear ratio (R_1/r_1).

Then if we write

$$1/c_1 + \rho_1^2/c_1^1 = 1/C_1$$

and

$$C^1 = rc_2$$

the subsequent procedure is as in Case I.

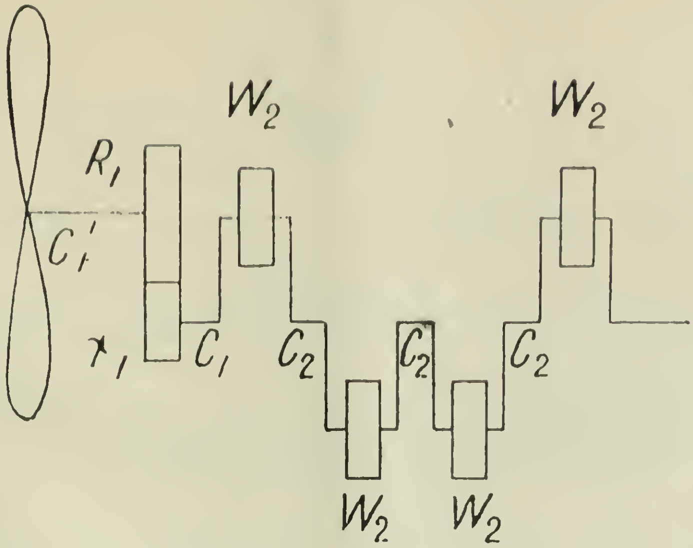

FIG. 2.

CASE III.

Double Reduction Drive (Fig. 3)

Otherwise as in Case I.

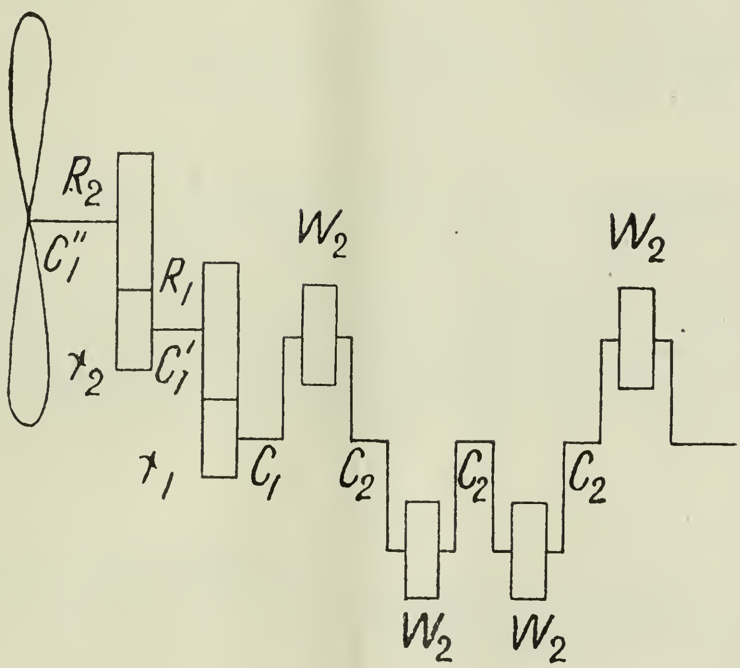

FIG. 3.

$$\rho_1 = \text{first gear ratio } (R_1/r_1)$$
$$\rho_2 = \text{second ditto } (R_2/r_2)$$

Then if we write

$$1/c_1 + \rho_1^2/c_1{}^1 + \rho_1^2\rho_2^2/c_1'' = 1/C_1$$

and

$$C_1 = rc_2$$

the subsequent procedure is as in Case I.

ROYAL AERONAUTICAL SOCIETY

(Founded 1866).

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Aims

The Society exists for the furtherance of the Science of Aeronautics, and its activities fall into the following headings:—(1) Protecting the interests of the aeronautical profession by conferring a technical status on those qualified for such distinction. It acts as the professional society or institution of qualified aeronautical engineers. (2) Organising discussions and publishing papers on subjects of importance in connection with the various branches of aeronautical science. (3) Encouraging and assisting technical students who desire to adopt the aeronautical profession for their careers. (4) Providing an organisation wherein those interested in aeronautics from scientific or other motives, but who are not professionally connected with aviation, may meet together, have opportunities of study and keep themselves in touch with aeronautical affairs.

Membership

The membership is divided into two categories:—

(*a*) *Technical*—(1) *Students*: Reserved for those under the age of 26 who are receiving a technical training such as will fit them in due course to become Associate Fellows. No entrance fee; *Subscription*, 1 guinea.

(2) *Associate Fellows*: Reserved for those who are duly qualified in accordance with the Regulations. Entitled to use the letters A.F.R.Ae.S. after their names. *Entrance fee*, 3 guineas; *Subscription*, 4 guineas.

(3) *Fellows*: Reserved for those who have attained to considerable eminence in the science of aeronautics, and are duly qualified in accordance with the Regulations. Entitled to use the letters F.R.Ae.S. after their names. *Entrance fee*, 5 guineas; *Subscription*, 6 guineas.

(*b*) *Non-technical* (open to all).—*Members*: All who desire to support the Society are eligible. *Entrance fee*, 1 guinea; *Subscription*, 4 guineas.
Foreign Members: Ditto. Domiciled abroad. No entrance fee; *Subscription*, 2 guineas.
Associate Members Ditto. No entrance fee; *Subscription*, 2 guineas.

Privileges

Members in any of the above grades are entitled to attend the Society's lectures, receive the monthly Journal free of charge and consult books in the Society's library. All grades except Associate Members are entitled to borrow books from the library. Students and Associate Members are not entitled to vote.

THE JOURNAL

OF THE

ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

No. 155 NOVEMBER, 1923 VOL. XXVII

NOTICES

Election of Members

The following Members were elected at a meeting of the Council held on October 16th :—

Associate Fellows.—Mr. S. H. Evans, Mr. A. P. Rowe.

Students.—Mr. G. A. Chamberlain, Mr. L. G. Brazier, Mr. S. W. Slaughter, Mr. F. Small.

Temporary Honorary Member.—Lieutenant S. Kato, I.J.N.

Foreign Member.—Mr. S. A. U. Rasmussen.

SCOTTISH BRANCH.—*Member.*—Captain A. N. Kingwill.

Honorary Librarian

Mr. J. E. Hodgson, Associate Member, has kindly agreed to act as Honorary Librarian. Mr. Hodgson has recently undertaken the re-arrangement of the Library, which has resulted in the valuable additions purchased with the grant from the Carnegie United Kingdom Trustees being now adequately displayed. It is hoped shortly to publish a complete hand list of the books, pamphlets and periodicals.

Associate Fellowship Examination

The following is the list of successful candidates in the Society's examination which took place on September 25th: Mr. P. A. Ralli, Mr. P. H. Watson and Mr. M. W. Wood.

Scottish Branch

The complete programme of lectures arranged for the Session by the Scottish Branch is as follows :—

1923.	
Oct. 11th, 8.0 p.m.	Major-General Sir F. H. Sykes, "Some Aspects of Aeronautical Progress." *Chairman*—Lord Invernairn.
Nov. 13th, 8.0 p.m.	Mr. F. T. Courtney, "The Practical Difficulties of Commercial Flying." *Chairman*—Brig.-Gen. J. G. Weir.
Dec. 10th, 8.0 p.m.	Professor Gordon Gray, "A Complete Solution of the Cloud and Night Flying Problem."

1924.

Jan. 29th, 8.0 p.m. Mr. A. H. R. Fedden, "Radial Air-Cooled Aero Engines." *Chairman*—Mr. Harold E. Yarrow.

Feb. 12th, 8.0 p.m. Mr. A. E. L. Chorlton, "The Beardmore 1,000 h.p. Engine." *Chairman*—Professor Hudson Beare.

March 11th, 8.0 p.m. Captain W. H. Sayers, "Gliders." *Chairman*—Sir John Reid.

April 4th, 8.0 p.m. Col. the Master of Sempill, "The British Aviation Mission to the Imperial Japanese Navy." *Chairman*—Professor Cormack.

April 10th, 8.0 p.m. Mr. F. Handley Page (subject to be announced later). *Chairman*—The Right Hon. Lord Weir of Eastwood.

Cambridge University Aeronautical Society

The following programme for the first half of the Session has been received from the Secretary (Mr. Basil B. Henderson, Gonville and Caius College):—

1923.

Oct. 17th, 8.30 p.m. Mr. F. Handley Page, "Commercial Aviation."

,, 24th, 8.30 p.m. Mr. E. C. Gordon England, "Gliders of the Past and Present."

,, 31st, 8.30 p.m. Mr. J. D. North, "Science and Art in Aviation."

Nov. 7th, 8.30 p.m. Admiral Mark Kerr, "The Air and the Empire in Peace and War."

,, 14th, 8.30 p.m. Sq.-Ldr. G. S. Trewin, "Aircraft Co-operation with the Fleet."

,, 21st, 8.30 p.m. Maj.-Gen. Sir W. Sefton Brancker, "Air Transport."

,, 28th, 8.30 p.m. Captain W. H. Sayers, "The Theory of Flight."

Dec. 5th, 8.30 p.m. Major H. E. Wimperis, "Some Aeronautical Problems."

Other Societies

The following list of lectures on aeronautical subjects before other Societies and Institutions is published for the information of Members. Tickets for any of these lectures may be obtained through the Secretary:—

1923.

Dec. 14th, 6.0 p.m. *Institution of Mechanical Engineers.*—Prof. A. H. Gibson and Mr. A. Wright Baker, "Exhaust Valve and Cylinder Head Temperatures in High-Speed Petrol Engines."

1924.

Jan. 9th, 6.30 p.m. *Institution of Automobile Engineers.*—Mr. A. J. Rowledge, "Water-Cooled Aero Engines."

March 3rd, 5.30 p.m. *Institute of Transport.*—Mr. G. Holt Thomas, "Air Transport and Its Uses."

Date to be announced. *Institution of Mechanical Engineers.*—Dr. H. Moss, "Air Consumption and B.H.P. of Aero Engines."

Donations

The Council desire gratefully to acknowledge gifts of lantern slides from the following firms :—

The Bristol Aeroplane Company, Limited,
The de Havilland Aircraft Company, Limited,
The Fairey Aviation Company, Limited,
Short Brothers (Rochester and Bedford), Limited,

and of photographs from the Blackburn Aeroplane and Motor Company, Limited.

Forthcoming Arrangements

Thursday, Nov. 1st, 5.30 p.m.	Major H. E. Wimperis, "Some Recent Developments in Aircraft Instruments."
Tuesday, Nov. 13th, 5.30 p.m.	Council Meeting.
Thursday, Nov. 15th, 5.30 p.m.	Mr. H. R. Ricardo, "The Thermodynamics of Aircraft Engines."

The lectures take place at the Royal Society of Arts, 18, John Street, Adelphi, W.C.

W. Lockwood Marsh, *Secretary.*

R38 MEMORIAL PRIZE, 1924

Regulations

From the income of the above fund a sum of twenty-five guineas will be offered as a prize for the best paper received by the Royal Aeronautical Society on some subject of a technical nature in the science of aeronautics. Other things being equal, preference will be given to papers which relate to airships.

The prize is open to international competition. The Royal Aeronautical Society retains the right to withhold the prize in any year if it is considered that no paper is of sufficient merit to justify an award.

Intending competitors should send their names to the Secretary of the Royal Aeronautical Society, 7, Albemarle Street, London, W.1, on or before December 31st, 1923, with such information in regard to the projected scope of their papers as will enable arrangements to be made for their examination. The closing date for the receipt of papers will be March 31st, 1924.

Papers, which must be submitted in either French or English, should in all cases be typed, and a copy should be retained by the author as the Society can take no responsibility for the loss of copies submitted to it.

Successful papers will become the absolute property of the Society and will in most instances be published in the Journal of the Royal Aeronautical Society. A signed undertaking must accompany each paper to the effect that publication has not already taken place and that the author will not communicate it elsewhere until the Society's award is published.

The Society attaches special importance to papers showing original work, and due acknowledgment must be made by the author of the source of any special information.

PROCEEDINGS

Inaugural Meeting, Fifty-Ninth Session

The first meeting of the 1923-24 Session was held in the rooms of the Royal Society of Arts, John Street, Adelphi, London, on Thursday, October 4th, 1923, when the Chairman (Colonel A Ogilvie) delivered his Inaugural Lecture, entitled, "Gliders and Light Planes." The chair was occupied on this occasion by Professor L. Bairstow, retiring Chairman.

Presentation of Pilcher Memorial Prize

The Chairman said that the first business of the evening was the presentation of the Pilcher Memorial Prize, which had been awarded to Mr. A. P. Rowe for his paper on "Air Navigation." On behalf of the Society, therefore, he asked Mr. Rowe to accept the prize, which consisted of books.

Mr. A. P. Rowe received the prize amid loud applause.

The Chairman, proceeding with the ordinary business of the meeting, said that in accordance with the scheme introduced last year for the first time, one of the penalties of becoming chairman of the Society was that the holder of that office must read the first paper of the session and spend such time as was necessary to prepare it. He believed that during the last few weeks Colonel Ogilvie had found it difficult to find time to prepare his paper. The competition for the prizes offered by the Duke of Sutherland and the "Daily Mail" for gliders and light planes was to be held during the coming week and Colonel Ogilvie had a great deal of work to do in connection with this.

Colonel Ogilvie then read his paper.

GLIDERS AND LIGHT PLANES

BY COLONEL A. OGILVIE, C.B.E., M.I.MECH.E., F.R.AE.S.

Before starting on my paper to-night I should like to express two things which are much in my mind, gratification at being elected chairman of this long established scientific society and nervousness at the prospect of stepping into the shoes of Leonard Bairstow.

Professor Bairstow is one of the most important assets which this country possesses and he has been of great service to the Society for many years and particularly for the last year as chairman.

Although few, if any, of us present are able to reach his high level of scientific attainment, we are all able to appreciate his courage when carrying through arduous research work as well as his modesty when it is done.

On the Council of the Society as well as in many other connections I have worked with Professor Bairstow for many years, and I am sure that I am only voicing the views of members when I say that he has been one of the very best chairmen we have ever had, conscientious, patient under difficulties, and always steadily pressing towards the high ideals he has set up for the Society and himself.

Regarding the paper which I am supposed to be reading to you to-night as my inaugural address as chairman, I must ask for your indulgence for its inadequacy; the preparation of it has been so much interfered with by the recent

international seaplane race and by the forthcoming competitions at Lympne that I have not done proper justice to the subject, interesting though it is to me. My best excuse is that I am affording you an opportunity for a discussion on gliders and light planes which we have not had as yet in the Society and which should be of value.

My first thought on this subject is that we are opening up another path, which may lead us to a really successful application of aeronautics to peace purposes. Success in peaceful commercial work is now, even in the highest circles, becoming recognised as a necessity for success in times of trouble.

In my paper to the Air Conference in February on gliders, I attempted to draw attention not only to the great benefit which aeronautics had sustained in the past from experiments with gliders, but to the benefits obtainable in the present and in the future.

Gliders and light aeroplanes afford the best possible means of full-scale research, from which data can be gathered and ideas stimulated, towards the development of the commercial aircraft of the future. Make no mistake, it is to commercial aircraft, not to war aircraft, we must look for our real future in the air and for that we must make commercial aircraft to be real paying propositions. Can this be done? My answer is no; at the present state of knowledge the best designer in the world cannot make an aeroplane which is a real paying proposition in the commercial sense. I say this in a room full of designers for whom I have the utmost admiration and respect and from whom I never got "No" for an answer to any problem set in the war time. Then it was different, for when a designer found he could not give the necessary performance with the engine proposed, he looked around till he found one big enough to do the job. Now, unfortunately, that will not do as we, and not an indefinite State department, have to pay for the petrol and for the depreciation and insurance of our expensive machines and simply that system cannot be adopted. The experience of the last few years has shown that clearly enough to those who seek the truth.

With your permission I will read the following paragraph from the Conference paper:—

"Civil aircraft are not sufficiently safe in the event of a breakdown of the motive power, necessitating a forced landing. Undoubtedly some types are safer than others, but in all types the demands on the pilot are too severe and the risk of an accident too great.

"Civil aircraft are also much too expensive to buy, to run and maintain in running order, and until very considerable improvements can be effected in this respect, it cannot be claimed that they are transport vehicles of a really commercial character.

"The size of commercial aircraft is so great that the cost of building such machines for experimental purposes is practically impossible to private firms, apart from the cost of the large quantities of fuel required, of the maintenance parties and of the large shed accommodation and other overheads.

"State support and control of the experimental work necessarily means a measure of control of that work and it is not and cannot be really satisfactory because the man who has initiated and is conducting the experiments must be free to follow out his own ideas."

The position is therefore that those manufacturing firms who are willing and anxious to do the experimental work which will enable them to design and make better aircraft cannot afford to do so because of the small number of orders they can expect and because of the prohibitive cost of full-scale experimenting with aeroplanes.

Certainly the transport firms, notwithstanding their urgent need for improved machines for their services, cannot afford to experiment with unknown types.

It is of the utmost importance to them to simplify their organisation as much as possible. So we have to fall back on the Air Ministry, whose method is to get out a specification just beyond the attainment of designers and ask for tenders. Sometimes a firm puts up so strong a case that it gets an order for a machine incorporating some of its own ideas.

Neither system is satisfactory as regards results, owing to the delays and restrictions inevitable with Government departments, even the best of them. It is only the very exceptional designer who can supply the transport firms with anything which they can use at all.

It is hoped that the difficulties of this situation will be relieved by the glider and light plane, which enable firms to experiment at a comparatively low cost. It strikes me as a very significant thing that there should be so keen an interest in the forthcoming competitions. The actual prizes so wisely and generously put up have formed an immense attraction to pilots and designers in Belgium and France as well as all over this country, and we look like having a remarkable gathering of new designs. Would anyone a year ago have believed that a competition could take place for which nearly thirty new types of aeroplanes would be designed and built? Does anyone now doubt that valuable data and ideas will not result from such an effort? It is baiting with a minnow and catching a whale. Usually in official competitions it is the other way about. It is interesting to note that all the machines have been built as in pre-war days without departmental restrictions.

What results may we look for from the light plane? Personally, I am sceptical about the immediate appearance of large numbers of small sporting machines for one or possibly two people to cruise about the countryside. Up to date the control problem is not sufficiently solved for an aeroplane to be suitable for the general public to use for their own private touring from place to place. The performance of a new machine is usually as anticipated, but there is not a designer living who can guarantee that his new type of machine will behave in a perfectly normal manner under all circumstances, and even if he could the normal control is not good enough. The very fact that test pilots are employed proves this, unless the possibility is admitted that all test pilots are banded together in a secret society to bamboozle designers—a possibility which I personally should hesitate to accept. Until the problem of aeroplane control, particularly at low speeds, has reached a more satisfactory solution, there would be so serious a crop of accidents that people would become alarmed and aeronautics would take a set back instead of a step forward.

As far as can be judged we are not likely to get any very startling advance on control questions. In fact it is possible that we may be rather disappointed in this respect. Whether that will be so or not I am convinced that the time is not yet ripe for a widespread issue of small sporting aeroplanes among the general public.

It is a risky business to prophesy, particularly so near to the event, but in my opinion the most valuable result from these trials, apart from the further arousing of public interest and the beneficial effect of such an encounter of designers, will be the demonstration of higher aerodynamic efficiencies than we have known up to date—in other words, of better gliding angles.

What is the best gliding angle of a modern aeroplane? I mean by this the slope up which the engine has to pull the aeroplane in level flight, not the slope down which the aeroplane slides with stopped engine to make a landing.

I noticed in some recent publication that Professor Bairstow puts this angle at 1 in 9. I should have thought that figure a little optimistic and that 1 in 8 is more correct for our present commercial aeroplanes. It is certain that some of these small machines are doing better than 1 in 10. Although I myself have never seen any proof that 1 in 12 has been accomplished, I feel confident that

figure will soon be reached. I once heard Major Gnosspelius make a remark which has been in mind ever since, "A train will run down a slope 1 in 50, why not an aeroplane?"

Putting figures like that on one side—suppose one of the machines is demonstrated to have an angle of 1 in 16, what results from that? We should then know that comparing that particular machine with our present commercial ones, only half as big an engine would be required for the same work, meaning half the fuel bill, half the initial cost of the engine, half the insurance and depreciation costs of the engine and probably much less maintenance cost.

Some of a pedantic turn of mind might argue that the general arrangement of a commercial machine is so widely different from that of the light plane, that the design of the former does not benefit from improvements of the latter.

To that I would answer that a demonstration of what can be done is always of great value in stimulating thought and ideas and in setting up a standard to work towards.

From such information as I have been able to collect about these small aeroplanes, one or two interesting points appear. The first is that there is evidently no uniformity of opinion amongst designers as to the right general type to adopt, monoplane or biplane. One would certainly think that in a case like this the monoplane is the most efficient, but if the span becomes too great that structural considerations would affect the application of the full aerodynamic gain to large machines. From that point of view what we really want is an efficient monoplane of low aspect ratio.

Another point is in connection with the climb. There seems to be a singular reluctance on the part of some of these little machines to climb the first 100 feet, and no one seems to understand why. The only serious attempt at an explanation which anyone has given me was that this defect was caused by the super-efficiency of the machine. Certainly the defect if translated to the large machine would be a very serious one indeed. At present the limiting factor in practical transport technique is the ability to get off the ground and climb the first few hundred feet. The greatest efforts are applied to improve commercial aircraft in this respect. Personally, I find this point of poor initial climb very interesting because it appears to be the point where these modern machines are different from the old Wright machines, with which I am very familiar and which in some respects of loading per square foot and per horse-power were quite comparable. I have seen Wilbur Wright flying at over 55lbs. per horse-power and he had no difficulty in climbing steadily as soon as he got off the ground. My own first machine was loaded about 2½lbs. per square foot and about 40lbs. per horse-power. Its top speed was about 40 m.p.h. and I remember flying a circuit at Lanark at less than 25 m.p.h. There were two propellers of 8ft. 6in. diameter turning at about 400 revolutions and I suspect that it is in the propellers that the main difference of climb is to be found.

I hope that some experiments will be directed to the development of geared-down propeller systems.

These machines to be useful should be able to climb at a steep angle, in other words, at a slow forward speed.

On the other hand engines are steadily increasing their revolutions in all forms of locomotion—look at the geared-down turbine as well as the modern car. This process of increasing the revolutions is sure to go on, and I expect to see internal combustion engines at 10,000 revolutions per minute within a few years.

I am unable to see a direct drive propeller at 10,000 operating efficiently on an aeroplane climbing at a low forward speed of say 30 miles per hour.

It appears to me that it should be possible to develop a propeller system which would enable the full power of the engine to be effectively used from the time it is opened up, which would give a very steep angle of climb, which would be efficient at top speed and which would at the same time act as an air brake when gliding down without engine. A fine angle of descent is an advantage if the engine stops in the middle of the Channel to help you across to one side or the other, but is a serious disadvantage when actually landing, even on an aerodrome.

Members who have seen Hamel land will remember how he used to stand his Bleriot on its head and stop exactly where he wished in the smallest field.

This was chiefly because the gliding angle of the Bleriot was about 1 in 4 and the machine could be forced down at a steep angle without gaining too much speed.

Discussion

Mr. W. O. Manning said that he thought they were all indebted to Colonel Ogilvie for again drawing attention to the early work of the Wright brothers. The past machines made by them, although larger and more powerful than the modern light aeroplane, flew quite well with a horse-power loading of between 40 and 50lbs. per horse-power, a figure which has only been again approached quite recently.

During the war the improved performance of aircraft was obtained almost entirely by the progress in engine design; in fact, it is probable that no war machine at the present day has a better gliding angle than the old B.E.2, and although more is known about controls, it is not unfair to say that the improvement in aerodynamical efficiency has been nil.

Those of us who have been experimenting with light aircraft have felt that with the increased amount of information and experience that is available now it should be possible to produce types which are materially more efficient than the older forms, and the present light aircraft give some idea of what is possible in this direction.

It is not possible at present to construct commercial machines to similar models as structural considerations impose limitations, but there is no doubt that lessons will be learnt from these small machines which can be used to improve larger ones.

Colonel Ogilvie's list of structure weights for light aircraft shows that these weights are distinctly in proportion higher than those of larger machines, but it is possibly fairer to compare the weight empty and weight loaded. As the percentage engine weight is very low this ratio compares favourably with that usual in large aircraft.

With reference to the difficulty experienced by certain machines in climbing near to the ground, three explanations appear possible—that the wind conditions on the aerodrome low down are such that there is a gradient equal to the climbing angle of the machine; that the machine may be side-slipping slightly and that the design is such as to increase resistance abnormally when side-slipping; and that the machine may have two types of air flow, either of which may be to some extent stable. Of these explanations I prefer the first and it would be interesting to learn if this trouble occurs in all wind directions on the particular aerodrome used and whether or not it is peculiar to this aerodrome.

Major Gnosspelius, who designed the "Gull" light aeroplane, said that there was one remark made by Colonel Ogilvie which had appealed to him, and that was that aircraft at the present time did not pay, from a civil point of view. They were quite hopeless; and really, if we wanted to do anything we had to

make them pay somehow or other or else they were bound to die. No business could subsist on a subsidy for very long. It seemed to him that there was only one way of making them pay, and that was by decreasing the power necessary for flight; the small machine might help by enabling the experimental work to be done at reasonable expense, although the expense really was far from light. People talked of £100 and £200 for a machine, but he thought Mr. Manning would agree that £500 was about the lowest figure at which small machines could be turned out, at any rate in small numbers; perhaps the cost could be lowered if they were turned out in larger numbers. But there was another factor which entered into the problem, namely, that it did not much matter what we made, there seemed no market at present. Of course, one fact which helped very much in connection with these small machines was that there were no Government restrictions so far; that was a distinct help, because when we had to get designs passed the process always took an infinity of time and we never got ahead. Another point that Colonel Ogilvie had brought out was the question of engine power. Frankly, he did not know what the power of these small engines was. He had been told what the makers said it was, but possibly that was not what the engines gave. One of the things that should be done was to try to test these engines in order to find out what their actual power was and at what revolutions they developed this power, because the power actually obtained from different engines seemed to vary a great deal. Again, no two men obtained anything like the same power from their engines. It was a curious thing that the makers of motor bicycle engines told users to run them as fast as they could and to get as many revolutions out of them as possible, but not to open the throttle wide. He supposed that the compression ratio was so extraordinarily high that if a motor bicycle engine were run with the throttle wide open for any long period the plugs would get too hot. If we could only get data as to the actual power of these engines it would help a great deal in judging the performance of the machines, because otherwise we could not really say whether the machines were an improvement on that which already exists. He believed that the Blackburn 700 c.c. engine gave something like 24 h.p., and the Douglas gave about 26 h.p. at maximum h.p. With regard to controls, he did not know that they were so very difficult; he had not been troubled with them. The question of control had nothing to do with physical dimensions; slow speed, perhaps, did affect it slightly, but even then he personally was inclined to think that control would be easy where thin wings were used rather than where thick wings were used.

Captain G. T. R. Hill said what he wished to press for when small machines came into more general use—as he hoped they would—and what would be an absolutely essential feature if they were to come into more general use, was that they should have a sufficiency of power or, more strictly, a sufficiency of surplus power in order that the engines should not be required to run near their maximum revolutions throughout a flight. At the present time there was talk of the Government waiving its many regulations and restrictions as to flying aeroplanes, provided the power of the engine was below a certain figure. He did not think that that was really wise, and in illustration of his point he made use of an analogy. A few years ago, he said, cattle were slaughtered by means of the pole-axe, but more recently the humane killer had been introduced, which destroyed the animal by exploding a cartridge. No doubt this destruction of cattle was carried out under the supervision of the Ministries of Health, Munitions, and so on (laughter), and it seemed to him that the waiving of the restrictions on the light aeroplane below a certain limit of power would correspond with the waiving of restrictions on slaughtering cattle on condition that the cartridge used should not exceed a certain size. Surely it was at this point that restrictions should be most severe, because if the cartridge were not quite powerful enough it might only partly kill the animal; in the same way, if designers of small aeroplanes were working to a

definite engine power limit, beyond which they were not allowed to go, the probability of trouble would be obvious. Naturally they would want to build machines as large as possible up to the engine power limit, and that would lead to the restriction of reserve power. The chief advantage of the limit of engine power lay in the fact that it could be easily comprehended by both the officials of the Air Ministry and by our designers. A better kind of limit would be to rule that any aeroplane must be able to fly on, say, half its maximum power.

With regard to control, it had been suggested that thick wings allowed less lateral control. It would be interesting to find out whether that was really so or not. He was not a thick wing enthusiast, as some people in Germany were, but it remained the fact that the Fokker D.7 biplane, which was used so much by the Germans during the war, had a thick wing and was extraordinarily good on the controls. There was that definite evidence that a thick wing section need not necessarily lead to trouble with the control.

The point raised with regard to failure to climb well soon after leaving the ground was very interesting. He felt quite unable to explain it, but it was no good denying that it did occur, and it was well worth seeking for the reason. Of course, down currents on a slow-climbing aeroplane would obviously make an appreciable and visible difference, but there seemed to be something more than that. As regards loss of power due to side-slipping, he did not know of any data which might support that. He was rather of the opinion that when side-slipping anything up to about 5 deg. on the bubble, there would be no appreciable loss in gliding angle. He did not know whether anybody could support the contrary theory with figures, but he hoped they would if they could.

Finally, he asked whether the Society, remembering it was up against the "Daily Mail," would do its best to abolish the abominable term "motor glider."

Major A. R. Low, dealing with structural weight, said he had a theory—which was supported by many people, including Professor Bairstow, but opposed by others—that as the dimensions of a machine increased the famous law of the cubes came and sat on the structural weights and made them rise so rapidly that presently a limit was reached beyond which an aeroplane could not be constructed. That theory, quite definitely, was not based on the best practice he could find recorded, that at about five or six tons we were getting everything we could out of the materials, at 10 tons we were not getting the full return, and at about 15 tons we ran into a dangerous load where the factors of safety and controllability could not be kept up adequately. At the other end of the story came the small machine and the model and a very interesting point arose. It was claimed by the advocates of large machines that they could fine a number of details, and he believed that was quite true up to about five tons. Beyond five tons, however, the amount of fining that was going to be obtained could be expressed as a very modest percentage, but with the very small machine it was the other way. To take the case of the "Wren," he understood from Mr. Manning that his minimum factor of safety was four or five, but in lots of places it was 30 or 40, because the members could not be fined down without including a spar or something which would break if one laid one's hand on it. He had plotted a curve, ranging from the 12-tonners of the American Navy at one end to the "Wren" at the other. If one could build a small machine as finely as one could build a large machine, then structural weights would be the same for both machines. As an example, he said that if we could build a bridge with a span of 10 or 20ft., with a piece of lace-work such as that of the Forth Bridge, with its 1,700ft. span, then the curve of structural weights should be a horizontal straight line. But with his curve the result came out that the ratio of structural weight of a small machine to the structural weight of a really large machine was something like 12 to 4. It was really three times as heavy in its wing as the large Handley Page or Farman Goliath, and that was quite unavoidable, because

we could not make a practicable aeroplane with a lattice-work of structural members such as those of the Forth Bridge. With regard to gliding angle, 1 in 9 was considered by the lecturer to be too high and 1 in 8 to be more probable, but he did not think that squared with the results obtained from the "Wren" in actual flying.

Colonel OGILVIE pointed out that his figure of 1 in 8 referred to the commercial machine of to-day and had nothing to do with the glider.

Major LOW replied that as the lecturer had saved his position he would attack the Chairman, who had published, he believed, a figure of 1 in 9 as a suitable estimate of what the motor glider could do. That did not quite square with the known performance of the "Wren." He considered that it ought to be placed very much higher and invited Mr. Manning to give some sort of estimate based on his tests. He should think that it should be considerably above 10. As to controllability, this, he believed, was largely a psychological problem. At one time he had had to collaborate in carrying out climbing tests on all sorts of machines at Brayle station. Some were well-known standard machines and others were fearful and wonderful freak machines of novel design. With a standard machine they obtained a nice straight climb, but the creep machines always went up on a beautiful curve. He believed that that was largely a psychological effect and was possibly due to the pilot being nervous near the ground, but when he got to 2,000 or 3,000ft. he pulled the machine back to its climbing speed and climbed faster. On a number of the standard machines there was always a tendency, especially on gusty days, for the climb at lower levels to be slower relative to that at higher levels. There seemed to be a loss of energy; there was no reserve power and a gust of wind might make the machine lose quite an appreciable amount of climb by forcing it momentarily over the fast climbing angle into a stalling position, owing to side-slip. With regard to the propeller, the smaller the diameter the higher the speed it will attain. If the diameter were reduced from 9ft. to 3ft. the revolutions would increase from 1,000 to 3,000 without loss of efficiency.

In reply to Mr. Handley Page's remarks on the possibility of larger machines, I should like to know whether he is speaking as a designer of genius or as a salesman of genius.

Mr. F. HANDLEY PAGE said that the figures which Major Low had given for the structural weight of large size machines, or the conclusions he had drawn from them, were hardly correct. Speaking of the 30,000lbs. Handley Page machine, the factor of safety for that was four and the structural weight was, he believed, 31 per cent., so that, although the machine weighed about 15 tons, this machine had not the ascending part of the structure weight curve of which he (Major Low) was speaking.

With regard to motor gliders, one of the things that instantly occurred to most people, particularly those associated with the Press, was that the motor glider heralded the dawn of a new era in commercial aeronautics. It was said that we were going to have machines which, carrying 100lbs. per horse-power, would attain a speed of about 100 miles per hour, but the actual figures showed that, although the machines were more efficient in the weight per horse-power which was carried, the speeds reached were not anything like sufficient for commercial work. Machines such as the "Wren," with a top speed of 50 miles per hour, were extraordinarily efficient machines, but their speed would have to be increased to two or two and a quarter times the top speed to enable them to be of any use for a service between two points in all kinds of weather. But when we sought to attain greater speed we began to increase the horse-power and the weight, and to add all sorts of things, and eventually arrived at a commercial aeroplane not very much different from the machine which Major Gnosspelius had described as perfectly hopeless.

In that connection it must be borne in mind that the cost of upkeep and operation of the aeroplane, on the technical side, was not the only cost involved in running an air service. During the months of November, December and January there might be a week in which there was fog over the whole of the country in which they were operating, and during which there was no possibility of flying at all. If the people on the ground who were connected with the machine did not want paying during that week, and there were no upkeep charges, all would be well, but an aeroplane, unfortunately, required a staff of people to deal with it on the commercial as well as the technical side, whether flying or not. At the present time, if it were possible to fly all the year round as one could fly during June, July, August and September, the question of subsidies would not arise. Without a doubt the commercial efficiency of a machine would be actually increased by being able to carry more weight per horse-power, but that would not be a complete solution of the problem.

However, one did feel that the use of motor gliders, and the results that had been obtained with them, certainly marked a move forward in aerodynamical progress, and he hoped that we might look forward to a three-engined machine of this category, built something like the "Wren," when we could, with the extra horse-power available, perhaps be able to carry two passengers as well as the pilot. He hoped he was not too optimistic, and that, with that added security, a pilot would be able to fly in worse weather than he could fly in at the present time. There would be the added advantage that such a machine, being fitted with motor cycle engines, which were cheap, would not be so expensive to run or to maintain as some of the bigger machines with modern aircraft engines. Manufacturers of aircraft engines might look on them with alarm, but the manufacturers of the motor cycle engine might possibly be able to achieve the same kind of horse-power that the former could. In thanking the lecturer for his paper, Mr. Handley Page said that when one saw the original pictures of the Wright machines, one realised how old an experimenter he was and the immense amount of technical work he had done for the aeroplane and for its development in this country; the country and the Society owed him a great deal for the part he had played in the development of aviation in this country.

The Chairman said that Colonel Ogilvie had taken them back to pioneer days and had shown his connection with the first gliders, and in other respects the paper had indicated a going back to the past in a beneficial sense. It might be quite true—and he agreed with Mr. Handley Page in this respect—that if we tried to do anything we are doing now with gliders on a larger scale we should get back very nearly to where the commercial aeroplane is at the present moment; on the other hand, he believed some improvement was obtainable and that the improvement was worth having. It might, for instance, have kept the Schneider Cup in this country. One thing had struck him during the lecture as worthy of special note. From the point of view of getting valuable scientific information from the coming trials at Lympne it was desirable to know the horse-power of the engines. Colonel Ogilvie had said—and the point had been emphasised by Major Gnosspelius—that the horse-power of a particular engine was not accurately known, the various estimates varying by as much as 33 per cent. Whilst Colonel Ogilvie had put the horse-power at 14 or 15, Major Gnosspelius had put it at 24 or 26, and if the output of an engine is unknown to within 33 per cent., that particular factor was useless in a competition to determine the most efficient glider, for the differences between the best and worst would not be so big as 33 per cent. He did not see why it should not have occurred to the makers of these aeroplanes or to the people who were to test them, that the glider, with its engine, was almost ideal for the purpose of measuring the power of the engine. It was comparatively light, with a maximum weight of something of the order of 600lbs.; it could be hung from the roof of the hangar on two or three wires and

the whole arrangement, with the airscrew running, became an admirable torque reaction balance. That was a method of measuring horse-power which we knew to be accurate and which was our best standard method. He hoped that before the end of the trials some means could be taken of finding out, in the case of all the machines that made any show in the air, exactly what horse-power was developed in each case for given revolutions of the airscrew. With regard to control, he was not quite sure whether it was clear, from what Colonel Ogilvie had said, that he was not looking for startling developments in control at all or whether he confined his remarks to the coming trials. His (the speaker's) own impression was that the startling improvements in the aeroplane of the future would come in its control. Commercial aviation could not afford to leave the question of control in its present position. It was well known that if a pilot —a careful, skilled pilot—came to a difficult landing, he wished, of necessity, to come down at the least possible speed, because that made the bump at the end so much less. But if, in attempting to get that least speed, he made a slip— instead of flying at 50 miles per hour he flew, say, at 45—then there was all the difference in the world between the controls of the aeroplane as he intended to have them and as he had actually got them. For some seconds—it might not be very long, but for seconds, at any rate—he was powerless to control his machine. The sequel was well known; one wing went down and a spinning nose-dive was initiated. There was no danger in the spinning nose-dive at an altitude of several thousand feet. Every pilot knew just how to move his stick and how to recover; the manœuvre required to recover from the spin was, first of all, to push the stick forward, *i.e.*, to keep the machine in the dive, to reduce the angle of incidence, as a result of which the machine would stop spinning, gather speed and fly. A spin low down was dangerous just because there was not sufficient time to carry out the manœuvre. Measurements had been made as to the height necessary for complete recovery, and he believed that no aeroplane, not even the most controllable, could be trusted to recover from a stall in less than 500ft. fall of height. That was a serious state of affairs and one to which this country was directing attention, and he expected that in the near future, depending to a large extent on the facilities provided by the Air Ministry, it would be possible to so re-design the controls of an aeroplane and the aeroplane itself that it would be controllable when stalling. If the engine stopped the pilot must come down, but he believed it would be possible to give the pilot so much control over his machine that he would come down on his wheels and not on the front of the machine. This work is not essentially furthered by the forthcoming trials on light planes, the main object being the development of high efficiency. For the sake of the general progress of aviation it was desirable to devote as much energy as possible to the problem of reducing the head resistances of aircraft, a problem to which the construction of the light aeroplane seemed to be admirably adapted. Finally, he congratulated Colonel Ogilvie on his energy in getting his collection of slides together at such short notice, and also on giving the meeting one of the earliest pieces of advance information on the subject of low-powered aeroplanes.

Mr. R. K. Pierson (Messrs. Vickers, Ltd.) said that one point which occurred to him was that the weight per horse-power was too low. Assuming the weight of the aeroplane to be about 500lbs., every horse-power lost in the engine, due to faulty running, etc., reduced the rate of climb about 40ft. per minute. The rate of climb of the average light aeroplane was about 200 to 250ft. per minute, which meant that if the engine were 6 h.p. down it would be impossible to climb and if the load per square foot were reduced to overcome this it would be very difficult to fly in gusty weather. A light wind would take the machine off the ground.

With regard to the horse-power of the engine, he believed a particular Douglas engine was giving about 19 h.p. at 3,300 revs. per minute. He was using a propeller on that machine which was a small scale reproduction of one

used on a Napier Lion motor, and although the scale effect was unknown, the h.p. worked out to about that figure from the torque curve.

Colonel OGILVIE, in reply to the discussion and referring to the Chairman's remarks about control, said that what he meant was that he did not imagine that we were going to advance the solution very much in the coming competition; but, of course, one could not tell. Certainly one man, who was in great difficulty last year, had improved his machine with a view to getting better control over it. So far as he could understand, there was likely to be a very severe rush as soon as the weather was calm, but he trusted that it would not be too calm. That day he had received news that there was to be a competition for altitude, for a prize to be offered by Sir Charles Wakefield for the best height attained. He was very pleased to know that, because he had always felt that there was a serious lack of stimulus to the climbing of these machines, and climbing was one of the most valuable features; in fact, from the commercial point of view, climbing from the ground was the most important. As to the percentage of engine weight, as far as he could make out, the percentage of the total machine which the engine, tanks and fuel comprised was certainly very low—something in the neighbourhood of 15 to 20 per cent. on these machines. For structure percentages, such figures as he had were 40, 48, 55, 45 and 50, which he had estimated from the figures given him by different people. As regards the power of the engines, which Major Gnosspelius had referred to, he should have thought that with a light aeroplane one would have had an extremely good chance of measuring it. Dealing with Captain Hill's remarks as to control, Colonel Ogilvie said that what he himself meant was that he did not consider the control of any aeroplane to-day was good enough, because of this question of stalling and uncertain action of the machine. It was quite different from what the pilot ought to be allowed to have. Pilots put up with it, however, quite customarily and without thinking about it, but it was not good enough, and certainly not good enough to allow of the general issue of small machines to the public. Captain Hill was particularly interested in control and, in fact, had been engaged for some time on the design of a special machine which should have a much better control and which should not be prone to stalling or spinning; he hoped he would be successful with it. As to the gliding angle of the "Wren," he believed he was about right in saying that, as far as we had got at the present time, the angle was about 1 in 9 at top speed and 1 in 14 climbing. We ought to get higher lift over drag ratios in climbing at top speed.

A hearty vote of thanks was accorded Colonel Ogilvie at the conclusion of the discussion.

THE DESIGN OF MARINE AIRCRAFT IN RELATION TO SEAWORTHINESS

BY FLIGHT-LIEUTENANT D. F. LUCKING, R.A.F.

The present paper is an attempt to describe the disabilities from which various types of marine aircraft suffer in regard to seaworthiness, and its scope is limited strictly to that laid down by the title.

Some notes made by the writer formed the basis of a discussion at which were present several officers who have had very valuable experience in the handling of marine aircraft under all conditions. It was then remarked that there was more agreement upon controversial points than might have been expected, and the task of recorder is therefore comparatively simple. The greater part of the matter which follows has, however, come from other sources, such as Aeronautical Research Committee reports, and the technical press.

Qualitative data only is available to any extent. Full scale experiments to obtain a quantitative solution of some special seaworthiness problem have hitherto been few and far between (25), and improvement in this respect cannot be looked for in the near future. The sort of information now available, and which is constantly being supplemented, is in the nature of the observations and experience of the users and manufacturers of seaplanes. It might be mentioned here that the term "seaplane" is frequently used in its generic sense of including all kinds of marine aircraft.

A large amount of model work has been done, and the most important part has been in regard to water resistance and stability under way. It is not purposed to go into the question of resistance in any detail, as this can be studied at first hand in the Aeronautical Research Committee reports. Other special branches of research, that of corrosion for instance, although possibly included in seaworthiness in the broad sense, cannot be done anything like justice to in a paper such as this, and are therefore not touched upon.

The possibility of constructing a seaplane or flying boat to be seaworthy in the sense that a ship is seaworthy is not in sight owing to the amount of what a seaman would call "top hamper" which it is impossible to stow away or even, as a last resource, to jettison. The only thing that can be done in this way is to fold the wings towards the tail or in some other way. At present this folding operation is done to save storage space and is not carried out afloat as a rule. It does not reduce the exposed surface, but may improve its disposition. In any case it is doubtful whether the additional weight and complication can be afforded in the case of large types. The results of carrying out the folding operation afloat in the case of certain seaplanes during the war were so unsatisfactory that the practice had to be forbidden owing to the risk of capsizing stern foremost.

It must be realised at the outset that many of the more obvious means of improving the sea-going qualities of marine aircraft may not readily be adopted for aerodynamic reasons. Lowering the centre of gravity for instance—a question which will be enlarged upon later. Another obvious solution of all seaworthiness problems is to build very large machines. Even if such a course were theoretically justifiable (1*a*), it would be undesirable to take it at the present time owing to the expense involved with each separate experiment, and systematic research with smaller types should provide data for the elimination of radical weaknesses before very large designs are commenced. However, so far the theorem that seaworthiness improves with unlimited increase of size lacks that completeness of proof in the case of aircraft which holds good for ships, and which practice has confirmed as far as these are concerned.

The term "seaworthy" as used hereunder is to be understood as meaning comparatively so. As far as can be seen at present, even greatly improved aircraft will have to make use of their relatively tremendous speed to return to port when warned of the approach of really dirty weather.

Unfortunately, the maximum duration of heavier-than-air craft is insufficient to enable them to remain in flight long enough to outlast a disturbance, though it is satisfactory to observe that the length of life of a disturbance is usually of opposite measure to its severity. Against this is the fact that the swell following a gale is often of some persistence. A seaplane may therefore be able to alight only with difficulty and quite unable to take off for some time after the air has become relatively calm.

Some of the more elementary but not universally known characteristics of marine aircraft will now be outlined.

In order to maintain a satisfactory clearance between the propellers and the water line a high centre of thrust is essential (2*a*). Matters are made rather worse in this respect because a seaplane propeller has a larger diameter and smaller pitch than flying problems demand (25). This is in order to provide a large thrust below flying speed to assist taking off. Aerodynamic requirements, however, necessitate an approximation of the thrust line to the centre of gravity. So that it follows that a high centre of gravity cannot be avoided. Therefore the only way to secure a positive metacentric height is to distribute the buoyant components.

The provision of an adequately lengthy hull or float to give a positive longitudinal metacentric height presents little difficulty, unless the wings are folded; but wind effects on the aerostructure necessitate a considerable lateral metacentric height. In the twin-float seaplane a satisfactory dimension may be obtained by making a sufficiently widely tracked undercarriage. In the ordinary single-float seaplane or flying boat the metacentric height of the hull alone is negative, and therefore small floats attached to the wing tips are necessary (3). The effect of even slight immersion of one wing-tip float is to displace the centre of buoyancy considerably over to that side so that the metacentric height becomes positive. Alternative methods of reaching this end are discussed later.

Here the deduction might be made that because the thrust line must be kept as low as possible consistent with adequate water clearance, therefore it will be necessary to develop multi-engine craft, each engine with its own propeller—that is to say, one should have two or more small engines and propellers rather than one big engine and propeller. Against this, however, it might be said that it is easier to shield a single than a number of propellers by using the hull or floats as a screen between the water line and the propeller disc. In addition, moments of inertia are increased by separating the engine weights.

If a smooth-bottomed hull such as that of an ordinary motor-boat is propelled at high speed through the water, it tends to suck down by the stern and increase of power will make practically no difference to the maximum speed attained with a given hull. In racing motor-boats and in aircraft this difficulty is overcome by breaking the continuity of the hull bottom and making a definite step, so that the bottom ends abruptly and then restarts an inch or so higher up. It is essential that air should be allowed to reach the space just abaft the step either by leaking in past the chines or by means of ventilators (3, 5, 6, 7). When a certain speed is reached, the water breaks away from the hull bottom abaft the step, and hydroplaning commences. Considerations of stability when hydroplaning as a rule necessitate more than one step being fitted, and up-to-date practice favours a main step in the neighbourhood of a vertical line through the centre of gravity and one auxiliary step aft, an arrangement tantamount to that of the conventional disposition of flying surfaces (3*a*).

A considerable amount of model research has been carried out in the Froude tank at the N.P.L. Probably the most valuable results obtained have been in

regard to the maintenance of longitudinal stability when under way (10, 11, 12), and to low resistance shapes. In some ways, model tests are unsatisfactory, such as :—

(1) When a model hull is run at the N.P.L., it has been customary to allow for the wing lift by assuming it to vary as the square of the speed, and by the use of counter-balance weights accordingly. This method is faulty in that it does not allow for the effect of variations in incidence on the wing lift, nor can the conditions during acceleration be reproduced. It would appear that better results might be obtained by the use of a small submerged plate appropriately attached to the model hull. It is believed that something of this sort has been done in the U.S.A. (18).

(2) Much of the spray which appears innocuous in a model test is, in the full scale, caught by the wind and propeller with destructive results.

(3) A revolving propeller not only catches any spray that is going, but actually sucks up miniature water spouts, which are quite large enough to be a nuisance. This effect is not seen during a model test.

(4) The actual manœuvres of getting off and landing cannot readily be imitated by a model.

(5) It calls for craftsmanship of a very high order to ensure that a model and its prototype shall be geometrically similar to the order of accuracy required. A difference between model and full scale behaviour has been traced to slight discrepancies in form in one case at least.

The water resistance of a stepped hull or float increases up to a point and then decreases as the speed is gradually increased. The failure of early types of marine aircraft to " unstick " was usually due to insufficient power to get past the hump in the float resistance speed curve. The hump occurs at a speed less than that of hydroplaning. When hydroplaning occurs the craft virtually ceases to have displacement, the weight being supported by the water reaction on the planing bottom of the hull or floats and by the wing lift. As the speed increases the wings gradually take over the load, and the wetted portion of the hull bottom decreases in area.

The porpoising of marine aircraft when hydroplaning is probably analogous to the slow speed or phugoid oscillations of a machine in flight. After it has been initiated this pitching oscillation as a rule tends to increase in amplitude and may become dangerous. It is chiefly dependent upon the positions of the steps and the contours of the hydroplaning surfaces.

A good many variations of the features referred to above have seen service, and a short description of the nature and characteristics of some of them may be useful.

The single-step planing bottom is exemplified in the Short and Fairey seaplanes of which the main floats terminate with a broad heel, forming what is effectively a step if not actually so, so that this type of float is sometimes called "stepless." A stern compartment with a raked-up bottom of no hydroplaning significance, but useful in providing tail buoyancy, is sometimes added to such floats, which then become " single-stepped " floats.

The modern N.C. flying boats and the early America, from which the F boat was developed, are single-stepped types; but F.5 has two steps—a main step about three feet abaft the centre of gravity and a tail step seven feet abaft the main step. Both steps terminate planing bottoms which show as straight-sided vees in the body plan. In the case of the F.5 the term " tail step " is misleading, the arrangement being really that of tandem main steps.

As mentioned earlier, a small tail step is now as a rule added well aft to act as a stabiliser to the main step. This form of two-stepped hull may be made to

hydroplane at all speeds with little or no porpoising tendency and is becoming popular for this reason.

In favour of the single-stepped or tandem-stepped types, and against the stable type, it is often said that the former possesses a wide range of trimming angles which a skilful pilot may use to advantage, particularly in rough weather (25). On the whole, pilots seem to think little of this argument, as owing to paucity of air control the water forces cannot be overcome except when hydroplaning in the neighbourhood of the flying speed, and then only with difficulty. Viewed solely as a hydroplane, a single-stepped seaplane may possess neutral stability, but in general is unstable and porpoising has to be damped by the action of the tail plane and elevators. One can infer from this that the single-stepped type of hull or float is suitable for smaller rather than larger craft.

The successful development of the stable two-stepped hull is due to model research at the N.P.L. The best form arrived at so far has the forward or main step in the neighbourhood of the centre of gravity—that is, further forward than the F type and the interstep distance two or three times that of the F type. Both planing bottoms show as hollow-sided vees in the body plan. In the sheer plan the two steps are arranged so that when hydroplaning the craft trims at the most favourable attitude for getting off. Some tendency to porpoise may still exist in this type, but occurs, if at all, at so high a speed that it is damped automatically by the aerostructure without the pilot using the elevator control.

A very large potential buoyancy seems to be necessary to give cleanliness at low speeds and low water resistance, and attempts to reduce hull sizes have not so far been successful. Existing float seaplanes have a potential buoyancy of from two to three times the displacement; in other words, a reserve of buoyancy of from 100 per cent. to 200 per cent. The similar figures for flying boats are greater, principally owing to the necessity for sufficient free board to keep green water from the cockpits and because the tail unit is carried by an otherwise unnecessary extension of the hull proper (*2d*, *3b*, 9, 13).

Consideration of first principles might lead one to conclude that it would be impossible for an economical aerostructure to stand up to water shocks. The relation between the two densities is of the order of 1 to 800, so that the velocities will have to be inversely as the square root of this ratio for water pressures to remain as low as air pressures. That is to say, that water moving at more than 1/28th of the flying speed may produce greater pressures on the aerostructure than occur in flight. Put in another way, one per cent. of water in the air will increase the unit loading to eight times (26). However, it should be realised that water shocks, to anything except the hull, will only be local as a rule, and therefore stress the skin or covering and ribs rather than the main structure. It seems probable for this reason amongst others that that form of cantilever wing construction in which the skin takes the major stresses will take an important place amongst design developments on the marine side. Designed to this end, the skin will probably be heavy enough to withstand severe local stresses due to water forces. With conventional methods of construction one must be content with a craft of which the aerostructure cannot stand up to water shocks and must therefore be kept as free from them as possible.

The lower plane and elevators of marine aircraft are most vulnerable parts. The former more particularly in flying boats and single-float seaplanes; the latter in the case of tail-floated seaplanes which lie very tail down at rest or during slow speed taxying. The effect of severe water shocks is to tear the fabric and to break or bend the trailing edge and ribs. Damage to the main spars does not usually occur. Cases have happened when the crew of a flying boat have judged it expedient to cut away the fabric of the lower plane after a forced landing in bad weather. This is done to allow the seas to break through the plane structure freely, so causing it to be subjected to less severe blows. The lower plane cannot

well be raised so that the reduction in size and boat-building of this component is receiving attention. Owing to the moving control surfaces of the tail unit, boat-building it alone would not be a sufficient protection as the series of fittings from the elevators and rudder to the control column and rudder bar have also to be considered. Also minor damage to a control surface may more readily have serious results in flight than that occurring to a fixed surface. In consequence, the only thing seems to be to keep the tail unit as high as possible. Open cockpits may be fitted with canvas covers when afloat in bad weather, and these are not unsatisfactory apart from the trouble of fitting them under difficult conditions.

Lack of seaworthiness may be betrayed in many different ways, according to the speed at which the craft is moving and the manœuvre which is being performed.

When lying at rest the aerostructure, particularly the lower plane and tail unit, may be damaged by receiving water shocks, while the cockpits may be swamped. When taxying at a slow speed the nature of the conditions changes from that at rest, chiefly in that the bows of the hull or floats give rise to a wave formation and spray, from which the propellers and crew may suffer. Diving may occur at low speeds with a big sea running (3, 3*b*). In general terms the tendency to dive is inversely proportional to the length of forebody. In smooth water and with a normal load, a machine will trim slightly down by the nose as it moves off from rest owing to the high centre of thrust. Taxying with a beam wind is unpleasant for the crew in the nose of a flying boat hull, which is usually very wet under this condition. As the speed is increased, cleanliness forward is obtained as the forefoot lifts and the bow wave retreats aft. Under this condition and during the commencement of hydroplaning the bottom plane and tail unit are usually deluged with spray, but the propellers are running cleaner if well forward. During high speed hydroplaning, immediately previous to taking off porpoising may occur, but can be damped out more or less by the air controls. The loading on the hull bottom is locally severe under this condition, but less severe than that due to a heavy landing.

The result of a bad landing may be the breakage of almost any part of the structure according to the nature of the fault in the manœuvre. Rather unexpectedly wing-tip float failure is not uncommon in this connection (22*c*). The rigidity of the interplane trussing and the fact that wing-tip floats are not as a rule sprung in any way may account for high impact stresses being reached. There is always a risk of waterlogging a wing and capsizing if a wing-tip float goes; but, in service, flying boats have been brought in safely with only one sound float (22*g*).

If a nose-down landing is made owing to an error of judgment, diving usually occurs and the machine is badly crashed (3, 3*b*). The results of a pancake landing are not as a rule so disastrous.

Important to remember is that a bad landing may injure a craft in such a way as to make it succumb when riding out afterwards, and a final test of seaworthiness may be the ability of the craft to remain afloat when holed. A partial loss of buoyancy may result in a loss of trim sufficient to cause further flooding.

Nevertheless, it has been found by experience that taking off is the most difficult operation under adverse conditions, and the ability for a craft to carry this out successfully is the principal criterion of its value in service.

It has been found by experience that when taxying with a big sea running it is necessary to go dead slow, otherwise one wing tip dips into green water. This may be due to an unbalanced water drag checking one wing and causing the machine to yaw so that the lift of the arrested wing is decreased. The heel so initiated makes matters worse. This digging-in tendency is probably encouraged by the shape of the nose of the conventional aerofoil profile with its heavy top camber.

It is noteworthy that in one direction only—that of hull flexibility—has there

been a serious attempt made in this country to improve radically the seaworthiness of the larger flying boats since the advent of the early America type of twin-engined boats at the beginning of the war. From this type the F.5 and more recent and larger types have been developed chiefly by increase of size and minor alterations.

The very serious difficulty of making a float or hull structure light enough not to handicap performance unduly and yet capable of withstanding severe water shocks without failure is not perhaps fully appreciated by those inexperienced in the construction and use of marine aircraft (3*d*), though it may be remarked in passing that the actual air performance of flying boats is very little inferior to that of the average aeroplane under identical conditions of useful load. A flexible hull of the Linton Hope and Supermarine types has a bottom that is capable of considerable vertical movement in relation to the remainder of the structure, by this means obtaining a shock-absorbing capacity. As in other engineering structures, this makes for lightness. The nature of the construction is such that great skin strength is given to the hull by means of considerable wall thickness and closely-pitched timbers and longitudinals, but there are no rigid frames, which are replaced by hoops. The transverse sections of the hull are usually circular or nearly so. Flexibility and shock-absorbing capacity are not the only advantages possessed by this type. In addition, a better distribution of stress is obtained owing to structural homogeneity.

One of the theoretical objections to the use of flexible hulls which does not seem to be borne out by practice so far is that of the rigidity of the main plane structure carried by the hull. This objection would not appertain if the aero-structure were only a simple pin-jointed frame in which the hull would take the place of one or two members in each truss, but the conventional wire-braced box girder with its continuous spars can only allow for the panting of the hull by spar flexure and the working of ostensibly rigid joints. However, the writer has heard of no case so far in which the attachment of a rigid interplane structure to a flexible hull has been attended by the unsatisfactory behaviour of either, with the exception of the case of the first Phœnix Cork flying boat, of which the main planes became " soggy " after very little flying. This trouble did not re-occur with a new set and is therefore thought not to be connected with the hull design. It may be argued, but only by those without personal experience of the manifestations of hull flexibility, that the foregoing shows that the hull does not really pant to any extent.

It may be mentioned here that even with the so-called rigid hull the bottom deflects upwards, locally causing considerable strains and tending to open joints. During some purposely badly-made landings in an H.16 type of flying boat a deflection of $\frac{3}{4}$in. relative to the keelson was measured at a point midway between two floor beams at a distance of 18ins. from the centre line. The floor beams were pitched $14\frac{1}{2}$ins. The maximum deflection occurred about 6ft. forward of the main step (21).

One serious trouble that has occurred with flexible hulls has been caused by attaching too rigid an outer or hydroplaning bottom to the flexible body of the hull. As might have been expected, this has repeatedly resulted in local failure of the outer and rigid bottom in the neighbourhood of the main step. There seems to be no reason why this trouble should not be overcome.

It should not be forgotten in connection with hull design that not only upward but also downward forces are experienced. Bottom suction occurs when a craft gets off at a low water speed, owing to meeting a head-on gust for instance. It probably occurs locally under quite normal conditions, but cases are not unknown in which a seaplane has got off and left both float bottoms on the water (18).

Primary failure outwards, as described above, has not occurred, within the

writer's experience, to a flying boat; probably the vee bottom which nearly always forms a feature of boat rather than float design is advantageous in this respect.

An interesting summary of the merits of flat and vee bottoms is that the first gives a quick take-off and consequently a lessened risk of damage during the process. A vee-bottomed hull with its greater water drag gives a longer get-away run but a much softer landing (3*c*, 3*d*). Another disadvantage attendant on the vee bottom is that it throws up more spray than does a flat bottom, and the steeper the vee, the higher the spray (3).

It is now purposed to discuss the varieties of marine aircraft in common use.

Twin-Float Seaplanes

The twin-float type of seaplane is, in its present stage of development, little more than a single-engine tractor aeroplane on floats. This assertion has been contradicted by seaplane designers, who, perhaps, wish to claim more than a fair share of specialist knowledge, but its truth is made apparent by the fact that some firms offer essentially identical aerostructures with land or sea undercarriages to suit, and even as interchangeable components.

The outlines of the well-known Short and Fairey types are very familiar and need no description. The single-engined pusher type of twin-float seaplane seems to have disappeared altogether with the similar type of land machine, probably for identical reasons of poorer performance and greater vulnerability to attack from the rear than the tractor type.

In the tractor type the propeller disc is usually some six inches above the top of the floats at its lowest point, but between them. When taxying with the tail up, some two feet of clearance from the nominal water line is all that obtains, and it is obvious that damage from spray and from green water hitting the propeller must certainly and does in fact occur when the height of the waves approaches the amount of nominal clearance (4). This disadvantage carried with it the attendant propensity for the propeller to throw up spray, which is carried back and soaks the crew in the fuselage.

Owing principally to racking in both transverse and fore and aft directions, it has been found necessary to design the undercarriages of twin-float seaplanes with greater normal load factors than land machine undercarriages, and failure of the undercarriages or floats is usually literally a one-sided affair. The disadvantage of unsymmetrical float loading, of secondary importance in a small single-engined twin-float seaplane with both floats strutted to each other and to a common fuselage, becomes more serious when the larger twin-engined type with an engine over each float is considered. Then the two buoyant structures will be of such distance apart as to make the interconnecting structure of prohibitive weight, if of adequate strength. This disadvantage has latterly been assumed to outweigh the many attractions of the types from points of view other than that of seaworthiness. A comparatively recent and very large type of twin-float seaplane was the four-engine Zeppelin Staaken biplane. It is understood that one of the floats of this machine pulled off while taxying. Various British firms constructed twin-engined and fuselaged seaplanes during the war, but none of these was considered successful.

Twin-floated seaplanes may be divided into two main classes, according to whether they have tail floats or not. The tail-floated class may be subdivided into varieties differing by the extent to which the tail float is necessary in maintaining longitudinal stability afloat. Some have the main floats with sufficient buoyancy astern to keep the tail float clear under ordinary conditions; in others the tail float is partially submerged until hydroplaning commences (25).

This last variety possesses two distinct advantages which both spring from the tail down trim taken up at rest and not greatly departed from during slow-

speed taxying. The greater freeboard and raked-up bottom forward renders the floats less liable to dig into green water and a considerable propeller clearance is maintained.

The class without a tail float but with long main floats possessing buoyancy abaft the main step may have two or more steps in each float. As a rule, however, the after-bottom is raked up and performs no hydroplaning duty. Little change of trim occurs over the whole range of water speeds, the craft always maintaining a tail-up attitude, so that the tail, more especially the elevators, is kept free from water damage, the liability to which forms what is perhaps the principal weakness of the tail-floated type.

There does not appear to be much demand for a two-stepped float seaplane having positive water stability when hydroplaning. The magnitude of both air and water forces is much less than with a large flying boat, and the sensitivity to air control greater, so that a neutral stability characteristic may even be an advantage.

There are one or two other points to consider when comparing the merits of the tail-floated, tail-down class with those of the long-floated, tail-up class. When drifting head to wind, the former presents its wings at a large angle of incidence, and the centre of gravity is well abaft the centre of buoyancy of the main floats, so that gravitational and wind forces combine to subject the tail float to a considerable live load which stresses the fuselage somewhat severely in anything of a lop (*22e*, 25).

It is sometimes argued that the long-floated type may be dangerous to land in a short sea because the after-bottom may make contact with the receding face of a wave while the main step is still clear of the trough (25). The inferred result is that the bows would dig into the face of the approaching wave. This point is weakened by the fact that a liability to perform such a feat is present in the short-floated type because it is necessary to keep the main step well aft in order to lift the tail prior to hydroplaning.

Two very real advantages of the tail-floated type are, firstly, that it is easier to handle on the slipway, as the after-end of the fuselage is more readily manhandled, and secondly, it is easy to fit a water rudder below the air rudder without complication of controls.

In the twin-float seaplane, the lower plane and cockpits are usually higher from the water than in the single-float seaplane or flying boat of similar size; but even when wing-tip floats are fitted, the wing tips and ailerons of the bottom plane often suffer when taxying.

Owing to the fact that their method of attachment permits springing, and that they are not used for the disposal of crew, the floats of a seaplane may be rigidly bulkheaded, thus giving a considerable factor against total loss.

While causing yawing to an extent which prevents getting off, partial loss of buoyancy in one of the main floats, or even complete loss of buoyancy in one float if the wing-tip floats are of sufficient size, will not cause further flooding, but serious damage to the tail float, or to the after-compartments of both main floats in the type without or with too small a tail float, might easily cause the craft to capsize stern foremost. Damage in this way is, however, comparatively unlikely to occur since these parts are, except in the case of floats which terminate with the main step, free from water shocks at high speeds.

From reasons of the small amount of freeboard forward, and the comparatively high centre of gravity, a seaplane is very liable to dig in the nose of the floats and to capsize forwards if flattening out previous to alighting is delayed a fraction too long. This may also happen if porpoising is allowed to become too violent. The pitching forward tendency of a float seaplane on landing due to the resultant force from the float bottoms passing abaft the centre of gravity used occasionally to result in collapse of the foremost undercarriage struts.

One of the most conspicuous disabilities of the ordinary twin-float single-engined seaplane is the difficulty of directional control when taxying at slow speeds at which the air controls are ineffective. A water rudder attached to the tail float or one attached to the stern of each main float should always be fitted; but even so the control is poor in a strong wind, into which the seaplane tries to weathercock. The steering trouble is fundamental and is due to the necessity of actually retarding one float and accelerating the other on a turn (14).

Single-Float Seaplanes

The type of seaplane having a central main float and maintaining lateral stability when afloat by means of wing-tip floats in the same manner as does a flying boat has never been developed in this country to any extent, and the only British representative of the type at present is a tractor machine fitted with amphibian gear. Unfortunately this craft was designed to a particularly stringent specification in the matter of overall dimensions, and therefore can only be compared to other types designed to the same specification, which was such as to make seaworthy qualities practically unattainable.

With a tractor design it is difficult to provide at once sufficient freeboard and propeller disc clearance forward. The question of propeller clearance is complicated by springing the main float—a very desirable feature with a flat-bottomed float. Springing may be avoided, however, by fitting a vee-bottomed float. When this step is taken three further disadvantages appear—firstly, the get-away run will be longer; secondly, it will not be safe to run the craft up on to a slipway (25); thirdly, the bow wave with its spray will be thrown higher.

With a single central instead of twin floats the propeller is certainly better protected from green water, but with a vee-bottomed float the spray is caught by the slipstream and flung back over the top plane into the fuselage cockpits.

The amphibian referred to earlier has a rounded bottom, of which the keel is very little lower than the chines. While certainly uncomfortably wet, this craft has a remarkably short get-away run.

Considering the main float alone, the metacentric height is negative and wing-tip floats are necessary. The lower plane has not therefore with convenience so great a lower plane clearance as a twin-floated craft.

An important difference between the rolling of the single as compared to that of the twin-floated seaplane is that in the first case the roll is more positively damped at the outset by the wing-tip floats, and therefore the craft does not acquire as great an angular momentum before the wing tips reach the water, whereas if the wing tips of a twin-floated seaplane meet the water, the contact is probably violent.

The alteration in trim resulting from a partial loss of buoyancy in the main float, which may be conveniently bulkheaded, would not be as a rule serious, but the loss of a wing-tip float would probably cause the craft to capsize, but not necessarily to sink, after the lower wing on that side had become waterlogged.

The pusher variety of the type has attracted little attention here, but in this case the propeller would be even more free from water than that of the tractor machine, and the crew in a drier position.

In other respects, the single-float seaplane resembles the twin-floated craft in seaworthiness characteristics, except in regard to steering when taxying. The single-floated machine here possesses a definite advantage owing to narrowness of track, provided a good water rudder is fitted, and the wing-tip floats are not carrying too much weight.

Single-Engined Flying Boats

From the point of view of propeller protection the single-engined flying boat is better off than the single-float seaplane owing to the hull of the boat being used for the disposal of the crew and other load, and therefore conveniently larger than the float of the seaplane. The crew, usually in open cockpits in the nose, are, however, worse off.

In regard to lower plane position and wing-tip floats, the type is in similar case to the single-float seaplane (15); but in regard to the tail unit, the proximity of this to the water line when carried as usual on the after-end of the hull is a serious defect.

Owing to the fact that most of the load is carried in the hull, no special shock-absorbing apparatus is fitted, and some successful examples of the type have embodied a flexible hull, to which the provision of suitable bulkheads is a difficulty which is receiving attention. At present, however, protection from total loss of buoyancy due to the hull being holed is provided only by means of a double bottom forward of the main step, so that flooding will only occur if both bottoms are pierced (2c).

The loss of lateral stability consequent to loss of or serious damage to one wing-tip float may result in total loss, owing to the hull being readily flooded through the open cockpits, so that if the machine capsizes it will almost certainly sink.

The question of whether to make a single-engined flying boat a tractor or a pusher has usually been decided from consideration of engine position with regard to centre of gravity, and the disposal of the crew in the nose of the hull has usually resulted in a pusher design which possesses a slight seaworthiness advantage in better propeller protection.

There are one or two instances of the type having been fitted with booms, as those of a pusher aeroplane, for carrying the tail unit. This has the result of enabling the tail to be kept higher from the water and of shortening the hull (19, 20). The saving in weight of the hull is, however, more than counter-balanced by the additional weight and aerodynamic resistance of the tail booms, so that this version of the type, though probably more seaworthy, has never become popular.

The signal weakness of the small flying boat is the wetness of the cockpits in any but the calmest weather. It is principally this characteristic which has made the type less popular than the twin-floated seaplane of similar overall dimensions.

The Twin-Engined Flying Boat

Of the various types of marine aircraft with which experience was gained during the war, the most seaworthy proved to be the Curtiss or America type of twin-tractor biplane flying boat, which later developed into the existing F.5, which may be taken as an excellent and representative design.

Originally the type was single-stepped and possessed a slab-sided hull (1). The earliest actually used on this side of the Atlantic, however, whilst retaining the single step, had watertight fins added to shorten the run to take off by increasing the hydroplaning area. The hull skeleton of the F.5 forms a rigid steel-clipped wooden frame. The single skin is built up for the most part of two layers of mahogany, but this is reduced to a single layer wherever possible. Watertight compartments are provided in the fins, and half-bulkheads at intervals athwart the hull rise to a height above the load water line, and while not interfering with intercommunication, these fulfil a limited service in providing against flooding. The sharpness of the vee bottom of the early Americas was increased

in later developments to reduce water shocks, and the original single step was replaced by the tandem arrangement described in the introduction.

A sharp vee bottom has, however, the result of increasing the length of the get-away run by increasing the resistance. This may readily be understood by regarding the hydroplaning drag as proportional to actually wetted area (2). Also, the sharper the vee the higher rises the spray thrown up by the blister.

The tail unit is mounted on the after end of the hull, and the single tail plane is carried some six or seven feet above the water line by means of a system of struts and the vertical fin. The engines are mounted rather higher than midway between the planes.

The propeller discs are forward of the leading edge of the lower plane, and extend slightly below it, so that during taxying they pick up a certain amount of spray from the fringe of the blister thrown up by the vee-bottomed hull (3*a*, 9). Serious damage to them is unusual but occurs sometimes (3*c*). The cockpits in the nose do not take in spray in a swell or in small waves, except when taxying with a beam wind, and when lying to it needs a very big breaking sea to swamp the hull.

The most conspicuous disability of the F.5 is the fact that the hull is unstable fore and aft when hydroplaning and the oscillations cannot wholly be checked by use of the air controls. The pilot is thus unable to hold the machine at the most favourable attitude for taking off.

Towards the end of the war a modified type of two-engined flying boat was produced in the Phœnix Cork or P.5. The hull of this type was designed by the late Mr. Linton Hope, and is of flexible construction. The hull volume is much less than that of the F.5, and this is the principal reason why the type is wetter when carrying a heavy load (16). With light loads the P.5 hull behaves as well or better than the older type (2). A double-bottom forward is used, and the main step is formed where the outer bottom is discontinued. That part of the hull included by the two bottoms is sometimes called the "step" (1), and is formed into fins at the sides, but fins which are much smaller than F.5 fins so that the hydroplaning area is also much smaller. The lines of this type of hull were a result of model research, and it possesses considerable advantages over the F type in regard to longitudinal stability under way. In addition, its flexibility enables it to withstand greater water shocks without failure. Its dirtiness with heavy loads is probably due to the water line meeting the bows high up where the buttock lines are bluff, and to insufficient freeboard.

The P.5 runs at moderate speeds with the water in contact with both steps and with the bottom between them. The tail step is vee-sectioned to lie in the furrow left by the main step, and running at a small but positive angle of incidence gives a slight lift which produces a moment balancing the stalling moments due to the lift of the planing bottom forward of the main step and to the slight suction which occurs between the steps (3).

In regard to wing-tip floats and tail units, the F and P types are very alike. The tails are carried so high as to be practically free from water shocks in ordinary weather, though this is done at the expense of a clean and economical structure. The wing-tip floats are bracketed to the lower plane spars at the outer interplane struts, and put stresses in these members which may be considerably greater than the flying stresses.

Repeated water shocks on the lower plane and transmission of acceleration and retardation forces between the wing, engine and hull masses have been followed by failure of the wing attachments to the hull in cases where a twin-engined flying boat has had to taxy in a heavy sea. The present tendency towards fuel disposal in the wings would be disadvantageous in this respect.

Watertight compartments are provided in the fins of the F type and the

double bottom of the P type boats, but bulkheads in the ordinary sense are not fitted for several reasons, chiefly of interfering with intercommunication, and in the case of flexible hulls, of a suitable design of panting bulkhead not being available.

Since the end of the war two British firms have produced twin-engined flying boats larger than the F and P types. In one case the firm concerned was without previous experience of marine aircraft, and F and P boat practice was departed from with unfortunate results, in that the first two of the type both crashed shortly after completion. The bottom of the first failed forward of the main step. It is thought that the second damaged its tail when taxying, and a defect was thereby occasioned which developed in flight and caused loss of control. Damage so caused had previously occurred to the first of the type, but not to any serious degree.

Only one example was built by the other firm concerned, and this met with some success. It did not, however, represent any great advance, beyond increase of size, over the F.5 type.

In each of the above cases the rigid type of hull construction was retained.

The writer has little information relative to the capsizing of the larger types of flying boats, and it is believed that the rare cases of total loss have been by settling in a more or less normal attitude.

A water rudder is not nearly so essential a fitting on a twin as on a single-engined machine, as the engines may be used for turning in the former case. But the necessity of opening up one engine may be undesirable for several reasons —the acceleration of one wing tip against a heavy sea might be mentioned—so that in any case a water rudder is nearly indispensable.

In conclusion, there is one point of immediate interest in connection with the use of the F type flying boats for anti-submarine patrols during the war by the Felixstowe squadron. The seaworthy qualities of this design are such that not a single life was lost owing to any fault in this respect (3*d*, 22*a*).

Multi-Engined Flying Boats

Multi-engined flying boats first appeared here with the Porte baby—a three-engined machine. It was not a great success in the air chiefly owing to heaviness and sluggishness of control, but handled comparatively well on the water. On one occasion one of these craft was taxied across the North Sea from the Belgian coast to Felixstowe without suffering damage of any sort (22*b*). It was followed by the Porte Super Baby or Felixstowe Fury, the latter being the official name. This boat had five Rolls Royce Eagle 8 engines, and was a triplane with the tail carried on the stern of the very broad hull. Wing-tip floats were carried on the bottom plane as usual. It was found inadvisable to carry out heavy load trials with the Fury owing to the fact that she hydroplaned too easily and was too liable to be thrown into the air at less than her minimum flying speed. This was probably the indirect reason why she finally crashed. In any case, it is indicated that if it is necessary to use a very broad hull, then means must be taken to render some of the potential hydroplaning area non-effective (26).

The only German examples of the type of which details are available were made by the Dornier firm, and were very nearly "all-metal" machines. It may be of interest to note here that, on the authority of a statement made to the I.A.A.C.C., 100 tons of nickel steel was used for the wing spars, and the wing weight per square foot was of the order of 1 lb.—a very low figure for such large craft. This data should, however, not be accepted without verification.

The first Dornier boat was the R.1, a three-engined pusher with Warren girder interplane trussing. The tail plane and elevator were mounted on the hull

with only a small water clearance, and probably for this, among other reasons, this experimental boat was not a success. In other respects the craft was not dissimilar to the America type (17*a*).

The second Dornier, the R.2, embodied the lessons learnt from the R.1. The necessity for wing-tip floats was avoided by giving great beam to the hull, which contained three engines driving three propellers through gearing. This latter arrangement was replaced in the R.2a by tandem pairs of engines in nacelles with directly-driven propellers. The top plane was of considerable span, but that of the lower plane had been cut down to equal about half that of the top.

The R.3, the next development, differed from the R.2 chiefly in having the tail booms replaced by a fuselage carried above the single plane—a considerably cleaner form of design. The tandem power units were retained, but the small lower plane was dispensed with.

The fourth Dornier multi-engined boat was generally similar to the third, but the lines of the hull were different, and the R.4 had elementary bottom wings on the water line providing buoyancy outboard for assisting lateral stability.

The R.1 and R. 2 had cut-water entries to their hulls resembling those of the Norman Thompson boats here. In the R.3 the sharp but nearly vertical stem was discarded, and in the R.4 the bows were further modified to a form not very different to that of the floats of the familiar Short and Fairey seaplanes (17*e*).

The American three and four-engined N.C. boats had a wing structure resembling that of the F type, but carried their tails on booms. This latter measure seems to have been taken without full realisation of its possibilities, as biplane tails were fitted with their lower portions little better off in regard to water clearance than if they had been mounted directly on top of a longer hull. No fins were fitted to the vee-bottomed hulls, which had no greater beam than has the F.5, in spite of an increase of gross weight from about 13,000 lbs. to 22,000 lbs.

The bows of the N.C. boats were not bluff like those of the F boats, but had vertical stems. The forefoot, however, was above the l.w.l., so the vertical stem could have little effect on the running.

A rigid form of construction was adopted for the N.C. boats. Taken in order from the bottom up, the following members took the load consecutively:—

(*a*) 45deg. outer skin.
(*b*) Thwartships inner skin.
(*c*) Longitudinals.
(*d*) Bulkheads.
(*e*) Keel, chines, etc.
(*f*) Wing spars and struts.

The Hydrovane

From time to time experiments have been carried out with submerged cambered planes, usually called hydrovanes, instead of using the hull bottom as a planing surface (4). Work on this problem was done by M. Forlanini, M. Santos Dumont and Commander Burney with the Bristol Co., before the war, but it is believed only the last of the three named applied the principle to aircraft. In some ways the suggestion is attractive. For instance, the landing shocks might be restricted to a few members specially designed to take them instead of being spread over a large area as at present, because the necessary hydrovane area is very small. One of the difficulties experienced with large flying boats is getting off in a long swell (3*c*). After the boat has passed over the crest of a wave it tends to leave the water even if flying speed has not been attained. Then the water lift being lost the boat pancakes down again. There are certain arrange-

ments of hydrovanes which should prevent this trouble, the planes being superimposed vertically so that they successively leave and enter the water. In other words, the lift obtained from the planing bottom of a hull varies too rapidly with vertical movement of the water line. It is possible that a hydrovane system may be designed to give a gradual change of lift over a range of vertical movement of water line of several feet with aircraft not larger than the flying boats in use.

There are, however, many difficulties, the more important of which may be mentioned.

The projection of the hydrovane and their struts below the bottom of the hull prevents the use of the usual trolleys and slipways so that the craft must either remain afloat or the hydrovane must be collapsible in a somewhat similar manner to the wheel gear of amphibian aircraft. Probably the first solution might apply to large and the second to smaller craft.

The problem of maintaining longitudinal stability on the water as well as in the air is complicated by the necessarily high thrust line. By suitably arranging the tail surfaces in the slipstream the flying problem is solved fairly satisfactorily. The hydrovane case does not appear to be so simple. It might be possible to arrange the main hydrovane or vanes so that the mean resultant passes as near as possible to the centre of gravity and to balance the thrust couple by means of a controlled tail hydrovane which could leave the water earlier than the main hydrovane. The final movements of taking off would be carried out by means of the air controls.

Stability in roll whilst taxying is not easy of solution owing to the small area and span of the main hydrovanes. In these circumstances a large dihedral angle on the hydrovanes may have an opposite effect to the dihedral angle on the wings, being equivalent to a vertical fin below the centre of gravity. If the main hydrovane is bisected and a half put under each wing the racking stresses and strains are likely to give trouble.

It is understood that Commander Burney overcame the rolling trouble by fitting vertical rudders below the main hydrovanes which could be controlled to produce a yaw to correct a roll.

Directional stability on the water does not appear to present any difficulties and it is doubtful whether the effect of wind across tide would be any more serious than with existing marine aircraft.

Recent Developments

Post-war versions of flying boats by the Roland and Dornier firms in Germany resemble each other to some extent. In both cases the conventional lower plane with its wing tip floats is dispensed with. In the case of the Roland, as in the later R.2 and R.3 described above, a hull of great breadth is used. In the case of the various Dornier designs the rudimentary lower planes of the R.4 have been retained, but their span is such that they can hardly be regarded as more than exaggerated fins. These craft are said to be remarkably seaworthy (24). In another direction the Roland designer appears to have sought improvement. The freeboard forward is greatly increased so that the pilot in the forward cockpit is well above the bows, which are very bluff in plan form.

A very notable suggestion has recently been put forward and patented by Mr. Manning, who was responsible for the general design of the P.5 flying boat. The proposal resembles the Dornier design in having bottom planes to maintain lateral stability on the water, but there are important differences. The Manning proposal is to construct the stabilising wings with their roots below the waterline, and with a pronounced dihedral angle, so that when the craft heels over on the water the immersion is progressive outwards instead of inwards as with a hori-

zontal plane which will obviously dip its tip first. This modification is to prevent sudden loading at the tip, which stresses the structure severely as well as causing the craft to yaw unduly. It is particularly in alighting that the Manning hull should show its advantages as this is the condition under which the wing tip floats of a flying boat have as a rule failed. The progressive immersion outwards of the Manning bottom plane replaces the suddenness of contact of the wing tip float bottoms as the hull sinks into the water.

The Manning wings in span and profile much more nearly resemble conventional wings than do the Dornier wings, which can contribute little lift to the structure owing to their poor aspect ratio.

Model tests have been carried out and have given promising results. It is believed that the construction of a full-sized machine has been commenced.

Some Details of Design

There are about four distinct types of bow design of hulls and floats with which experience has been gained and this part is most important as far as keeping dry is concerned, whether the type is a float seaplane or a flying boat.

Firstly, there is the square-ended type of which Short and Fairey floats are examples.

Secondly, there is the cut-water entry possessed by the Norman Thompson and Vickers Viking boats.

Thirdly, there is the rounded nose of the F type boats.

Fourthly, there is the type represented by the Phœnix Cork and Supermarine A.D. boats.

The last named firm has, in its single-seater and Seagull designs, produced a bow form with fin modelling apparently based on some of the earlier N.P.L. tank tests. This departure seems to have effected a considerable improvement at low-water speeds.

Looking at the various longitudinal sections or buttock lines of a hull from the centre line to near the chine it appears that the water line should meet the bottom line at the smallest possible angle. If the water meets the bows at a coarse angle, experience has shown no reasonable amount of flair or freeboard will keep the spray from the splashes from reaching the forward cockpits. In addition it appears to be necessary to have breadth and length of forebody and depth of freeboard forward in order to prevent digging in and also breaking seas from pouring into the cockpits (3*d*).

A cut-water entry consisting of a nearly vertical stem does not appear to effect much improvement, at low speeds, to a hull of which the buttock lines are bad, and it seems unnecessary when the buttock lines are good (5), as in any case the forefoot is running clear at hydroplaning speeds. In addition the extra side surface so far forward introduces aerodynamic complications (11).

Briefly, it might be stated that fine buttock lines are more important than fine plan lines. It is the writer's opinion that the Short type of float nose and the "F" type of boat nose are the most seaworthy that have been evolved in their respective classes.

The Manning version of the partially submerged wing in its present form is set rather far forward on the hull, which was found necessary in the model tests, and has only a slight rake aft. For aerodynamic reasons the position of the top plane is fixed abaft that of the lower so that a negative stagger results. This feature has structural advantages since the interplane trussing is nearly parallel to the mean resultant force on the wings in normal flight, but tends to increase autorotation. It is suggested a more comprehensive series of model

tests than have yet been carried out would be very valuable to elucidate the effect of variations of bottom plane dihedral and aspect ratio on the water stability.

From experience gained with existing types of flying boats it appears to be necessary that, for low speeds, the hydroplaning area should be the maximum possible to obtain cleanliness and that, for high speeds, it should be a minimum to prevent the water forces from throwing the machine into the air at less than a safe flying speed (*22d*). Research into the variants in hull design affecting this question is urgently needed. The effect of varying the position of greatest beam might be studied to this end.

The question of the disposal of fuel is rendered difficult because the present trend of land machine design, which is to put the petrol in the top plane and use gravity feed, is unfavoured towards stability on the water in the case of marine aircraft. Probably in the majority of cases the hull position will be used.

It is perhaps desirable to emphasise the need for water rudders on all marine aircraft. They are particularly useful when approaching a slipway or in crowded waters. The mounting of a water rudder should be such that there is little risk of its being damaged when the craft is handled on the slipway, a trouble which has often occurred in the past. In amphibians a combined steering tail skid and water rudder is sometimes fitted. It is perhaps better to approach the design of such a fitting with the idea of making a really efficient steering tail skid and then to add the necessary surface to make it effective as a water rudder.

A neglected point in past designs has been the lack of provision for bilgeing wing-tip and tail floats if such are fitted (*22h*). There are mechanical problems to be solved, but the lack is severely felt when the parts referred to become waterlogged, not necessarily owing to being holed, but because of repeated water shocks over a long period causing small leaks.

Another very important point is the desirability of having ailerons on the top plane only. This applies to all types with the questionable exception of the twin-float seaplane. It should be remembered that damage to a lower plane aileron or its attachments which has occurred previous to taking off may become apparent in flight by the locking of the control with possibly grave results.

A serious risk attendant upon the use of a bulkheadless hull, or one with longitudinal or keelson bulkheads only, is that of bilge water running from one end to the other as the incidence of the machine changes in flight, with dangerous effects on control. As even a half-bulkhead, say one foot deep, would interfere with hull flexibility, there is here a problem of which the satisfactory solution would be very valuable in view of the increasing popularity of the flexible hull. Apart from the type of hull construction it is perhaps desirable to provide water-tight compartments in the extreme nose and tail of all kinds of craft as the various objections to the use of bulkheads generally apparently do not apply to these parts, which may provide a convenient reserve of buoyancy capable of maintaining something above water as the last resource of the crew after a crash.

Inferences and Speculations

There are certain conclusions to be drawn from the history of marine aircraft, as will be realised from the foregoing brief account of recent developments.

It would appear to be necessary to protect the propeller from green water, especially in smaller craft, by placing it immediately above a main float or boat. For this reason alone the twin-float single-engined seaplane is unsatisfactory as a type when seaworthiness is important. It must be remembered in this connection that it is not only the propeller itself that suffers from the blades hitting water. The shocks to the engine components and mounting are also severe (*3c*). Metal propellers may well be found to possess increased durability, but their comparative lack of elasticity may be found disadvantageous. Theoretical con-

siderations will show, in addition, the harmful results of the increased weight of metal propellers, particularly in regard to gyroscopic couple. This is not only an aerodynamic question, since the rapid angular variation of the propeller shaft axis during porpoising may cause high stresses.

Considering the two remaining single-engined types, the flying boat and the single-float seaplane, it appears that a desirable flying boat should carry its tail on one or more fuselages, the only economical structure to carry it sufficiently clear of the water (*3a*, *3c*, *22f*). In this case the separate nomenclature as to flying boat and seaplane really refers to the disposition of crew, which is generally settled from considerations other than that of seaworthiness. Whether the single-engined flying boat, with part at least of its crew in the hull, should be a single-fuselaged tractor or a necessarily twin-fuselaged or outrigger pusher, must be decided in favour of the pusher type when possible, as in this type the bows can conveniently have greater freeboard as the question of propeller clearance does not arise there, and freeboard aft is of less importance. But, nevertheless, questions of structure and disposition of crew may result in a single-fuselaged tractor being decided upon.

It appears likely that some version of the Dornier-Manning partially submerged boat-built wing or an abnormally broad hull may eventually replace the conventional structure with its wing-tip floats. It should be clearly understood that it is not only that wing-tip floats are themselves undesirable as the advantages, in any case, of boat building the bottom plane that makes this development so important (*22c*, *22f*).

It is possible that the final solution of the lateral stability problem will be in the form of a hull of which the metacentric height is just positive, but with the addition of small emergency wing tip floats which will come into use occasionally, when turning in a strong wind, for instance. Normally both of these would be well clear of the water.

Much larger types of single-engined flying boats should become practicable with the engines now available, such as the Rolls-Royce Condor and the Napier Cub, and a single-engined pusher flying boat to carry the service load of an F.5 or P.5 might make a very attractive machine (*26a*).

An advantage possessed over twin-engined types generally which has not hitherto been mentioned is the smaller lateral moment of inertia of the latter, so that the craft heels more readily with the sea with a reduction in impact stresses.

Bibliography

Ref. No. Authority.

1. Nicholson, "Flying Boat Construction," Aeronautical Journal, August, 1921.
1a. Low, Remarks during discussion of above.
2. Linton Hope, "Notes on Flying Boat Hulls," Aeronautical Journal, August, 1920.
2a. Wiley, Remarks during discussion of above.
2b. Wright, Remarks during discussion of above.
2c. Nicholson, Remarks during discussion of above.
2d. Linton Hope, Data, Aeronautical Journal, September, 1920.
3. Baker, "Flying Boats," Proceedings of North-East Coast Institution of Engineers and Shipbuilders.
3a. Farren, Remarks during discussion of above.
3b. Vernon, Remarks during discussion of above.
3c. Samson, Remarks during discussion of above.
3d. Rennie, Remarks during discussion of above.
3e. Pippard, Remarks during discussion of above.
4. Aeronautical Research Committee, R. & M. 69.
5. Aeronautical Research Committee, R. & M. 70.
6. Aeronautical Research Committee, R. & M. 98.

7. Aeronautical Research Committee, R & M. 165.
8. Aeronautical Research Committee, R. & M. 166.
9. Aeronautical Research Committee, R. & M. 300.
10. Aeronautical Research Committee, R. & M. 410.
11. Aeronautical Research Committee, R. & M. 412.
12. Aeronautical Research Committee, R. & M. 473.
13. Aeronautical Research Committee, R. & M. 655.
14. Isle of Grain Report XI., M. 174.
15. Isle of Grain Report XI., M. 201.
16. Isle of Grain Report XI., M. 219B.
17*a*. Flugsport, 1920, page 372.
17*b*. Flugsport, 1919, page 442.
17*c*. Flugsport, 1919, page 504.
17*d*. Flugsport, 1921, page 476.
18. Richardson, "Aeroplane and Seaplane Engineering," "Aerial Age Weekly," 1918-19, page 1180.
19. Isle of Grain Report XI., M. 132A.
20. Isle of Grain Report XI., M. 149.
21. Isle of Grain Report XI., M. 220.
22*a*. P.I.X., "The Spider Web," page 4.
22*b*. P.I.X., "The Spider Web," page 25.
22*c*. P.I.X., "The Spider Web," page 73.
22*d*. P.I.X., "The Spider Web," page 89.
22*e*. P.I.X., "The Spider Web," page 98.
22*f*. P.I.X., "The Spider Web," page 105.
22*g*. P.I.X., "The Spider Web," page 106.
22*h*. P.I.X., "The Spider Web," page 170.
23. Laws, "Stability and Equilibrium of Floating Bodies."
24. "Aeronautics," May 5th, 1921.
25. Fairey, "Seaplanes," Air Conference, 1923.
26. Cave-Browne-Cave, "Practical Aspects of the Seaplane," AERONAUTICAL JOURNAL, June, 1923.
26*a*. Maycock, Remarks during discussion of above.

THE LAUNCHING OF AIRCRAFT FROM SHIPS

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As it may be necessary in the future, as it has been in the past, to launch aircraft from ships, other than aircraft carriers with large aerodrome decks, the following investigation has been carried out.

It is required to find the minimum propeller thrust necessary to enable an aircraft (particularly a seaplane or flying boat) of known weight and fineness, to take off from a railway platform, on a ship steaming at given speed.

The following assumptions are made:—

(1) The air is supposed still relative to the earth. The wind velocity relative to the ship is then due entirely to the speed of the ship.

(2) The aircraft is assumed to move along the platform at a constant attitude. The attitude taken is that for the best climbing rate.

(3) The resistance of the aircraft is taken as equal to Av^2 where A is constant for the attitude at best climbing rate.

(4) It is assumed that the aircraft shall take off by running along a short railway track. This has been done because it is thought that some form of railway track would be preferable to a staging on small naval vessels, and also on the forecastle of mail steamers.

The arrangements of the trolley wheels may vary, but it has been assumed that the resistance due to friction on the railway is equal to Bv where B is a constant.

(5) The propeller thrust has been assumed constant for the whole length of the run. It is therefore necessary to suppose that the engine has been opened up to full power before allowing the aircraft to move along the railway. This assumption is permissible owing to the very small diminution in thrust up to the speed of taking off.

Let (v) ft./sec. = aircraft speed relative air.

Let (V) ft./sec. = speed of ship relative to the earth.

Let (T) lbs. = thrust of airscrew.

The railway friction is zero when $v = V$, that is when the aircraft is stationary relative to the ship.

The force tending to accelerate the aircraft is therefore: (Thrust)—(railway friction)—(air resistance on aircraft)

$$= T - B(v - V) - Av^2$$
$$= (T + BV) - Bv - Av^2$$

As V, the speed of the ship, will be constant during the time taken to take off, then $(T + BV)$ will be constant $= T_1$ say.

We have then

$$T_1 - Bv - Av^2 = (m/g)(dv/dt) = (m/g)(dv/dx)(dx/dt) = (m/g)(v)(dv/dx)$$

where x is the length of run relative to the earth and m is the weight of the aircraft.

Whence

$$dx = (m/g)\{v/(T_1 - Bv - Av^2)\}\,dv$$

Integrating, we get

$$x = -(m/g)\int [\{B/2A + 1/2A(-B - 2Av)\}/(T_1 - Bv - Av^2)]\,dv$$

$$= -(m/g)\left[(B/2A)\int dv/(T_1 - Bv - Av^2) + (1/2A)\int \{(-B - 2Av)/(T_1 - Bv - Av^2)\}\,dv\right]$$

$$= -(m/g)\left[(B/2A^2)\int \{dv/-(-T_1/A + Bv/A + v^2) + (1/2A)\log_e(T_1 - Bv - Av^2)\}\right]$$

which we shall call equation (1).

To integrate the term

$$\int dv/-(-T_1/A + Bv/A + v^2) = \int dv/-\{(B/2A + v)^2 - (B^2/4A^2 + T_1/A)\}$$

$$= \int dv/-\{(\alpha + v)^2 - \beta^2\}$$

where

$$\alpha = B/2A \text{ and } \beta = \sqrt{(B^2/4A^2 + T_1/A)}$$

and since $(\alpha + v)^2$ is less than β^2

$$= (1/2\beta)\log_e(\beta + \alpha + v)/(\beta - \alpha - v)$$

Substituting in (1) we get

$$x = -(mB/4gA^2\beta)\log_e(\beta + \alpha + v)/(\beta - \alpha - v)$$
$$- (m/2gA)\log_e(T_1 - Bv - Av^2) + C$$

which we shall call equation (2), where C is a constant of integration.

When $x = 0$, that is, at the instant of starting, $v = V$, and substituting in (2) we get

$$C = (mB/4gA^2\beta)\log_e(\beta + \alpha + V)/(\beta - \alpha - V$$
$$+ (m/2gA)\log_e(T_1 - BV - AV^2)$$

which gives a value of C for the particular speed of ship during the take off.

By substituting the numerical values in (2), the length of run (x) relative to the earth, to attain the minimum flying speed v can be found.

It is now necessary to find the time required to attain minimum flying speed and this is done as follows:—

Taking the force equation as before,

$$T_1 - Bv - Av^2 = (m/g)(dv/dt)$$

whence

$$dt = (m/g)\,dv/(T_1 - Bv - Av^2)$$

Integrating,

$$t = (m/g)\int dv/(T_1 - Bv - Av^2)$$

by the same method as before

$$t = (m/2gA\beta)\log_e(\beta + \alpha + v)/(\beta - \alpha - v) + C_1 \quad . \quad . \quad (3)$$

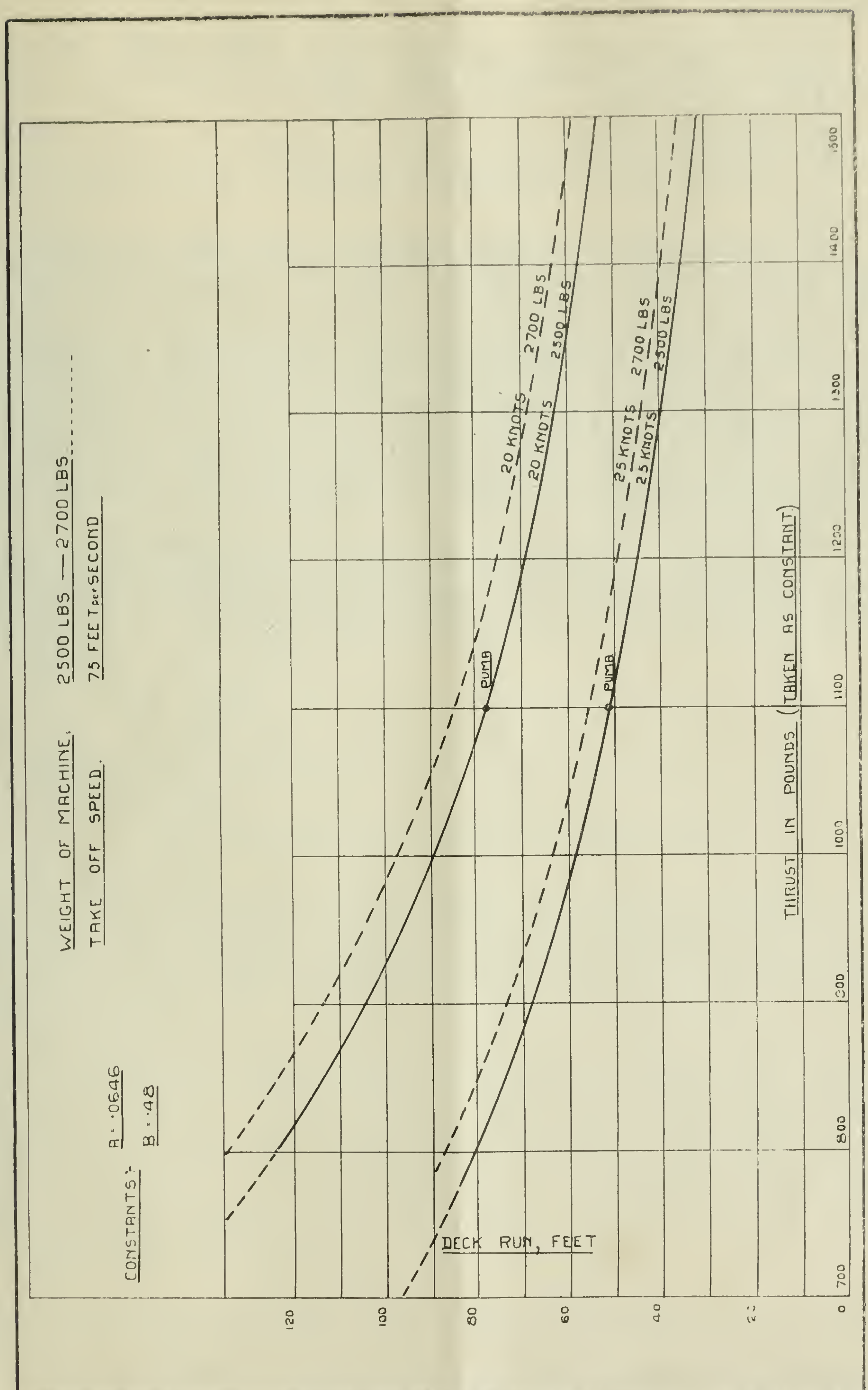
where α and β have the same values as previously and where C_1 is a constant of integration.

When $t = 0$, that is, at the instant of starting, $v = V$ and by substitution in (3), we get

$$C_1 = -(m/2gA\beta)\log_e(\beta + \alpha + V)/(\beta - \alpha - V)$$

which gives the value of C_1 for the particular speed of the ship during the take off.

While the aircraft is running on the railway the ship will be moving forward with speed V. The distance moved by the ship, relative to earth during the


WEIGHT OF MACHINE. 2500 LBS — 2700 LBS
TAKE OFF SPEED. 75 FEET per SECOND
CONSTANTS:- A = ·0646
B = ·48
20 KNOTS — 2700 LBS
20 KNOTS 2500 LBS
25 KNOTS — 2700 LBS
25 KNOTS 2500 LBS
PUMB
PUMB
DECK RUN, FEET
THRUST IN POUNDS (TAKEN AS CONSTANT)
700
800
900
1000
1100
1200
1300
1400
1500
0
40
60
80
100
120

take off will be Vt and it is then obvious that the length of run along the railway will be $x - Vt$.

These calculations have been performed for a number of ship speeds and for aircraft of a number of different weights. Appropriate values of the constants A and B have been taken.

The minimum flying speed was taken as 75 ft. per sec. and was assumed constant for small variation in total weight of aircraft.

The curves emphasise the pronounced advantage of a speedy ship for such a scheme.

Between the limits investigated an increase of ship speed of one knot will permit the total weight to be increased by 200lbs. for the same length of run in taking off.

Using these curves in conjunction with curves of propeller thrust it is possible to ascertain readily the feasibility of using any particular engine in an aircraft intended for this purpose. For instance, having estimated that a Puma engine could be installed for a total weight all up of 2,500lbs. and that a suitable propeller is obtainable which would give a thrust of 1,100lbs. at a take off speed of 75 ft. per sec. it is then seen by reference to the curves that the deck run will be about 52 feet when the ship's speed is 25 knots, and about 77 feet when the ship's speed is 20 knots.

Yet another use of these equations is in the design of catapult launching gear from stationary or moving ships. It is possible to calculate directly the thrust of the catapult gear necessary to augment the engine thrust in order that the taking off speed may be attained in a run of given length.

THE EFFECT OF VARIABLE GEARING ON AEROPLANE PERFORMANCE

BY ANNIE D. BETTS, B.SC.

It has been shown that considerable improvement may be made in the performance of an aeroplane by the use of an airscrew, the pitch of which can be varied. The matter was discussed by Mr. H. A. Mettam in R. and M. 577, Part II.* There are, however, certain practical drawbacks to the use of a variable pitch airscrew. Such an airscrew is expensive to construct and probably more liable to breakage or derangement in flight than the ordinary type. It also imposes on the pilot the necessity of making constant and accurate use of the variable pitch control, if the full advantages of the system are to be obtained.

The present writer considered, therefore, that it might be worth while to inquire whether a similar improvement in performance could not be obtained by other and simpler means, namely, by the use of variable gearing between the engine and airscrew (the latter being of the usual fixed pitch type). The calculations have been made for the same machine as was used to illustrate R. and M. 577, the S.E.5 with Wolseley Viper engine. As in the variable pitch airscrew calculations, the weight has been increased from 1,976lbs. to 2,000lbs., thus allowing 24lbs. for the weight of the gearing.

The data are therefore :—

$W = 2{,}000$lbs.

$S = 248$ sq. ft.

For the airscrew we have :—

$D = 7.87$ft.

P the experimental mean pitch of no thrust $= 7.6$ft.

$\eta = 0.70$.

$K_t = 12.4$ and $K_q = 96.0$. (These quantities are such that $K_t k_T$ and $K_q k_Q$ both $= 1.0$ when $V/nP_m = 0.5$. See R. and M. 474, Appendix.) Curves for k_L and k_L/k_D, b.h.p. and r.p.m., engine b.h.p. factor $f(\sigma)$ and height, air density σ and height, and the airscrew characteristics K_t and K_Q and V/nP_m will be found in R. and M. 577, which should be consulted by those desiring to verify the results following.

Assuming that infinitely variable gearing is at our disposal, the performance can be calculated by a method similar to that used in R. and M. 577. Guess a number of values of R (the gear ratio), and thence calculate the forward speed and best rate of climb, on the assumption that the r.p.m., and consequently the horse-power, are kept constant at the values 1,900 and 212 respectively. (Compare R. and M. 577.) As there is certain to be a loss of power by friction, due to the gearing, it has been assumed that this loss amounts to 5 per cent. of the total, so that the effective power at 1,900 r.p.m. is 201.5.

The formula of R. and M. 474,

$$k_Q = 550 \text{ b.h.p. } f(\sigma)/2\pi\rho n^3 R^3 D^5$$

where $f(\sigma)$ = engine factor and ρ = air density, n = revs./sec. of the engine,

* Empirical Formulæ for a Variable Pitch Airscrew, with Applications to the Prediction of Aeroplane Performance. By Miss A. D. Betts, B.Sc., and H. A. Mettam. February, 1919.

R = gear ratio and D = airscrew diameter, is made use of to find the values of k_Q corresponding to given values of R. With our data this formula becomes:—

$$k_Q = (0.00775 \times f(\sigma)/R^3 \times \sigma)$$

where σ is relative air density.

Two examples only of the working are given here, for economy of space; the results for 6,500, 10,000 and 15,000ft. have also been worked out and are given in Table I.

2,000ft.

R	$K_q k_Q$	V/nRP_m	V ft./sec.	k_L	k_L/k_D	η/η_{max}	η	$V=\eta(k_L/k_D) \times 53.0$	$v(k_L/k_D)$	v ft./sec.
.90	1.015	.476	103.2	332	9.03	.829	.58	277.6	174.4	19.3
.91	.982	.526	115.3	266	9.05	.885	.62	297.4	182.1	20.13
.915	.966	.561	123.7	231	8.84	.919	.643	301.5	177.8	20.12
.92	.950	.568	126.0	223	8.75	.925	.647	300.4	174.4	19.94
1.00	.740	.757	182.4	106	5.29	.997	.698	195.7	13.3	2.514
1.005	.730	.765	185.2	103	5.15	.993	.695	189.8	4.6	0.893
1.01	.718	.772	188.0	100	5.00	.990	.693	183.7	−4.3	−0.86

On plotting, we find that v (rate of climb in ft./sec.) is a maximum at $R=0.912$ and is then = 20.13; v is $\doteq$ 0, and the machine is therefore flying level, at $R=1.008$; V here is 186.5 ft./sec.

In this table, $K_q k_Q$ is obtained from the formula for k_Q; V/nRP_m is then read off from Fig. 1 of R. and M. 577; V is thence found by multiplication; $k_L \doteq 3400/V^2$, k_L/k_D and η/η_{max} are read off Figs. 10 and 1 of R. and M. 577 respectively, and v is obtained by means of the formula

$$v = (k_L \times HP \times f(\sigma)\ 550/k_D \times W) - V(k_L/k_D)$$

20,000ft.

R	$K_q k_Q$	V/nRP_m	V ft./sec.	k_L	k_L/k_D	η/η_{max}	η	$V=\eta(k_L/k_D) \times 24.55$	$v(k_L/k_D)$	v ft./sec.
.87	.942	.577	121.0	.436	—	—	—	—	—	—
.875	.925	.597	125.9	.403	8.86	.946	.663	144.0	18.1	2.042
.88	.910	.613	130.0	.378	8.94	.959	.671	147.3	17.3	1.935
.91	.823	.693	152.0	.276	9.07	.995	.696	155.1	3.1	0.342
.915	.809	.705	155.4	.265	9.05	.998	.698	155.2	−0.2	−0.022
.92	.796	.715	158.5	.254	9.00	.999	.699	154.4	−0.1	−0.455

This gives level speed = 155 ft./sec. at $R=0.914$ and $v=2.04$ ft./sec. at $R=0.875$.

Further calculations were made on the assumption that the blades of the airscrew could be rotated (as if it were a variable pitch airscrew) and fixed in new positions, so as to give two other airscrews of $P_m=6.0$ and 9.0ft. respectively. The results for these are also included in Table I.

TABLE I.

Height.	S.E.5. $W=1976$lbs. $P_m=7.6$.	V.P. Airscrew. $W=2000$lbs. $P_m=$ Variable.	Variable Gear. $W=2000$lbs. $P_m=6.0$.	7.6.	9.0ft.
	No friction.		5% loss by friction.		
Speed.					
2,000	—	—	125.4	127.1	126.1
6,500	—	—	—	125.3	—
10,000	125.5	125.5	120.7	123.0	122.8
15,000	116.0	120.5	115.6	118.3	—
20,000	84.5	110.0	102.2	105.7	117.8
Climb.					
2,000	1020	1338	1259	1208	1100
6,500	760	1012	—	897	—
10,000	560	765	702	663	588
15,000	280	478	432	396	—
20,000	0	186	132	122	72
Ceiling is at	20,000	23,500	22,400	22,300	21,300ft.
		Gear Ratios.			
Speed.					
2,000	1.0	1.0	1.183	1.008	0.893
6,500	1.0	1.0	—	0.994	—
10,000	1.0	1.0	1.15	0.982	0.871
15,000	1.0	1.0	1.122	0.959	—
20,000	1.0	1.0	1.061	0.914	0.818
Climb.					
2,000	1.0	1.0	1.048	0.912	0.822
6,500	1.0	1.0	—	0.904	—
10,000	1.0	1.0	1.028	0.896	0.810
15,000	1.0	1.0	1.014*	0.884	—
20,000	1.0	1.0	1.016*	0.875*	0.787*

In the cases marked * the rate of climb is limited by the occurrence of the stalling attitude at the value of R given.

Some points in these results call for remark. In the first place it will be noticed that the airscrew of $P_m=9.0$ft. gives speeds which fall off much less rapidly as the ceiling is approached than do those of the other examples. This indicates that it should be possible to build a machine which would fly at a constant speed at all heights, and keep the engine revs. simultaneously at their most economical value, by the use of a variable gear.

Further, we see that if too low a gear ratio is used (especially if the P_m of the airscrew be small), it is possible to stall the machine. This is not however likely to occur where it would be most dangerous, at low altitudes, for there the best rate of climb occurs at a value of R greater than the stalling value. This fact may, however, limit the usefulness of the device unless some means of guarding against the difficulty can be found. Accidents might also result from this cause were the gear to become deranged during flight; but this could be obviated by so constructing the gear that, in case of breakage, it took up automatically a reasonably high value of R.

An infinitely variable gear is open to one of the objections to a variable pitch airscrew; it requires constant attention from the pilot.

These considerations suggested that it might be possible to get better results from a three-speed gear, of the type familiar to motorists, in place of an infinitely

variable gear. Inspection of the figures for the case $P_m=7.6$ indicate that the ratios should be:—

$R=1.0$, for flying level up to at least 10,000ft.

$R=0.9$, for climbing from the ground to 15,000ft. and for flying level above that height.

$R=0.88$, for climbing at heights above 15,000ft.

The working (omitted) proceeds by guessing several values of n (and therefore of the b.h.p.), calculating k_Q and thence the other quantities as before. This process must be repeated for each altitude, and separate calculations are in general needed for speed and climb as their gear ratios are generally different. The results are:—

TABLE II.

Height.	3-speed gear (5% friction loss).		S.E.5 (no friction).	
Speed.	Speed.	Revs.	Speed.	Revs.
2,000	125.3	1900	—	—
10,000	122.0	1855	125.5	1900
20,000	117.8	1953	84.5	1590
Climb.	Climb.	Revs.	Climb.	Revs.
2,000	1163	1910	1020	1700
10,000	666	1910	560	1675
20,000	118	1890	0	1590

The three-speed gear machine is very near the stalling attitude at the value of climb for 20,000ft.; possibly this might be avoided without much loss of performance by slightly raising the lowest gear if it is thought desirable to do so. The climb and speed level are nearly the same as those for the infinitely variable gear machines with $P_m=9.0$ and 7.6ft. respectively.

It appears, therefore, to be possible to construct a combination of variable gear and airscrew which will allow of a machine being flown at a nearly constant speed at all heights, while the engine revs. are kept at (or close to) any chosen value. This, it will be noticed, could apparently be done with a two-speed gear, if economy and simplicity so dictate; though the climb of such a machine at great heights would be adversely affected by the absence of the third gear. Against this must however be set increased immunity from stalling due to a low gear ratio before referred to.

If an accurate infinitely variable gear with a gear ratio recorder or dial were available, it would probably be of much value as an instrument of full-scale research, apart from any possible commercial or military advantages it might have. It is hoped, therefore, that this paper may draw the attention of aero engine designers to the matter even if considerations of engine construction and of the severe stresses involved render the practical value of such a gear doubtful.

CORRESPONDENCE

October 12th, 1923.

To the Editor of the Journal of the Royal Aëronautical Society.

ON THE STABILITY OF AERO ENGINES

Sir,—In response to a much-esteemed invitation from the Secretary of the Society, I beg to submit the following comments upon the paper contributed by Captain J. Morris to your April issue under the above title, and upon the subject generally.

In these comments I am enabled by kind permission of the Director of Research, Air Ministry, to make certain references to work which has been done in regard to torsional vibration at the Air Ministry, and to that now proceeding at the Royal Aircraft Establishment.

Resonant torsional vibration is a matter in which I am deeply interested, having found that serious troubles with certain important aircraft engines have been attributable to it in greater or less degree, and to my mind there is no question that further investigation of the problems involved will be much more than repaid by the resulting advance in engine design.

A purely mathematical treatment such as that given in the paper proves upon application to be inadequate, and may indeed be quite misleading, since, if the equations are used with blind faith, the results obtained will most probably be very wide of the mark; a word of warning is thus necessary.

It is not that the mathematical reasoning is unsound, but that it is necessary to exercise much care and judgment in determining the values to insert in the formula, and these are obtained most satisfactorily by experiment. For example, in the case of an eight-cylinder 90 deg. Vee geared engine which was investigated during 1918 because bad torsional failures were occurring, a purely mathematical treatment, based on the computed stiffness of the crankshaft journals and of the airscrew shaft, indicated that in the region of normal running speed, viz., 2,000 r.p.m., the *secondary* impulses of the engine torque synchronised with the lowest natural frequency of the crankshaft airscrew system, whereas a revised calculation based on the results of static twisting tests showed that the troubles experienced might be attributed to the *main* engine impulses producing torsional resonance.

In these investigations, which were made at the Air Ministry under my immediate direction, I treated the crank-throws as virtual flywheels and derived equations by the methods outlined by Chree, Millington and Sankey,* but developed to take into account the effects of the gearing. Captain Morris arrives at the same equations in a somewhat different way; also, he evolves some more general ones which take into account the *bending* of the crankwebs in the plane of their rotation, but he makes no reference to their *twisting*. For a crankshaft of normal design, the bending effects must be small, whereas the twisting effects may be quite large.

In the case mentioned above, static twisting tests were made on the crankshaft and on the airscrew shaft, because I doubted the validity of the stiffness computations. The tests revealed that the twisting of the crankwebs and crank-

* Proceedings Inst. of Mech. Engineers, 1904.

case considerably reduced the crankshaft stiffness, and that the effective length of the airscrew shaft varied with the torque, owing to the changing grip of the hub upon the shaft taper.

Although these investigations indicated that the troubles could be attributed to resonance effects, there was no direct means of settling, beyond dispute, the extent to which such effects were responsible for the failures. Even with the static tests made, one felt that the method of tackling the problem required some direct experimental support before much reliance could be placed on the results. Suitable experiments could not be undertaken at the time, and the results of the investigations made were considered with due reserve in deciding upon such alterations to the engine in question as could be entertained.

One such alteration was in respect of the reduction gearing, which was of the plain spur type with stub teeth. The pinion on the crankshaft had 27 teeth and the wheel on the airscrew shaft 45 teeth, so that the reduction ratio was 3 : 5. The torque reaction on the nosepiece containing the gears was thus two and two-third times the torque at the crankshaft pinion, and this might have accounted for the failures apart from resonance, particularly as the nosepiece was unsupported by the engine bearers.

If the torque variation alone was responsible, decreasing the gear ratio would reduce the torque reaction on the nosepiece, but the investigations indicated that this change would bring down the synchronous speed nearer to the running speed. On the other hand, increasing the gear ratio would make the synchronous speed more remote, but, disregarding resonance effects, would increase the torque reactions on the nosepiece. As radical alterations in design could not be effected, some gears giving a ratio of 35 : 37 were made with the object of reducing the torque reactions on the nosepiece and the torque in the airscrew shaft at the expense of airscrew efficiency. At the same time, owing to my apprehensions from the torsional vibration standpoint, some gears of 25 : 47 ratio were made for trial.

The first two engines fitted with 35 : 37 ratio gears suffered crankshaft failure under test on the hangar after about thirteen hours' running in each case, and this ratio of gearing was abandoned. It is very significant that the decreased ratio of gearing increased the troubles, for crankshafts had not broken with the original gears.

Owing to the intervention of the Armistice, the behaviour of engines fitted with the 25 : 47 ratio gears was not fully ascertained, but, from short bench runs and a few flights made, an improvement appears to have been effected.

Considering the matter now in the light of subsequent experience with other types of engine, I have little doubt that torsional resonance was chiefly responsible for the very serious failures that occurred. The investigations, so far as they went at the time, were communicated to the firms concerned, but have not been published. It was not, and still is not, easy to arrive at definite conclusions in such a case, for much remains to be discovered about the stress and strain changes a shaft will withstand for prolonged periods of running, making due allowance for oil holes, threads, splines, keyways, and fillets; and in this connection the paper read by Professor Jenkin before the Society during the last session is of extreme interest.

One has to remember that the necessary strength and stiffness must be obtained without undue weight, and it is only by scientific design that a high performance aircraft engine can be produced without a very real danger of torsional resonance trouble; this is particularly so in the case of radial engines.

Torsional resonance always exists in some degree, and the particularly pernicious thing about it is that the fact of its being present to a dangerous extent often does not manifest itself unmistakably until after prolonged periods of

running. Experience has shown that a test of 50 hours' duration is insufficient, and that at least 100 hours' successful running (at nine-tenths full power) is required to demonstrate the non-existence of the weakness in question—that is, in the absence of a satisfactory instrument devised and used for actually measuring the degree of resonant vibration that exists. The evolution of such an instrument will be referred to later in connection with the work proceeding at the Royal Aircraft Establishment.

The three main portions of Captain Morris' paper may be considered as :—

1. Investigation of Whirling.
2. Investigation of Airscrew Vibration.
3. Investigation of Torsional Resonance;

and some comments will now be made on each of these portions in turn.

Firstly, as regards Whirling

It is common engineering experience that shafts may be put through their whirling speeds and that they come back to a safe condition of running at higher speeds. The usual explanation is that the centre of gravity of the mass is not on the shaft axis and that beyond the critical speed the shaft deflects in such a way as to bring the centre of gravity nearer and nearer to the actual axis of rotation as the speed is increased. At first sight such a condition is unstable, but this impression is gathered by considering the shaft to deflect in one plane and to remain so during speed changes. My conception of the matter is that, as the shaft is accelerated through the whirling speed, the accelerating torque applied, combined with the tendency of the moment of momentum of the mass to remain constant, causes the shaft to take some slightly helical form, thereby complicating the motion in such a manner that stability results above whirling speed. There is a little experiment which might be made to test the soundness of this conception respecting whirling. Imagine a disc rotating about the normal through its centre, placed vertically to eliminate the effects of gravity, and attached to the disc through the medium of two equally-loaded springs placed radially, a mass whose position of rest is at the disc centre. If the mass is constrained to move in a diametrical direction, its angular velocity must be always that of the disc as it is varied, and when the speed corresponding to the "critical speed" of the arrangement is exceeded, the mass may be expected to fly out to a stop on one side or the other of the centre. If, however, the constraints be removed, one may expect the mass not to remain on a particular diameter, and, in accordance with the behaviour of shafts such as that of the De Laval turbine, it should assume a stable position nearer and nearer the disc centre. Perhaps some reader interested in the mathematics of whirling will try this arrangement. By putting the axis of rotation horizontal, the effects of gravity would be introduced and could be studied by means of a suitable stroboscope. In actual practice the mass on the shaft may have an appreciable polar moment of inertia, in which case stability may be influenced by gyroscopic forces.

I am not satisfied that Captain Morris's treatment proves the existence of the "speed of resonance," for it is clear that an investigation is at fault which does not lead to results consistent with actual observations.

Secondly, as regards Airscrew Vibration

This is perhaps the most original part of Captain Morris's paper, but one feels that the time is not yet for complicating the investigation of torsional vibration by considering airscrew-blade vibration in conjunction with it, although further work may show that this cannot be avoided. A given design of engine is fitted with various designs of airscrew according to the aircraft in which it is installed, and it is expedient to take as a basis its behaviour when fitted with an

airscrew which may be taken as rigid so far as vibration of the blades in the plane of rotation is concerned, and to treat the behaviour with any actual airscrews as particular problems in which the basis behaviour is modified. It may be mentioned that the polar moment of inertia of an airscrew is so large in comparison with the airscrew shaft and crankshaft masses that differences in magnitude of this quantity have little effect on the synchronous speed.

One despairs of reaching solutions of airscrew blade vibration mathematically, as the deflections are those of a twisted strip of varying sectional form; moreover, the frequencies are functions of the speed of revolution, whereas those of the virtual flywheel system are independent of this. Thus the investigations made by Captain Morris may serve as a guide, but prove to be based on assumptions that are too crude to enable the results to be used in computations. In the case of some tests made on a radial engine fitted with an airscrew and subjected to an alternating torque with the crankshaft not rotating, it was observed that at a certain frequency of torque variation, lateral vibration of the airscrew blade occurred with an amplitude of about $\pm \frac{1}{2}$in. at the blade tips. At the same time the amplitude of torsional vibration was considerably diminished, so that the lateral vibration appears to have had a damping effect on the torsional vibration.

With the shaft rotating the natural frequency of the lateral blade vibration would be increased by centrifugal force and the behaviour of the system modified accordingly. It should be mentioned that the phenomenon was associated with the engine mounting, as it was not observed when the tests were repeated with the engine mounted differently.

The error of assuming the airscrew to be rigid in the plane of rotation has been estimated by Dr. A. A. Griffith at less than $\frac{1}{2}$ per cent. of the synchronous speed, and an assumption which makes a result not more than $\frac{1}{2}$ per cent. low is quite satisfactory for practical purposes since other errors in the investigation may give an aggregate error of ± 3 per cent.

The reader will find some interesting remarks on the subject of airscrew blade vibration, by Griffiths and Hague, in R. & M. No. 452, June, 1918 (Advisory Committee for Aeronautics).

Thirdly, as regards Torsional Resonance

In his consideration of the two flywheel arrangement shown in Fig. 1, page 184, Captain Morris confuses matters by taking a system which is incomplete, and his mathematics only hold good if the gear P_2 is actuated by an inertialess system. The confusion is removed if instead of P_2 being taken as a gear, it is assumed to be the crank of a radial engine, the torque of which is produced by (inertialess) gas forces operating on the pistons, the inertia effects of the pistons themselves being that of one half their total mass located at the crank pin centre and included thus in the flywheel effect of the big end, crank pin, crank webs and balance masses.

A system such as this is shown in Fig. 9, but the complication is there introduced of assuming the cranks to be appreciably flexible in the plane of their rotation, and, as previously mentioned, this is quite an unnecessary elaboration.

Captain Morris states that within his knowledge the treatment he has adopted is entirely original, from which it is clear that he is very slightly acquainted with the work of others in this field.

A general treatment of a number of masses or flywheels on a shaft, subjected to a series of forces or torques, is given in the Appendix of Perry's "Applied Mechanics," the equations of motion being set down in the same fashion as in the paper under review. Moreover, in dealing with forced vibrations it is quite usual to express the varying load as a Fourier series, and the reader is referred

to "Vibrations of systems having one degree of freedom" * by the late Professor Hopkinson for a very clear outline of the method, in which, incidentally, the effects of damping are taken into account. Captain Morris makes no mention of damping, although this, *inter alia*, determines how near to the synchronous speed it is safe to run an engine. On this point it may be remarked that, assuming no damping, which is the safest thing to do for design purposes, there is a definite increase in torque variation produced by resonance at every speed up to the lowest synchronous speed and between synchronous speeds, and, since fatigue failure is largely a question of range of stress variation, it is clear that the amplitude of this variation at running speeds requires to be within certain limits. This amplitude can be calculated from the value of the synchronous speed, if known; and, by way of example, it has been found that the resonant effects at 1,600 r.p.m. may be sufficient to cause failure in a shaft system having a synchronous speed so far removed as 2,100 r.p.m., although, apart from resonant effects, the shaft would have a sufficient margin of strength.

Captain Morris's investigations are very much complicated by the inclusion of possible forms of airscrew blade vibration in relation to each type of engine, whereas according to Dr. Griffith's investigation, the assumption of airscrew rigidity in the plane of rotation is normally justified and the complication of taking it into account is unnecessary from the practical standpoint. For my own part, I prefer to assume the airscrew to be rigid and to build up the equations for the more elaborate flywheel systems from those for the simpler ones, as in this way one gets a better conception of the ways in which vibration may occur, the roots of the equation conveying clear physical meanings. This method also has the great advantage that it does not involve the use of really difficult mathematics.

Although the foregoing remarks are definitely critical, it is fully appreciated that a good deal of work and much mathematical ingenuity has been put into the investigations Captain Morris has made, and his approximate formulæ given on pages 210 and 211 are mathematically interesting.

As regards the effect of gearing, the method used in connection with the eight-cylinder engine previously mentioned was substantially that which Captain Morris gives.

In conclusion, brief reference will be made to some investigations of torsional resonance made, and in hand at the Air Ministry and the Royal Aircraft Establishment.

When renewing investigations subsequent to the Armistice, the synchronous speeds for certain single-throw radial engines were computed mathematically, and it was found that, in each case, the speeds were of the same order of magnitude as the running speeds, but that, according to the assumptions made in the calculations, they might be sufficiently far removed not to be of much importance; or, on the other hand, they might be dangerously close. The chief uncertainties were the true effective stiffness of the crankshaft, the permissibility of assuming the airscrew to be rigid in the plane of rotation, and the extent to which damping operated.

It was decided that the airscrew stiffness must be regarded as a matter for separate investigation in individual cases, and that in general the assumption of rigidity was justified for preliminary work; also that attention should be confined at first to single-throw radial engines, as this type is much simpler to deal with than multi-crank engines. Tests were made in the case of one such engine, comprising the static twisting of the shaft, and also the determination of the natural frequency of vibration by applying to the crank pin a tangential load about equal to the maximum crank effort of the engine, with the hub rigidly held, and suddenly

* Cambridge Engineering Tracts, No. 1, 1910.

removing the load, the resulting natural vibration being recorded photographically on a uniformly rotating drum.

Attached to the crank pin was a mass equivalent in inertia effects to the big end mass and the reciprocating masses. Close agreement was found between the frequency calculated from the stiffness tests and that recorded.

The design of engine in question had developed symptoms of torsional resonance trouble, and when the shaft was re-designed in the light of these investigations, the troubles disappeared, although but little increase in weight had been entailed by the alterations.

From the information gained in the course of these tests it does now appear to be possible to deal satisfactorily with the matter for new engines of the single-throw radial type when in the design stage. Even for engines of this type, however, it is desirable to get definite measurements of the amplitude of torsional vibration under various conditions of power and speed.

One point upon which more light is required is the degree of damping that occurs, and associated therewith the danger-speed range for the forced vibrations.

In the absence of heavy damping, the amplitude of vibration at speeds removed from the synchronous speed can be computed with a fair degree of accuracy, and provided one knows what the material will stand indefinitely in the matter of fluctuating stress, the danger range can be estimated.

Some progress has been made at the Royal Aircraft Establishment in the direction of actual measurement of cyclical twist changes. Promising polar diagrams of varying twist have been obtained photographically from the crankshaft of a nine-cylinder radial engine when running at speeds approaching 1,500 r.p.m. during rough preliminary tests of an instrument which is being developed there. In these tests, resonance was observed of a magnitude which checks roughly with that previously computed for little damping, and one is encouraged to expect that with further improvement of the instrument, satisfactory quantitative results will be obtained at speeds up to 2,000 r.p.m. or higher.

By means of such an instrument it is hoped to make experiments which will put the investigation of the whole subject on to a more substantial basis than it has been hitherto.—Yours faithfully.

B. C. Carter.

The Journal

OF THE

Royal Aeronautical Society

(Founded 1897 in succession to the Annual Reports)

Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

No. 156 DECEMBER, 1923 Vol. XXVII

NOTICES

Election of Members

The following members were elected at a Council meeting held on November 13th:—

Fellow.—H. N. Wylie.

Students.—C. E. T. Maguire, H. A. Miles, G. Ponsonby, H. C. de M. Seaman.

Foreign Member.—J. L. Merrill.

Scottish Branch.—*Member.*—B. H. Alexander.

Lectures

Owing to the dislocation of work and loss of some of the records, as a result of the recent earthquake, the lecture by a representative of the University of Tokyo announced for February 7th, 1924, will not take place.

It will be replaced by Squadron Leader Maycock's paper on "Airmanship at Sea," which was originally announced for November 29th.

Research

A statement of the views of the Council on progress in research with particular reference to the Helicopter prizes announced by the Air Ministry, will be found on another page.

Air Congress Report

The Report of the International Air Congress, London, 1923, which contains several papers additional to those read during the Congress as well as verbatim reports of all the discussions, is now being printed and will become available towards the end of the month. It will be obtainable from the Society's offices, price £1 5s. 0d. (£1 to members of the Congress), postage extra.

Forthcoming Arrangements

Wednesday, December 5th, 8.30 p.m.—*Cambridge University Aeronautical Society.*—Major H. E. Wimperis, "Some Aeronautical Problems."

Thursday, December 6th, 5.30 p.m.—*Newcomen Society.*—Mr. Ivor B. Hart, A.F.R.Ae.S., "The Scientific Work of Leonardo da Vinci."

Tuesday, December 11th, 5.30 p.m.—Council Meeting.

Wednesday, Dec. 12th.—*Royal United Service Institution.*—Wing Com. Edmonds, "Air Strategy."

Friday, December 14th, 6.0 p.m.—*Institution of Mechanical Engineers.*—Professor A. H. Gibson and Mr. A. Wright Baker, "Exhaust Valve and Cylinder Head Temperature in High-Speed Petrol Engines."

Thursday, Jan. 10th, 5.30 p.m.—Dr. L. Aitchison and Mr. J. D. North, "Materials from the Aeronautical Point of View."

W. Lockwood Marsh, *Secretary.*

THE AIR MINISTRY HELICOPTER PRIZES

The Council have lately been devoting considerable time and attention to the attitude of the Air Ministry towards research and in particular to the Helicopter prizes now on offer.

It will be recalled by members that in January, 1922, a deputation of the Council waited on the then Secretary of State for Air (Captain the Hon. F. E. Guest, M.P.) urging "the need for better safeguards to prevent the submerging of applied scientific research in aeronautics by technical *ad hoc* experimental work" (AERONAUTICAL JOURNAL, Vol. XXVI., p. 43).

The Council regret to say that they feel that there has been no improvement since that time and that in fact the position is, if anything, worse. The recent performances of American aeroplanes in speed, endurance and climb, which are the direct results of applied research, cannot fail to invoke comparison of an unfavourable nature with regard to British progress during the same period.

The Council of the Society have had certain correspondence with the Air Council relative to the prize to the value of £50,000 for helicopters. Such prizes, in the view of the Council of the Society, tend to give a wrong view of the relative values of serious research work on well established lines and such highly speculative experimental constructions as the helicopter.

The helicopter has on several occasions lately been the subject of discussions before the Society, the outcome of which has been the confirmation of the earlier impression that its limitations are such as to render expenditure on it unjustified in view of the amount of research work of an urgent nature which is required for the development of aeroplanes and airships. So far from being likely to solve the main problems of flight, the Council believe that the helicopter introduces more difficult problems. It depends for its safety even more than the aeroplane on freedom from power-plant failure and introduces extremely difficult problems of management and control. In the Council's view such questions as, for example, the definite diminution of power-plant failures, the control and stability of aeroplanes, the increase in the carrying capacity of aeroplanes, and navigation when the pilot is unable to see the ground are of immeasurably greater importance than the development of the helicopter. They also believe that the risks to life in any helicopter so far proposed will be vastly greater than in any type of aeroplane in use.

The Council are confident that members of the Society would endorse these views, which were laid before the Secretary of State for Air (the Right Honourable Sir Samuel Hoare, C.M.G., M.P.) by the Chairman and Vice-Chairman at a recent interview.

PROCEEDINGS

Second Meeting, 59th Session

A meeting of the Society was held in the rooms of the Royal Society of Arts, John Street, Adelphi, London, on Thursday, October 18th, 1923, Colonel A. Ogilvie (Chairman of the Society) presiding.

Squadron Leader R. M. Hill, M.C., A.F.C., read the following paper:—

THE MANŒUVRES OF INVERTED FLIGHT

Contents

Part I.—Introduction

1 *Definition of Inverted Flight*

In the following paper it will be convenient to adopt the definition of inverted flight used by the Accidents Committee in R. & M. 617 (Advisory Committee for Aeronautics), "Reports on the behaviour of aeroplanes when flying inverted, with special reference to some accidents on 'A,'" which was as follows:—"In normal straight flight the pressure of the pilot on his seat is equal to his weight. During inverted straight flight he would hang on his belt with an equal force. It is therefore considered useful to call inverted flight such flight only as occurs when the weight of the pilot no longer presses on his seat."

2 *Reasons for Investigation of Inverted Flight*

(*a*) During the latter part of the war an unduly large number of accidents had occurred in the training of pilots in aerobatics on unstable aeroplanes, such accidents being 50 per cent. in excess of similar ones on stable aeroplanes. The Accidents Committee had investigated the problem and as a result had issued R. & M. No. 617 (January, 1919), which commences as follows:—

I. INTRODUCTION.—During recent months a tendency has been observed in a number of accidents to "A" aeroplanes to assume the inverted position consequent usually on some error in flying. A feature was the apparent difficulty and frequent failure of the pilot to right the aeroplane. The Committee was therefore requested to consider the cases put forward and to investigate the flight of aeroplanes in the inverted position, with special reference to this particular type."

The analysis contained in R. & M. 617 went far to dispel the obscurity surrounding these accidents, and the conclusions arrived at were illuminating both to designers and pilots; based however, as they had to be, on inference from the general stability characteristics of aeroplanes rather than on the recorded experience of deliberate inverted flight, they required that such experience should be urgently sought. In the light of this experience it can now be said that these conclusions have received nothing but support from actual experiment.

As many of the accidents had been fatal, the pilots who had been able to supply information about the phenomena of inverted flight consistent enough for reasonable interpretation were few. Flying opinion was so divergent that even those who had made a habitual practice of flying inverted had failed to crystallise definite ideas on the subject. Such general lack of knowledge and the accompanying inability to distinguish real from imaginary risk, resulted in a loss of perspective that was injurious to flying morale.

The first and main reason for the inverted flying experiments undertaken at the R.A.E. was the necessity of testing and, if necessary, supplementing the argument of R. & M. No. 617. Only by throwing the maximum possible light on the causes of these accidents could they be eliminated in the future.

(*b*) While it was recognised that the accidents referred to must frequently have been caused by a failure on the part of the pilot to appreciate the characteristics of his aeroplane, it was suspected that the behaviour of an unstable aeroplane when inverted was such as to make the human failure far from unnatural; and even that under certain conditions the control might be inadequate for the recovery of normal flight. Cases more simple to understand were known to have occurred in which the pilot, either because he was so insecurely belted that he fell from his seat in the inverted position too far to be able to reach his controls, or because he was suffering from the mental and physical effects of involuntary inversion, or both, was unable to recover before striking the ground. On the other hand there appear to have been no cases where structural failure in the air could be directly attributed to manœuvres in the inverted position. This was certainly not because the loads that might be imposed on aeroplanes by inverted flying manœuvres had been specifically taken account of in design.

The second reason for the inverted flying experiments was the importance of forming an idea of the magnitude of the loads in inverted flying manœuvres, the possibility of which had not previously exerted a direct influence on stress calculations.

(*c*) The progress of the inverted flying experiments not only continued to clear away the doubt connected with past accidents on unstable aeroplanes, but opened up new avenues of thought. The idea that an unstable aeroplane flew inverted only too easily and was in some conditions difficult to bring back to normal flight, had its converse—that inverted flight on a stable aeroplane was difficult, if not impossible, to achieve.

The third reason for the experiments, a reason that as they developed became more cogent, was the desirability of analysing the behaviour in inverted flight of aeroplanes differing in stability characteristics, and the consequent formation of a sound tradition in a new sphere of aerobatics. The fear of involuntary inversion would pass away; the pilot would equip himself for handling his aeroplane

intelligently in any position it might happen to assume; better still, he would deliberately accustom himself to every possible attitude of flight, so quickening his sense of the niceties of aerial manœuvre that his descriptions of inverted flight, unlike those extracted from pilots whose senses the alarming experience of being inverted suddenly and without warning had blurred, would spring from a cool determination to observe, and thus have at least a fair chance of aiding scientific theory.

3 *Nature of this kind of Experiment*

It was impracticable at the outset to devise a programme of the precise form or sequence of the experiments. The stimulus was present in that unstable aeroplanes were known to have been the cause of a considerable waste of lives. Too little, however, was known about inverted flying to give any pilot definite instructions as to what he should do or how far it was safe to go. All that could reasonably be done with models, and with deductions from the behaviour of various types of aeroplane in normal flight, had been done. Until pilots could be found to reproduce intentionally the difficulties occurring in inverted flight, and describe them intelligently, the solution of the problem would remain as far off as ever.

The first step therefore was to secure the co-operation of pilots who either possessed some knowledge of inverted flight or were keen to learn about it, and leave it to their initiative to push ahead in their own way. Rather than urge them to confine themselves to any particular way of experimenting, I hoped that by stimulating their natural desire to develop skill in inverted flying, results, almost unconsciously, would come; and that miscellaneous information gathered from a number of pilots throughout a considerable period, would, when pieced together, form a more or less continuous whole. In grouping these results I have paid more attention to their place in relation to the experiments than to an adherence to chronological order.

4 *Brief Outline of its History*

The experiments on inverted flying followed naturally from previous experiments on the spinning of many types of aeroplane, and on the behaviour in looping of certain unstable aeroplanes, with which trouble had occurred. This line of thought found practical embodiment in an attempt to eliminate the undesirable characteristics of instability evident in the Sopwith "Camel" without spoiling its flying qualities from the service point of view; and a Sopwith "Camel" was modified by having its wings moved back and the weight redistributed, to bring the C.G. relatively further forward and gain longitudinal stability. The comparative experiments that were carried out on it are described in T.1457. They stopped short of actually flying it inverted.

The standard and modified Sopwith "Camels" formed the nucleus of material on which the experience of various pilots, some of whom had already had experience of inverted flight, and others of whom were subsequently trained, could be brought to bear. In the autumn of 1919 Flight Lieutenant Bulman, M.C., A.F.C., experienced both in flying a standard Sopwith "Camel" in war and in training war pilots, commenced experimenting and submitted a paper T.1440. He described his initial attempts at attaining the inverted position, both by half looping and half rolling, the aeroplane's behaviour in an inverted stall, and his attempts to reproduce the consequences of a failed loop.

So impressed was he with the accidents he had witnessed in the training of pupils, that he tried for some time to reproduce the inverted spin which he knew had frequently been a feature of these accidents, but without avail. In the autumn of 1920 Flying Officer Gerrard, D.S.C., one of the most experienced

pilots in the Service on Sopwith " Camels " and " Snipes," joined the R.A.E. He had succeeded in carrying out inverted spins intentionally, and, as far as is known, was the only pilot who had done so up to that time. He was able to demonstrate the method to Bulman, who immediately went up and successfully reproduced the inverted spin.

Meanwhile Bulman and Flying Officer Sainsbury, A.F.C., had both been flying inverted on the " Bat Bantam," and in the summer of 1920 they had succeeded in performing the first half of an inverted loop on this aeroplane. Bulman continued to enlarge his experience by flying inverted on the Sopwith " Snipe." From the autumn of 1920 onwards Bulman and Flying Officer Scholefield, D.C.M., commenced to fly inverted on the S.E.5A, an aeroplane that possesses a marked degree of longitudinal stability; and, although it had not up to that time been considered possible, they were able to maintain steady inverted flight on this type. Finally, the inverted spinning experiments were extended to the modified Sopwith " Camel " and the S.E.5A.

Part II.—Inverted Flight

1 *General Remarks*

It is known that an aeroplane which is longitudinally stable in normal flight will tend to be unstable in inverted flight, that is, it will drop its nose and return to its stable trimming speed in normal flight, if its balance is such that one exists. On the other hand an aeroplane that is longitudinally unstable in normal flight, if its balance permits, always tends to reach and maintain a condition of stable equilibrium inverted. As indicated in R. & M. 617 the important factor in settling these characteristics is the position of the C.G. relative to the wings. The more stable an aeroplane is in normal flight, the more difficult is the pilot's task of maintaining the inverted position, and *vice versa*. It is therefore relatively easier to fly unstable aeroplanes like the Sopwith " Camel " and " Snipe " inverted than stable aeroplanes like the S.E.5A. The " Bat Bantam," which is about neutral in stability at normal flying speeds, falls between the two extremes.

While the stability characteristics discussed above are the chief determinant of an aeroplane's behaviour, its balance has a most important effect on the ease of inverted flying, or alternatively the ease of recovery. The disposition of the Sopwith " Camel " to remain inverted is weakened by the standard practice of rigging it tail heavy in normal flight, whereby a tendency towards self-righting is introduced. When the " Camel " was rigged so as to balance correctly in normal flight, accidents occurred; for its inclination to follow its natural stability characteristics and remain inverted was thus allowed greater freedom. An aeroplane that is balanced to be nose heavy in normal flight will always, irrespective of its stability characteristics, be easier to fly inverted than one that is balanced to be tail heavy. If, therefore, inverted flight is attempted on a stable aeroplane, it is desirable, should the tail incidence be adjustable, to give the tail its maximum incidence in order to produce nose heaviness in normal flight, and a corresponding tail heaviness in inverted flight that will tend to overcome the aeroplane's natural self-righting properties. If the nose heaviness in normal flight thus produced becomes excessive, it is necessary to compromise, and about two-thirds of maximum tail incidence should be given.

To control his aeroplane effectively the pilot must, as it were, sympathise with its flying qualities. If he treats the aeroplane as something foreign to himself, he tends to make a purely mechanical use of the controls not in harmony with its flying qualities. If, however, he studies its motion closely he will find that its properties of balance, its stability characteristics, or the gyroscopic effect of its engine may be employed to relieve his control surfaces of a considerable part of their work, thus allowing them a margin of effectiveness to be acquired

FIG. I
PERSPECTIVE VIEW OF PILOT'S CONTROLS.

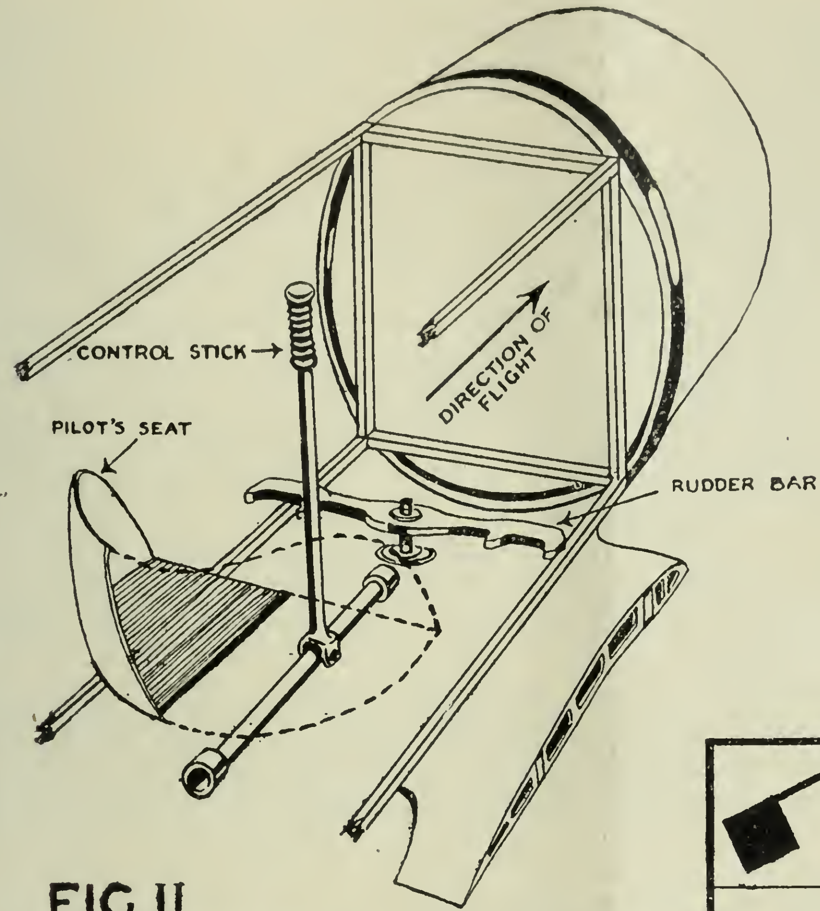
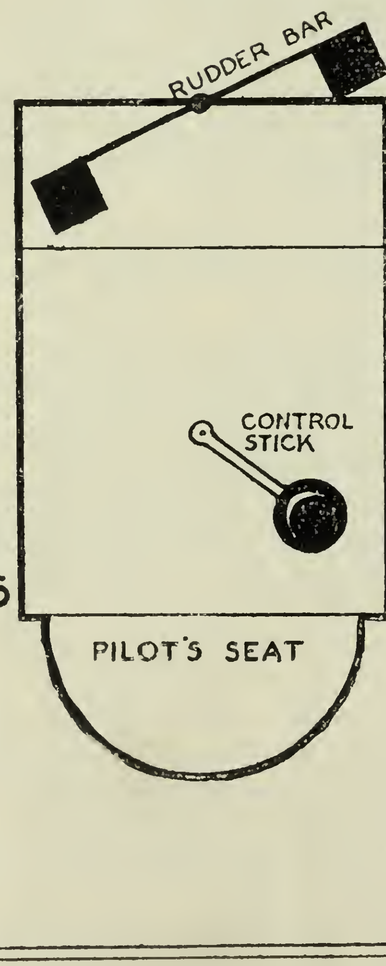


FIG. II
CONVENTIONAL DIAGRAM OF CONTROL STICK AND RUDDER BAR POSITIONS USED TO ILLUSTRATE THE CONTROL POSITIONS IN THE DIAGRAMS OF THE VARIOUS MANOEUVRES.



in no other way. More especially in an unstable aeroplane, a large number of the pilot's control movements are movements made to damp various kinds of motion which, though originally excited by the use of the controls, are yet the result of the aeroplane's natural propensities. This feature of control is important in inverted flying where the controls are as a whole less effective, and where perhaps a given manœuvre may depend entirely for its success on utilising them to the best advantage. Bearing this in mind the pilot will realise that if with his controls he excites a motion, which the qualities of the aeroplane are such as to stimulate, the longer he allows the angular velocity of the aeroplane to increase, the more difficult his task of damping the motion becomes; so much so that he may be compelled to abandon his original manœuvre and temporarily allow the aeroplane its own will.

To handle his aeroplane in accordance with the above considerations the pilot requires, apart from his instruments, which it is somewhat difficult to use consistently, some point or points of reference. Normally he refers the angular movement of his aeroplane to the ground; and it is of the first importance that the method he acquired of visualising his attitudes in normal flight should be applied to inverted flight. He may experience a temporary sensation of confusion similar to that felt in a loop or roll, while turning himself upside down. But once having attained the inverted position neatly, having allowed the belt to take his weight, and having experienced the satisfaction that it is really doing so, he will feel quite secure and the mental distress will disappear. He will, moreover, realise with some surprise than he can judge the exact attitude of the aeroplane by the horizon, and to some extent read his instruments. After half a minute or so he will probably feel the blood running to his head; it is, however, quite possible for a pilot to accustom himself to hanging inverted for considerable periods without experiencing any severe effects.

In each of the following sections I shall first indicate broadly the classes of control movement which govern all inverted manœuvres with all types of aeroplane, and then attempt to describe the refined differences of control movement that belong to aeroplanes of different stability characteristics (see Figs. I. and II.).

2 *Methods of Attaining the Inverted Position*

(*a*) *General*

Before practising inverted flight on any particular aeroplane it is essential that the pilot should familiarise himself with the standard manœuvres of looping, spinning, rolling, and rolling off the top of a loop. The last manœuvre is especially valuable in teaching him that an aeroplane is incapable of being manœuvred on the top of a loop if the speed has been allowed to drop too much.

There are two normal methods of attaining the inverted position; by means of a half loop, or a half roll. That of the inverted half loop is discussed in Part III. (2).

If the pilot intends to attain the inverted position by means of a half loop, he should loop the aeroplane several times and watch his airspeed indicator all the way round. In a normal loop the speed frequently falls to between 40 and 50 m.p.h. just on top of the loop. This speed is well below the stalling speed of the aeroplane when inverted, which may be 65 or 70 m.p.h. It is therefore essential that the half loop prior to inverted flight should be commenced, if the speed on the top of the loop is to be adequate, at a higher initial speed than that for a normal loop.

To commence inverted flight, the pilot pushes forward the control stick approximately half way through the loop to prevent the nose from dropping. With some aeroplanes he may experience difficulty in knowing just when to

push forward the control stick; with others he may have considerable latitude, and inverted flight will flow naturally from the half loop. If he pushes it forward too soon, the aeroplane may stall inverted; if too late, it will have gone too far into an inverted dive. The highly unstable aeroplane may possibly cause trouble if the control stick is pushed forward too soon, as any unpleasant characteristics peculiar to this class are accentuated near the inverted stalling speed. With such aeroplanes therefore it is wiser to be on the late side in pushing forward the control stick. If, with the more stable types, the control stick is not pushed forward at the right moment, the self-righting tendency will at the worst cause the aeroplane to drop its nose and come out in a straight inverted dive.

The above method lends itself more readily to a stationary-engined aeroplane in which the pilot experiences practically no gyroscopic effect tending to slew it about on top of the half loop. With a rotary-engined aeroplane the pilot must exercise a greater amount of care. In performing the loop he will find that the gyroscopic effect of the engine will necessitate rudder to counteract it. As soon as he commences flying steadily inverted the gyroscopic effect will disappear; and unless he is quick in centralising the rudder at the right moment, the aeroplane may roll out in a stalled condition or possibly fall into an inverted spin.

Attaining the inverted position by means of a half roll is a quieter manœuvre. It is possible to roll over upside down, fly inverted for some time and roll out on to an even keel in normal flight without ever exceeding a speed of 80 m.p.h. or imposing any undue stress on the aeroplane. A great advantage of the half roll is that the pilot need never lose sight of the horizon, thus retaining a more continuous sense of where he is. To assume the inverted position he may use a very slow half roll, in the process of which he must, if he is to avoid a rather severe side-slip, turn slightly off his course. On the other hand he may half roll quickly on to his back. In this case he gives the ordinary control movements for the roll, commencing the manœuvre at from 70 to 90 m.p.h. This will induce the characteristic whirling motion associated with auto-rotation. As, judging by the horizon, he approaches the inverted position (how near depends on how easily the aeroplane rolls and how easily it can be checked) he must check the roll by pushing the control stick forward towards the dashboard, centralising the rudder, or giving a little opposite rudder, and at the same time taking off aileron. The amount he will have to push the control stick forward of the central position will depend mainly on the stability characteristics of the aeroplane. A few trials may be needed to give precision and grace to the manœuvre.

The pilot will probably discover that the most comfortable speeds for inverted flight lie between 70 and 100 m.p.h. When he first attains the inverted position he may find that he is almost stalled, in which case he must ease the control stick towards him to gain more speed and control; on the other hand he may find himself rapidly falling into an inverted dive, when he must push the control stick away from him. If he does not carry out these corrective movements quickly enough, he may experience a tendency, at least during his initial attempts, either to stall and topple over sideways, or to fall out in a dive. He should not be disappointed, but should practise until he acquires the necessary judgment to turn the aeroplane on its back cleanly, with enough speed to make it controllable, yet not so much as to induce a dive.

Before the behaviour of individual aeroplanes is dealt with, a note on the management of the engine is necessary. The petrol systems of aeroplanes do not make special provision for petrol supply to the engine when inverted. It is only by chance that in one or two aeroplanes petrol continues to be supplied to the engine; and independently of the petrol system a stationary engine with a float carburettor will cease firing almost immediately on inversion. Although the method of attaining the inverted position by means of a half loop is suited to stationary-engined aeroplanes on account of the absence of gyroscopic effect, the

wisdom of turning the aeroplane upside down with the stationary engine running and allowing it to cut out, is questionable. A certain amount of petrol is bound to be spilt out, the presence of which is undesirable inside the cowling. Unfortunately an aeroplane cannot be half looped on to its back without the engine running, and Bulman has frequently done this on the "Bat Bantam" and the S.E.5A for experimental purposes. If petrol can be supplied to it, a rotary engine with a block tube carburettor will continue to fire. The Clerget engine of a Sopwith "Camel" will continue to run for half a minute on petrol contained in the pipes between the tank and the engine. There is evidence to show that the B.R.2 engine of a Sopwith "Snipe" will, even at full throttle, run for a longer period still. Oil is nearly always spilt into the cowling, and on the Sopwith "Camel" I have had trays fitted to collect it.

(b) *Application to Particular Aeroplanes*

Standard Sopwith "Camel."—The standard Sopwith "Camel" may be inverted by any of the methods referred to above. Inversion by the half loop is attended by the difficulties arising from the gyroscopic effect of the engine, accentuated by the very small rudder. In the second quadrant of an ordinary loop, full left rudder is needed to keep this aeroplane from yawing to the right. The rudder moment, which is then just sufficient to hold the yaw in check, induces a skid to the right. If a loop on a "Camel" is observed by another aeroplane from behind, the "Camel," when held straight by its pilot, is seen to skid bodily sideways. The skid is not, however, appreciated by the pilot in the "Camel." On a stationary-engined aeroplane where there is practically no gyroscopic effect, the pilot knows he has looped straight if he re-enters his own backwash; if on a rotary-engined aeroplane he has held the nose straight he will be displaced laterally and pass it by. Apart from the gyroscopic effect just referred to, the "Camel," being rigged tail heavy, behaves sweetly in the first half of a loop. The pilot is merely concerned in releasing his pressure on the control stick and allowing the nose to rise up. By the time he is inverted the control stick is fully back, and the rudder is full across to the left. To commence inverted flight, it is sufficient to exert a gentle forward pressure on the control stick. At this moment the gyroscopic effect disappears suddenly, and, if the rudder is not centralised at once, the aeroplane will slew rapidly to the left. To perform this manœuvre correctly the pilot has to ensure that he has enough speed on top of the loop to prevent an immediate stall, and to co-ordinate his elevator and rudder movements sensitively. A misjudgment either of speed or of control movement is enough to spoil the manœuvre (see Fig. III.).

The above difficulties are reduced if inversion is attained by a half roll. The slow half roll seems most adapted to the flying qualities of the "Camel." In this manœuvre the pilot cannot, unless he sideslips severely, avoid yawing off his course; as however he yaws the other way in the half roll for recovery, he regains his initial direction of flight. The gyroscopic effect of the engine asserts itself, but is not so difficult to deal with. If the pilot rolls to the right, the nose is forced down; if to the left, it is forced up. As the rudder is so small, a right-hand roll is easier; for it is better that the nose should drop rather than rise if the limit of rudder control is exceeded. Again, the pilot has greater control over the speed than at the top of the half loop. A convenient speed at which to commence the half roll is between 70 and 90 m.p.h. at half throttle. In half rolling to the right the pilot gently but firmly applies right rudder and right aileron. The "Camel" begins to roll over and to turn to the right. In anticipation of the gyroscopic effect the pilot has previously lifted the nose slightly; as soon, however, as the rudder and elevator interchange functions, he commences to push the control stick forward and turning ceases. As the wings pass the vertical he maintains right aileron, gradually takes off right rudder, and pushes the control

stick more forward, until as he approaches the inverted position he takes off right aileron completely and finishes with the control stick slightly forward of the central position. The movement of the control stick amounts to an anti-clockwise circular sweep in the right-hand side of the cockpit. Bulman found that he was able in half rolling to push the control stick forward at an earlier stage, and thus to keep on his course better. When I tried this I induced a severe sideslip. As however Bulman usually rolls to the left and I to the right it is conceivable that the pilot's instinctive allowance for the gyroscopic effect forms a habit which

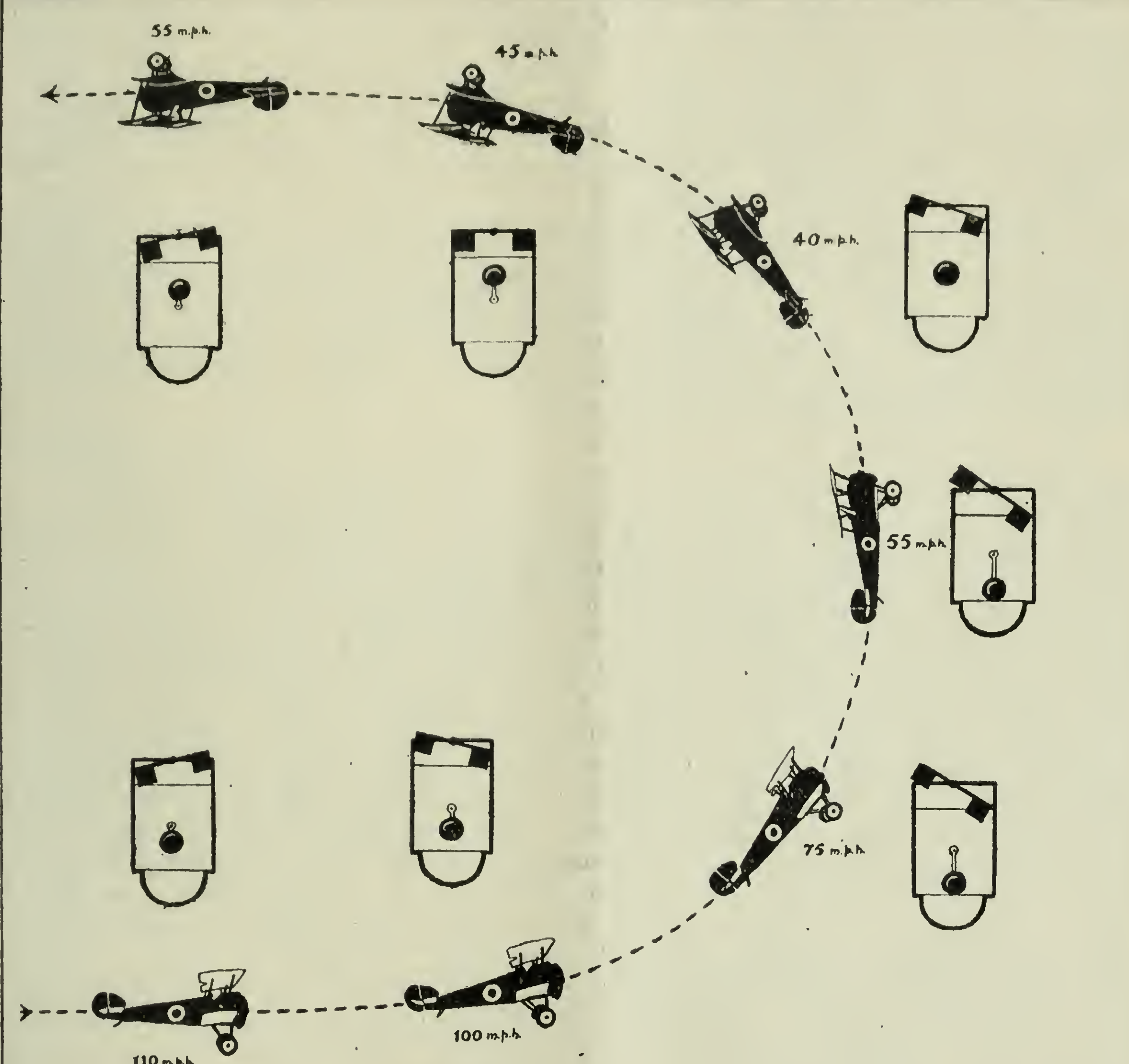


FIG. III
HALF LOOPING METHOD OF ATTAINING INVERTED FLIGHT
STANDARD SOPWITH CAMEL (UNSTABLE).

tends to persist even when the roll is made in the reverse sense to that which is customary. In this connection, it is interesting to note that nearly all pilots are right or left-handed, and that few experience the same ease in performing a complicated manœuvre to the left and right.

Modified Sopwith "Camel."—The modified "Camel" could be inverted either by half looping or half rolling. In an ordinary loop its longitudinal stability caused it to behave differently to a standard "Camel." Whereas during

the second quadrant the latter came over almost too quickly, the modified "Camel" had to be pulled over like the S.E.5A. Again, during the third quadrant, the modified "Camel" dropped its nose more quickly than the standard "Camel"; and therefore, in the half looping method of attaining the inverted position, the control stick had to be pushed forward more vigorously to keep the nose from falling. As in the standard "Camel," it was necessary to manipulate the rudder to counteract gyroscopic effect, but the likelihood of spoiling the manœuvre by a failure to centralise it as inverted flight commenced and the aeroplane became nearly stalled, was less.

I did not try the quick half roll for attaining inverted flight, though it would probably have been attended by no difficulty. In the slow half roll the controls had to be used, as a whole, more vigorously than in the standard "Camel." The movements were similar, with the exception that the control stick had to be pushed forward more definitely to keep the nose up as the aeroplane became inverted. In other words, the modified "Camel" showed characteristics similar to stable aeroplanes like the S.E.5A.

Sopwith "Snipe."—On the Sopwith "Snipe" Gerrard attained the inverted position both by half looping and half rolling, but he preferred the half loop. Unlike the "Camel," the "Snipe" has an adjustable tail; and as it is longitudinally unstable in normal flight its tail setting has a less important bearing on the case of flying it inverted than in the S.E.5A. At the same time, increased incidence on the tail does materially assist.

The half looping method is used as described for the "Camel," the commencing speed being 120 to 125 m.p.h., 10 to 15 m.p.h. in excess of that for an ordinary loop. At a point when the centre section spar appears to subtend about 10° to the horizon, the control stick is gently but deliberately pushed forward until the nose is steadied in the inverted position and held there. Gerrard says that he could keep his engine full on in inverted flight for a considerable period, and as a result was actually able to climb. If the engine were throttled back the nose fell somewhat and an inverted glide resulted. In this method the pilot is faced with a gyroscopic effect similar to that noticed in the "Camel," and has to use the same judgment in centralising the rudder at the commencement of inverted flight. Although the "Snipe" is fitted with a large rotary engine (the B.R.2), it has, relatively to the "Camel," a large rudder, which is better suited to counteract the violent gyroscopic effect. The centralisation of the rudder must be a direct consequence of the new conditions set up by the pushing forward of the control stick; and if, after pushing forward the control stick, the pilot is late in centralising the rudder, he stalls on his back with rudder on and introduces a set of conditions favourable to the inverted spin. (See Part III. (1).) Again, if the pilot pushes forward the control stick too late, the nose may have dropped too far, and the angular velocity of the aeroplane have increased so much that he will find it difficult to push the nose up again. At the same time an unstable aeroplane like the "Snipe" will always respond to this correction more kindly than a stable aeroplane; and as the involuntary inverted spin is to be avoided, the mistake of pushing forward the control stick too late is the less undesirable.

The slow half roll is most easily performed if the "Snipe" is flown at a speed in the neighbourhood of 100 m.p.h. Gerrard insisted on the necessity of selecting, if possible, some definite point on the horizon in order to stimulate the pilot's sense of direction. In half rolling to the right he pushed the control stick slowly and deliberately over to the right, meanwhile checking the tendency of the aeroplane to yaw off its course by gradually easing the control stick forward. With the steady application of right rudder the aeroplane then commenced to turn slowly over sideways. As the bank became steeper the control stick was eased more forward still, especially after the wings had passed the vertical. At this point Gerrard gradually reversed his rudder (gave left rudder)

until by the time that the aeroplane was completely inverted he had centralised it again. It will be noted that this use of reversed rudder in the final stages of the slow half roll differs from that in the "Camel," where the rudder was employed in varying degree but always in the same sense as the rotation of the aeroplane. Though unusual, this method was applied by Scholefield to the S.E.5A. It at least shows the difference of technique employed by various pilots. Gerrard's forward movement of the control stick in the "Snipe" before the wings passed the vertical agrees with Bulman's in the "Camel."

The quick half roll can also be performed on the "Snipe." The pilot, commencing at a speed of about 90 m.p.h., gives the ordinary control movements for a roll. Just before the aeroplane is half way round he pushes the control stick forward and centralises the rudder, or momentarily applies rudder opposite to the sense of rotation of the aeroplane. The effect of commencing the roll with the nose too much up or down is important, and a mistake in the attitude of the aeroplane in the initial stages may be irremediable. Once, however, the quick rolling motion has been excited, the elevator seems to be the essential factor in checking it; which is to be expected if this whirling motion is associated with auto-rotation. The rudder is only used to assist the elevator movement for the incorrect timing of which no skilful use of it can atone. As the control movements must be both sensitive and rapid, good judgment is required to perform this manœuvre with precision; and until practised the pilot, through having checked the whirling motion too early or too late, will be compelled to make subsequent corrective control movements that are detrimental to the grace of the manœuvre.

"*Bat Bantam*."—Bulman carried out a considerable amount of inverted flying on the "Bat Bantam." Its outstanding feature is its unusual controllability; in fact there is no other scout that equals it in the harmony of its elevator, rudder and aileron controls and the consequent effectiveness with which the pilot can use them. In longitudinal stability it is approximately neutral, and its flying qualities would not therefore be expected to be so favourable to inverted flying as those of the unstable types, the "Camel" and "Snipe." Perhaps in some degree this is true, but the unique effectiveness of its controls enables the pilot to carry out inverted manœuvres and maintain it in attitudes of flight that would be impossible with a scout of similar stability characteristics but with less control. In spite of its excellence of control, it has shown vice in a normal spin, not responding normally to the standard control movements for recovery. The inverted flying experiments, as far as they went, did not bring to light any further abnormal features.

The "Bat Bantam" can easily be half looped into the inverted position; and as it possesses a radial engine, practically no gyroscopic effect is felt. The engine ceases to fire almost immediately on inversion. The usual degree of care has to be taken to push forward the control stick at the right time to start inverted flight; to maintain it the control stick has to be held slightly more forward than in the "Camel" or "Snipe." Except for the difference in control stick position and for the absence of rudder movement to counteract gyroscopic effect, the control movements are similar to those for the unstable aeroplanes.

The "Bat" is also exceptionally straightforward to roll either slowly or quickly. Its responsiveness allows the pilot more latitude in his control movements; and if a mistake is made in allowing the nose to drop or rise too much it can more easily be rectified. In other respects the control movements do not differ essentially from those in the "Camel" and "Snipe."

S.E.5A.—For inverted flight on the S.E.5A, which is longitudinally stable in normal flight, the best position for the tail adjustment is two-thirds forward. Fully forward it would obviously do the maximum to assist inverted flight, but the excessive nose-heaviness that it causes in normal flight introduces an awkward

condition at the commencement of inversion and during the final stages of recovery. Before inverting himself by the half looping method Bulman tried a series of normal loops, watching the airspeed indicator all the way round. Whether the loop was commenced at 100 m.p.h. or 120 m.p.h. the speed at the top did not exceed 50 m.p.h. He finally commenced his half loops for inversion at 115 m.p.h., waited till the aeroplane was just over the top of the loop, pushed the control stick right forward and throttled his engine down. The engine appeared to fire for 10 to 15 secs. after inversion; but if the inverted glide was prolonged the propeller stopped, even though the airspeed indicator showed 80 m.p.h. In any case the aeroplane stalled immediately on inversion, and the stick had to be held firmly forward while the nose dropped and the aeroplane gained speed. The greater the speed at the top of the loop, the less was the stalled drop; under average conditions the nose only fell 15° to 20° from the horizontal.

Bulman and Scholefield both performed the slow and quick half roll for inversion. They commenced the slow half roll at 75 to 85 m.p.h. at about half throttle, but throttled right down on inversion; for if the throttle was left open the engine restarted with a violent jerk during the recovery. Bulman gave rudder and aileron in the desired sense of rotation, and after the wings had passed the vertical pushed the control stick fully forward. As he approached the inverted position he took off aileron and rudder. Schofield differed by pulling up the nose slightly at the commencement of the manœuvre; and instead of continuously applying rudder until nearly inverted, gave rudder during the initial stages, centralised it as the wings passed through the vertical, and then gave it again in the same sense. He also found that he could apply it in the opposite sense after the wings had passed the vertical, which corresponds to Gerrard's experience on the "Snipe." The aeroplane did not approach stalling at any time, and Bulman, though he experienced a small amount of sideslip, found remarkably little tendency to yaw off his course. Scholefield however found that if he did not allow some yaw he experienced considerable sideslip. All the control movements in this manœuvre, allowing for the firmness required with the S.E.5A, were comparatively slow and gentle (see Fig. IV.).

Bulman commenced the quick half roll in a similar way to the ordinary roll, by giving aileron and rudder in the desired sense of rotation and pulling the control stick back, with the exception that he made these control movements rather more gently. When nearing the inverted position he pushed forward the control stick, took off aileron and centralised the rudder. At the commencement of inverted flight he found that the aeroplane was as much stalled as when looping into the inverted position. This could be mitigated to some extent if the half roll could be commenced with no more backward movement of the control stick than was just necessary to induce the rolling motion. In manœuvres such as the quick roll, which seem to involve auto-rotation, the elevator, in controlling the angle of incidence, is the important factor in governing the motion.

3 *The Effect of the Controls in Inverted Flight*

(*a*) *General*

The feature of inverted flying that made the most forcible impression on me was the distortion of the pilot's sense of speed. The inefficiency of the wings in inverted flight causes the aeroplane to fly at an angle of incidence considerably greater than that for a corresponding speed in normal flight, and the pilot to receive the impression that the speed is lower than is actually the case. If the engine is off the gliding angle is relatively poor and the aeroplane loses height more rapidly than he expects. Added to this the aeroplane stalls inverted at a higher speed, sometimes as much as 30 per cent. in excess of its stalling speed in normal flight.

The pilot cannot consider the effect of the controls until he has made absolutely certain of being able to use them. For this he must be properly supported by a shoulder strap belt. Unless his belting arrangements are as perfect as he can make them, he feels insecure; and, if he slips from his seat, a violent jerk of the aeroplane may render control temporarily impossible. With a properly fitting belt, the actual use of the controls is no more difficult than in normal flight. If the pilot intends to fly inverted, he should fasten his shoulder straps so that in normal flight he is slightly constrained. Then on turning over he will

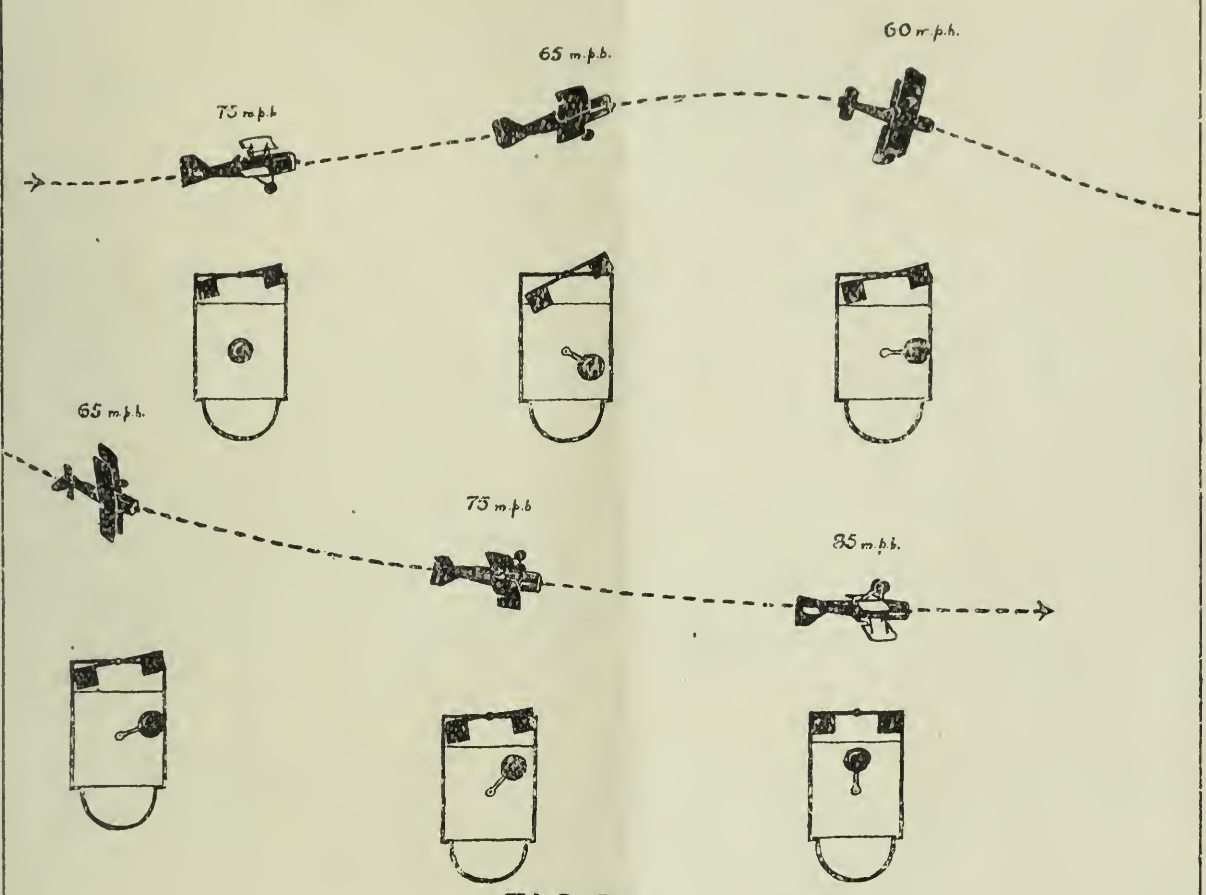


FIG. IV
SLOW HALF ROLL.
HALF ROLLING METHOD OF ATTAINING INVERTED FLIGHT.
SE5A (STABLE).

find that he is just held comfortably to his seat. Provided that he is securely belted in, he should not feel the want of toe straps on the rudder bar. The Sutton shoulder strap belt is, in spite of certain imperfections, the most satisfactory belt that I know of.

The control movements during inversion, though simple to visualise, are less easy to describe. The confusion arises from the difference in point of view of the pilot as he hangs inverted, and an observer on the ground. It is hardly necessary to add that in normal flight these points of view do not clash. If longitudinal control during inversion is considered from the ground observer's point of view, the effect of the elevator is reversed; to push the nose of the aero-

plane up, lose speed and approach stalling, the pilot pushes the control stick forward and away from him; to depress the nose (tilt it earthwards) and gain speed, he pulls the control stick back and towards him. The aeroplane answers to the control quite sensitively as long as the speed is kept at least 5 or 10 m.p.h. above stalling.

Now consider lateral control. If in *normal* flight the left wing is dropped by a bump, the pilot pushes the control stick to his right (away from the dropped wing) and depresses the left aileron to bring it up. If in *inverted* flight the left wing drops (equivalent from the pilot's point of view to his right wing) he pushes the control stick to his right and depresses the left aileron. It will thus be seen that if a wing drops, in order to bring the aeroplane on to an even keel he pushes the control stick towards the dropped wing, not, as in normal flight, away from it. From this it might be expected that aeroplanes would be difficult to hold on an even keel when inverted. The pilot will be agreeably surprised to find that this is not in general the case, neither should he experience much tendency to drop a wing if the speed is kept reasonably above stalling. His sense of balance will not desert him, he has a good view of the horizon, and after a little practice it is not difficult to keep the wings parallel to it.

If the co-ordination of the lateral and rudder controls is considered, it will be seen at the outset that a right-hand turn from the pilot's point of view is a left-hand turn as viewed from the ground. Right rudder to the pilot appears as left rudder from the ground. To perform a left-hand turn, say from north to west (from the pilot's point of view a right-hand turn) the pilot pushes with his right foot (thus putting on left rudder as viewed from the ground) and pushes the control stick to his left, thus raising the left aileron and depressing the right. Whereas in normal flight he gives aileron in the same sense as rudder, in inverted flight he gives it in the opposite sense; for a right-hand turn in normal flight, right stick and right rudder; for an inverted right-hand turn from the pilot's point of view, left stick and right rudder; and from the ground, right stick and left rudder. The control movements as described sound confusing. What actually happens is this—to produce a rotation about the aeroplane's longitudinal or vertical axes, the pilot pushes the control stick or the rudder bar in the direction that conforms to the desired sense of rotation, in a similar way to normal flight.

Time spent by the pilot in studying the effect of the controls will increase the certainty with which he can use them when he is actually in flight. To visualise the effect of the controls on the motion of the aeroplane, he will find it of assistance if he uses a small cardboard model with control surfaces that may be bent as desired. During his initial efforts at inverted flight he may also find it helpful to go up on a day with a well-defined cloud layer at about 4,000ft. He can then invert himself below the cloud layer; and by imagining this cloud layer to be the ground and *vice versa,* his control movements become simpler to understand. He uses his elevator control as if he were flying normally; to "depress" the nose, that is, tilt it towards the cloud layer which is now his "ground," he pushes the control stick forwards, losing speed however instead of, as in a normal dive, gaining it. If one wing "drops" (towards the cloud layer), to regain an even keel he pushes the control stick away from the "dropped" wing, as in normal flight. It is only when he comes to turn that the discrepancy remains. He pushes with the foot in the same sense as he wishes to turn, but instead of banking as he does in normal flight, he has to bank apparently the opposite way. He then feels as if he were on the "outside" instead of on the "inside" of the turn.

To summarise, the elevator control is reversed, but is on the whole fairly instinctive. The chief thing to be noted about it is that the movement which causes the aeroplane to gain speed in normal flight causes it to lose speed in

inverted flight, and *vice versa*. The aileron control is reversed, is the least instinctive, and to some pilots occasions slight difficulty at first. To the pilot the rudder control is not reversed, for he always gives rudder in the sense in which he wishes to turn; it is his idea of direction of the turn that is reversed.

(*b*) *Application to Particular Aeroplanes*

Standard Sopwith "Camel."—I flew the standard Sopwith "Camel" inverted and found that although it answered less sensitively to the elevator than in normal flight, the difference was less than I expected. I was unable to fly it slower than 65 m.p.h. inverted, at which speed it stalled. To fly inverted at any given speed the position of the control stick is slightly forward of that for the corresponding speed in normal flight; at 75 m.p.h. inverted the control stick is just forward of the central position. As far as I could detect, at low speeds the lateral control disappeared first, and near stalling the aeroplane showed a strong tendency to drop the right wing when inverted. If I could not check this dropping at an early stage the aeroplane frequently fell out from the inverted position in a half roll with severe sideslip. I found it quite possible to watch the airspeed indicator when inverted and fly steadily at a given speed. The tendency of the right wing to drop was particularly noticeable just after the half roll to become inverted.

I attempted to examine the stability characteristics inverted with engine off. The aeroplane was tail heavy in normal flight, and seemed to have a stable trimming speed at about 90 m.p.h. in inverted flight with engine off. This I investigated by abandoning the control stick when inverted, meanwhile steering with the rudder. The aeroplane settled down to what appeared to be a steady glide at 90 m.p.h. with "hands off." This observation, however, should be accepted with the greatest caution, owing to the shortness of the period during which the aeroplane could be allowed to glide while the trimming speed was under observation. The trimming speed was observed by producing an artificial disturbance in the neighbourhood of the supposed attitude of balance and investigating the nature of the oscillation set up. During inverted flight this is not an easy matter, as it is a physical strain to remain inverted for a sufficiently long time. It seemed however that if the control stick were pushed forward and released the speed tended to rise again towards 90 m.p.h.; and what is more important, if the control stick were pulled slightly back and released the speed tended to drop and the nose to rise; in other words the aeroplane actually wanted to remain inverted. Nevertheless the control force required to upset this condition was very small, and the "Camel" is not highly stable inverted.

The inverted stall is a matter of interest. If the "Camel" is stalled in the inverted position, it drops its nose quite definitely and shows the stalling characteristics associated with longitudinal stability; whereas in normal flight its stall shows the characteristics associated with instability, and is of a delayed and gradual character.

Modified Sopwith "Camel."—The modified "Camel" was longitudinally stable, and although tail heavy with engine on, with engine off it possessed a stable trimming speed between 60 and 70 m.p.h. After attaining the inverted position I at once noticed the difference in control stick position as compared with the standard "Camel." The control stick, instead of being just in front of the central position, was three-quarters way forward, in addition to which a noticeable control force had to be exerted to keep the nose from falling, and the aeroplane from commencing an inverted dive. The response was also less sensitive than on the standard "Camel." I then tried an inverted stall on the modified "Camel." I pushed the control stick full forward against the dashboard, and found that I could not fly slower than between 70 and 75 m.p.h. At this speed the lateral control became doubtful, and the right wing tended to drop. The

aeroplane, however, did not become properly stalled, neither did the nose heaviness decrease. At the slightest relaxation of pressure on the control stick, the nose fell and the airspeed increased. While I found it possible actually to induce an inverted spin on this aeroplane, I experienced no tendency for it to spin on its back involuntarily, even after a deliberate mishandling of the controls.

Sopwith "Snipe."—Gerrard and Bulman state that the Sopwith "Snipe" shows similar characteristics to the standard "Camel" in inverted flight. Unfortunately no detailed observations were made of the airspeed at stalling nor of whether the "Snipe" showed a stable trimming speed inverted. It responds if anything more sensitively than the "Camel" to the elevator control, and the use of the adjustable tail can be made to assist inverted flight. Like the "Camel," the position of the control stick had to be just forward of central. For a scout the "Snipe" possesses a large span, and an impression of its relatively sluggish lateral control is forced on the pilot by the "flickiness" of the longitudinal control consequent on its short fuselage. The "Snipe" showed on the whole less tendency to drop a wing near the inverted stall, and was therefore, in spite of its lateral control, easier to fly inverted at low speeds. Its relatively large rudder also assisted the maintenance of an even keel. As it is longitudinally unstable in normal flight, the same care had to be taken with the control stick in forward positions when the rudder was on.

"Bat Bantam."—Although approximately neutral longitudinally in normal flight, and with no tail adjustment, the "Bat Bantam" was found to be exceptionally controllable when inverted. Bulman actually preferred it to any other aeroplane. Its general response to control movements in all attitudes of flight examined, except that of a normal spin, was so straightforward as to give a pilot great confidence in his initial efforts. While the control stick had to be more forward of the central position than in the unstable types, there still remained sufficient margin of control to prevent the nose dropping unexpectedly.

The "Bat Bantam," due to its heavy loading, stalled inverted at as high a speed as 73 m.p.h., about 3 m.p.h. in excess of the S.E.5A. This difference in the lowest speed at which the two aeroplanes could be flown in inverted flight corresponds roughly with that of their stalling speed in normal flight. The inverted stall of the "Bat Bantam" differed from that of the "Camel"; the latter stalled sharply, the former hesitated and sank. While it must be admitted that the pilot can always produce, by handling the controls sharply or gently, a considerable variety in the kind of stall on any given aeroplane, beneath the variety he can usually detect certain permanent qualities which characterise the particular type. That the "Bat Bantam" hesitated and sank during the inverted stalling process may be accounted for by the fact that, being more stable than the "Camel" in normal flight, it showed less stable characteristics in inverted flight. Incidentally, its aileron and rudder controls were so evenly balanced and effective that the dropping of a wing could be counteracted with less trouble than on any other type. It also appeared that an error in the use of the controls near the inverted stall did not, as in the "Camel" and "Snipe," tend to induce an inverted spin.

S.E.5A.—Contrary to expectation the S.E.5A proved relatively amenable to the controls in inverted flight; its main difference from the unstable types lay in the position (about three-quarters way forward) in which the control stick had to be held to counteract its powerful self-righting properties. Scholefield found no difficulty in aileron technique to counteract a dropped wing, but was considerably puzzled by the use of aileron to carry out banked turns. As was explained previously, the bank necessary to produce an inverted turn is the reverse of that for a normal turn. Apart from this the controls had to be used as a whole more coarsely and vigorously on the S.E.5A, a feature that to some extent applies to its behaviour in normal flight. Although it was possible not only to fly the S.E.5A inverted, but even to stall it inverted, its self-righting properties were

such that, as far as investigation was able to show, no mishandling of the controls would result in the development of an inverted spin. If it be granted that the "Bat Bantam," with its extraordinary controllability, is an exception, the suppression of the risk of an involuntary inverted spin has generally to be paid for by a certain loss of ease in inverted manœuvres and by the extra force which is necessary to keep the nose of the aeroplane from falling.

Bulman found that, with tail adjustment two-thirds forward, he could stall the S.E.5A inverted at 70 m.p.h. Just prior to the stall it wobbled laterally, and frequently dropped the right wing. Though my experience relates to a different example of this type, I found that it was more often the left wing that dropped. In both cases the aeroplanes were, as far as the pilot could tell, in correct lateral trim. With the tail adjustment in the above position the aeroplane felt nose heavy both in normal and inverted flight; and although in the inverted stall the characteristics of instability were searched for, they could not be detected, being masked perhaps by the lack of elevator control. It is interesting here to note the difference between the S.E.5A and the modified "Camel," both of them longitudinally stable aeroplanes with powerful self-righting properties in inverted flight. The relatively long fuselage and effective elevator control of the S.E.5A enabled it to be stalled inverted in a similar way, apart from the greater control force necessary, to the unstable aeroplanes. The modified "Camel" had been made stable simply by a movement of its C.G. and without any modification to its tail organs except an appropriate alteration in tail setting. The control characteristics produced by its short fuselage and small tail, tolerable on a small highly unstable scout, became out of place when stability was secured, and spoilt the original unity of conception of the "Camel" design. Thus, although the modification to the "Camel" conduced towards greater safety in that its self-righting properties were enhanced, the pilot's control over it in certain attitudes of flight was reduced below what has been attained in the best example of a stable scout. The S.E.5A had even more powerful self-righting properties in inverted flight than the modified "Camel," and even then its elevator control was sufficient for it to be stalled inverted, whereas that of the modified "Camel" was not.

4 *Resuming Normal Flight*

(*a*) *General*

As for attaining the inverted position, there are two broad methods for resuming normal flight: by means of a half loop, or a half roll. The first method involves the loss of some height, say, a minimum of 300ft.; an average figure for height loss in the second method is 200ft.

To resume normal flight by means of a half loop, the pilot pulls the control stick towards him, and the aeroplane commences an inverted dive, from which it can be eased round to normal flight as in the end of a loop. But there the similarity to the second half of an ordinary loop ends. Two conditions account for this; firstly the control stick position, and secondly the speed. At the top of an ordinary loop the aeroplane is inverted, the control stick is fully back (as it is throughout nearly the whole loop) and the speed is very low, 40 to 50 m.p.h. At the commencement of the half-looping recovery from inverted flight the control stick is, depending on the stability characteristics, between just forward of the central position and fully forward, and the speed is between 70 and 75 m.p.h., slightly in excess of the inverted stalling speed. As mentioned later, the pilot has a considerable distance to pull the control stick back, during which movement the aeroplane is dropping its nose and tending to plunge into a high-speed inverted dive. Because the speed is greater than that at the top of an ordinary loop, the tendency in the recovery is for high speeds to be reached which hamper the backward movement of the control stick. The pilot therefore must handle the aeroplane gently but firmly. If he is at all uncertain in pulling the control stick back

it will gain speed in the inverted dive with great rapidity, and become harder, as explained above, to ease round and flatten out. If the aeroplane is flying quite slowly inverted, the pilot can afford, without stressing it unduly, to give a deliberate backward control stick movement calculated to swing it round to normal flight before it has gained an excessive speed. Once however it has been allowed to gain speed the pilot rightly feels that on the one hand he cannot make a sharp control movement for fear of stressing the aeroplane, and that on the other hand the gentler he is, the more will it continue to gather speed, and the longer will it take to come round.

Again, in using this method on a sensitive unstable aeroplane the pilot may find that the control stick comes back easily at first and then suddenly has a violent effect. It is necessary to feel for this carefully and damp an over violent elevator effect by easing the control stick slightly forward again. The behaviour of the aeroplane under these conditions constitutes a genuine difficulty in control, and to make a clean quick recovery the pilot has to seize his opportunity. To bring the aeroplane round in an even curve without letting it gain too much speed and lose unnecessary height, and yet without jerking it, needs, as can be seen from the above remarks, considerable care.

The other method of resuming normal flight is by means of the half roll. Whereas it is possible in most cases to attain the inverted position both by the slow and the quick half roll, to recover normal flight the pilot is compelled, except on an aeroplane like the "Bat Bantam" with its high manœuvrability, to use the slow half roll. In using the slow half roll experience shows that special care must be taken to harmonise the use of the controls with the stability characteristics of the type. On the more stable types, the best way to induce the half roll is to put on rudder in the desired sense of roll and push the control stick right forward (in any case to maintain inverted flight on these types the pilot will have the control stick well forward) and give aileron in the same sense as the rudder. As long as the aeroplane is inverted, the pilot cannot afford to pull the control stick back, for the self-righting tendency of the aeroplane will then be assisted by the elevator and the nose will drop immediately. With control stick forward and nearly full aileron and rudder the aeroplane should start to roll over. Then as it comes over the control stick is brought back; and the whole movement, allowing for the use of aileron, amounts to a circular sweep.

On a highly unstable aeroplane the pilot, in commencing the slow half roll, must be cautious in pushing the control stick forward; for if he pushes it right forward with rudder on, he sets up conditions favourable to the inverted spin. (See Part III. (1)). The further the control stick is pushed forward, the easier is the aeroplane to roll. While on unstable types the forward control stick position combined with rudder brings the pilot perilously near the inverted spin, these movements have consistently been employed with impunity by Bulman. My experience has been, however, that a reasonably good half roll can be induced by pushing forward the control stick slightly, putting on rudder, and giving aileron in the same sense. In many cases the aeroplane starts to roll out slowly and then suddenly develops a more violent motion that it is difficult to damp. This may be due to the fact that after the wings have passed the vertical the pilot is compelled to pull the control stick back to prevent the nose from dropping, and as speed has been lost in the course of the manœuvre, the aeroplane has become stalled, and auto-rotation has set in. If this happens, the pilot can only damp the motion by pushing the control stick forward again and finishing up with the nose down. On the other hand, if he keeps the control stick back, he overdoes the half roll, ends up with one wing badly dropped and is compelled to pull it up again, which is clumsy. The remedy can only lie in commencing the half roll from the inverted position with a sufficient margin of speed.

The pilot must find by experiment whether a particular aeroplane rolls out

more cleanly with the control stick relatively back or forward, the proportion of rudder to elevator that it is best to give, the correct synchronisation of the two controls, and the amount of aileron that eases the motion of the aeroplane. It is nearly impossible to recover normal flight by rolling without inducing sideslip. However sympathetically the controls are used no pilot has been able to avoid it, and a severe sideslip always means a certain height loss. In any case the slow half roll for recovery, though graceful to watch, always seems to the pilot somewhat of a compromise, and is, academically speaking, the least perfect of the manœuvres of inverted flight. As the quick half roll for recovery has only been performed on the "Bat Bantam," it is referred to in the notes on that aeroplane.

(*b*) *Application to Particular Aeroplanes*

Standard Sopwith "Camel."—To resume normal flight on the "Camel" by means of a half loop the pilot is compelled to lose some height, probably a minimum of 300ft. As explained before, he can make a quick recovery only by swinging the aeroplane round without allowing it to gather speed in an inverted dive. The "Camel" tends to go into this high speed inverted dive less easily than more heavily loaded or less unstable types. If, however, the inverted dive gains headway, the pilot may lose 1,000 to 2,000ft. before he has resumed normal flight. In my opinion it was the pilot's failure to appreciate the characteristics of this inverted dive that led to many accidents and concurrent reports that the controls were ineffective, or the elevator "blanketed." The aeroplane loses height so rapidly, and the elevator control feels so uneven in swinging the aeroplane round that it was natural for pilots to blame the control. The pilot has undoubtedly to learn what practically amounts to a trick in using the control; and if he is flying below 2,000ft. he may, if inexperienced, quite easily strike the ground in this inverted dive. The behaviour of the aeroplane appears inconsistent; under some conditions it may need a firm deliberate elevator movement and under others a series of gentle ones. The "Camel" itself is more likely to come round in a series of jerks than to plunge into the high-speed inverted dive. These jerks the pilot is inclined to check too violently, because in the various attitudes through which the aeroplane passes towards recovery the sensitivity of the elevator control varies, and he cannot easily foresee what effect his control movements are going to have. He may overdo the checking movements, in which case the aeroplane will start trying to re-invert itself, and he will lose a great deal of time and height in pulling it round to normal flight. In short, if the rapid low speed recovery is made, all is well; but if it fails, the pilot will meet difficulties that can only be overcome by practice (see Fig. V.).

An experienced pilot can resume normal flight by means of a slow half roll without losing more than 100 to 200ft. of height. He should not commence the manœuvre too near the inverted stalling speed; on the "Camel" a favourable speed is 80 m.p.h. He gives aileron and rudder in the desired sense of roll and pushes the control stick slightly forward. As mentioned before, Bulman was able to use a further forward position of the control stick than I was. Just after the wings have passed the vertical the pilot has to be careful with his rudder. Its use is determined by his effort to keep the nose of the aeroplane as far as possible in the direction in which the half roll was commenced. The aeroplane will inevitably sideslip and yaw slightly off its course. As soon as the wings are past the vertical the pilot can pull the control stick back to keep the nose up and gently or rapidly take off aileron, depending on how fast the aeroplane is coming round. If, however, he has lost too much speed, the aeroplane may swing round violently and the ailerons become useless. The motion can then be damped only by pushing the control stick forward again and allowing the nose to drop. To maintain an even rate of roll from start to finish the pilot must use good judgment in timing the control movements.

Modified Sopwith "Camel."—During the recovery of normal flight by the half looping method, the modified "Camel," as was to be expected, behaved more like the "Bat Bantam" and S.E.5A than like the standard "Camel." As soon as the control stick is pulled back from the far forward position, the nose drops rapidly, and the pilot has to aim at swinging the aeroplane round before a high speed is attained. If he hesitates during his backward movement of the control stick, the modified "Camel" does not tend to re-invert itself; it simply gathers speed in the inverted dive.

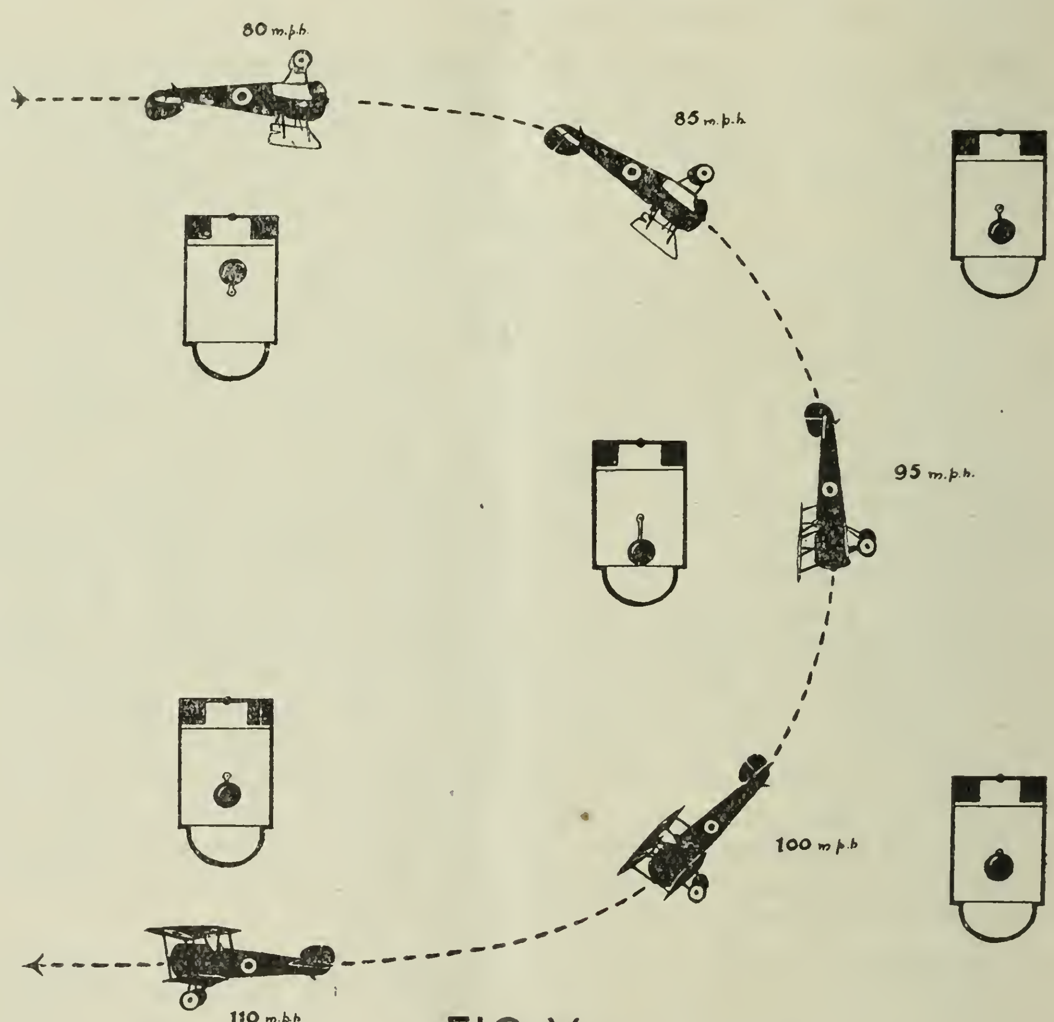


FIG. V
HALF LOOPING RECOVERY.
STANDARD SOPWITH CAMEL (UNSTABLE).

As this aeroplane shows no inclination to spin on its back, for the slow half rolling recovery the control stick may be kept well forward as rudder is given. Although some degree of sideslip and yaw are unavoidable, the manœuvre seems slightly easier to perform than on the standard "Camel," in that the timing and co-ordination of the controls need not be so delicate.

Sopwith "Snipe."—The control movements for the resumption of normal flight on the "Snipe" are similar to those on the "Camel." In the half looping method the "Snipe" shows the tendency to come round unevenly, and the same effort has to be made to achieve the rapid low speed recovery without jerking the

aeroplane. On the other hand the control stick must be brought back from just forward of the central position to full back before the speed becomes excessive.

The slow half roll for recovery presents the same problem as on the "Camel," and sideslipping is bound to occur. As the "Snipe" will go into an inverted spin with control stick right forward and rudder on, the pilot has to find for himself just how far forward he dare push the control stick to achieve the best roll without incurring the risk of an inverted spin. The similarity of the conditions favourable to the roll from the inverted position and the inverted spin is analogous to that of the conditions for the normal roll and the normal spin. This means that to attain favourable conditions for recovery by the half roll the pilot is bound to approach those for the inverted spin. It seems possible that he actually secures the conditions for the inverted spin and by some delicate economy of control movement which I have not been able to analyse, induces the half roll before the spin has scope to develop.

"Bat Bantam."—In the half looping recovery the "Bat Bantam" does not show the tendency of the "Camel" and "Snipe" to come round in jerks; it inclines rather to fall into the high-speed inverted dive. The pilot, with his control stick still further forward to maintain inverted flight, has a greater distance to pull it back to reach the correct position for swinging the aeroplane round. The "Bat Bantam," being heavily loaded, loses height very rapidly in the inverted dive, and it is particularly necessary to aim at the rapid low speed recovery.

The "Bat Bantam" does not tend with control stick forward and rudder on to fall into an inverted spin; and to induce the slow half roll for recovery the control stick may be pushed well forward. As with other aeroplanes, rudder and aileron are given in the desired sense of rotation. So effective are the controls that the half roll can be performed with an unusually small sideslip; in fact a quick half roll from the inverted position is also possible. The difference in control movement to induce the two kinds of roll seems to be one not so much of direction as of rapidity. To induce the quick half roll the control stick is pushed quickly forward, simultaneously with the application of rudder. A rapid whirling motion sets in, similar to that of the roll from normal flight; and once the aileron has been given to assist in starting the motion, it has little further effect. In the slow half roll, on the other hand, an alteration in aileron setting has an appreciable effect on the rate at which the aeroplane rolls over, and the pilot feels that he is flying the aeroplane round with the ailerons; in the quick half roll he has given aileron, stalled the aeroplane and allowed it to take charge.

S.E.5A.—During the half looping recovery the S.E.5A, in common with the "Bat Bantam," tends, if the pilot is not careful, to lose a considerable amount of height in the inverted dive. On the S.E.5A the pilot has the control stick further forward to maintain inverted flight than on an unstable or neutral aeroplane. The necessary backward movement of the control stick is therefore considerably greater, for in all the types investigated, the final position of the control stick for swinging the aeroplane round is fully back.

Although the slow roll for recovery, if skilfully performed, need only involve a comparatively small loss of height, the nose of the S.E.5A tends to drop so much that the pilot may lose as much as 700ft. in his initial efforts. When he is flying at low speeds inverted the control stick is nearly full forward. To commence the roll he pushes it along the dashboard to give aileron in the desired sense, and gives rudder in the usual way. As the wings pass the vertical the aeroplane will sideslip downwards, against its rudder. At this point the control stick must be pulled back along the side of the cockpit and, together with the rudder, finally centralised when the aeroplane has come round to normal flight. In unstable aeroplanes the corresponding control movement amounted to a circular sweep on one side of the cockpit; in the S.E.5A this sweep is elliptical, with its

major axis fore and aft of the aeroplane. The difference in movement arises from the necessity of using the elevator more coarsely. After the wings have passed the vertical the aeroplane, in addition to sideslipping, wants to yaw against the rudder. In the case of a right-hand half roll from the pilot's point of view, it yaws to the left, and *vice versa.* The tendency of the S.E.5A to yaw in the slow half roll for recovery seems more marked than in that for the attainment of the inverted position. Bulman suggests that when rolling into the inverted position, the pilot has the advantage of the engine with its consequent slipstream effect on the rudder until the last moment, whereas during recovery the slipstream effect is absent. If, by allowing the nose to drop, the pilot gains sufficient speed to compensate for this, the symmetry of the manœuvre is lost (see Fig. VI.).

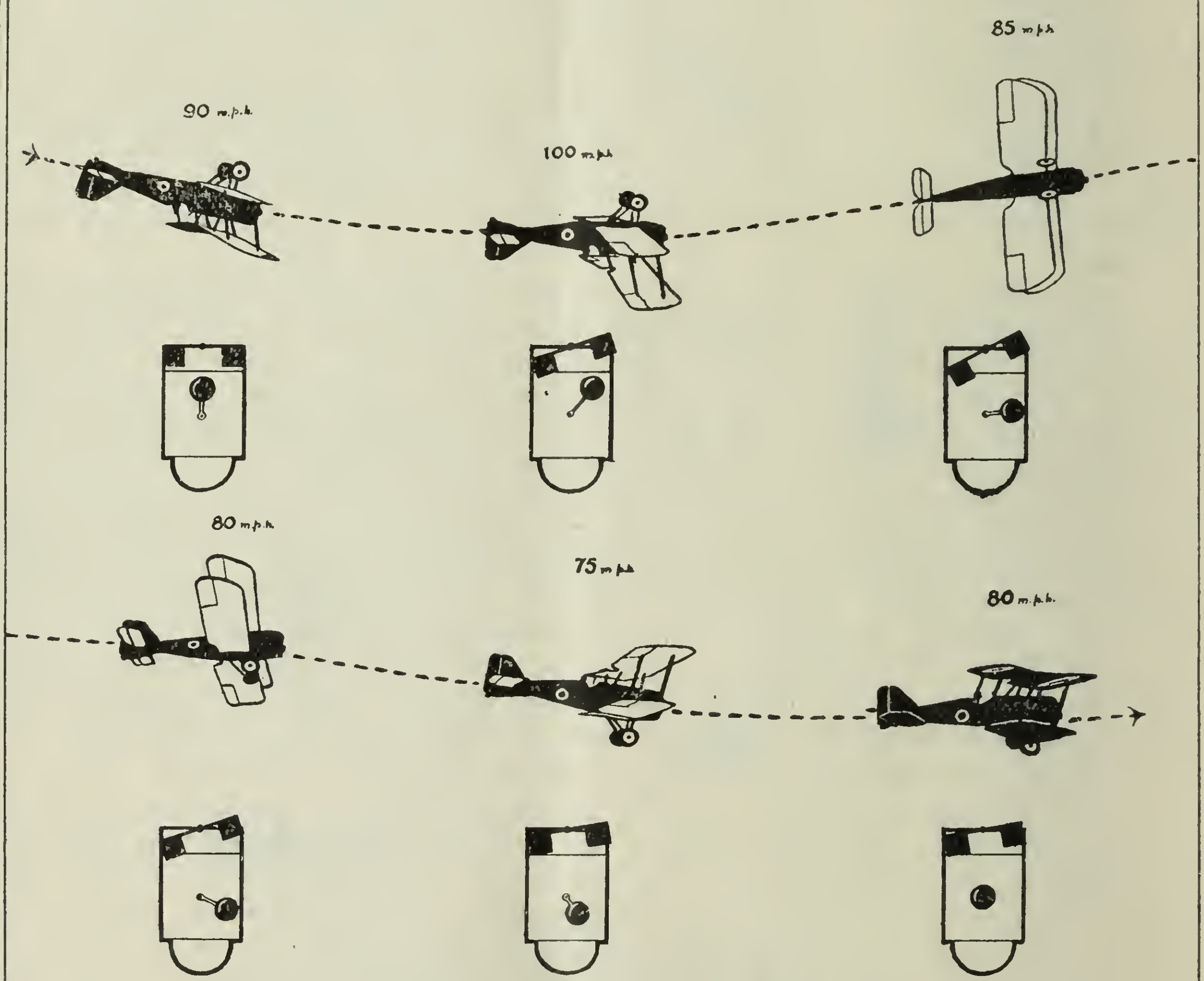


FIG. VI
HALF ROLLING RECOVERY.
S.E.5A. (STABLE).

5 *The Slow Roll*

This manœuvre is a synthesis of the slow half roll to attain the inverted position and the slow half roll for recovery. The two halves of the manœuvre should blend perfectly into a continuous whole. The control movements are therefore the same as those that have been described for the various aeroplanes in the attainment of, and recovery from, inverted flight. If an even rate of

roll can be maintained from start to finish, this manœuvre is undoubtedly the most graceful one at present known. Unlike the quick roll in which the control stick is kept well back the whole time, it cannot be associated in any way with auto-rotation. The fact that during the part of the manœuvre in which the aeroplane gradually approaches, passes through, and leaves the inverted position, the fuselage of the aeroplane remains approximately parallel with the ground, is due to the forward motion of the control stick which sets up conditions favourable to inverted flight. The rudder plays an important rôle, as distinct from the cardinal function of the elevator in setting up the stalled condition necessary to auto-rotation. Bulman felt that a more effective rudder would assist slow rolling, especially in handling the aeroplane during the awkward periods of side-slip. It was forcibly impressed on him in the following way:—The "Bat Bantam" was at one time rigged so that with the rudder bar central the rudder was offset to starboard. This meant that he had an increased range of rudder angle to port and a correspondingly decreased angle to starboard. He found that the extra rudder angle was of the greatest assistance in performing the slow roll to port. Although a rudder of larger area was at one time actually fitted to the "Bat Bantam," the rudder effectiveness, probably due to unfavourable gearing, was not sensibly increased.

Part III.—Special Inverted Manœuvres

1 *The Inverted Spin*

The investigation of the inverted spin was in a sense the crux of the inverted flying experiments. Whether aeroplanes really did spin on their backs, and if so, what the actual condition was like, had long been a subject of controversy. If inverted spins occurred involuntarily, they should be capable of being reproduced intentionally, and described. If this could be done, one of the least appreciated and most discussed problems of flying would be exposed to analysis.

In reference to accidents on "A" the report of the Accidents Committee, R. & M. 617, states:—"Thirteen accidents in which inverted flight was a feature were examined. . . . The initial stage of the accidents varied, but in 10 out of 13 cases the aeroplane at some period spun on its back."

It was hoped that the inverted spinning associated with these accidents might be reproduced; that the problem of recovery might be solved; that the behaviour of the aeroplane prior to, during and subsequent to the inverted spin might be investigated to see if the control surfaces were sufficiently operative to make consistent and repeated recovery possible; and that a pilot with his brain clear might experience the physiological effects concurrent with inverted spinning.

During the summer of 1920 Bulman was making persistent efforts to reproduce the inverted spin on the Sopwith "Camel"; but, curiously enough, he was always unsuccessful for the reason shown later. In the following autumn Gerrard joined the R.A.E. and reported that in 1917 he performed the manœuvre unintentionally while commencing inverted flight on a Sopwith "Camel" with a standard non-shoulder strap belt. To attain the inverted position he held on to the seat with his left hand, half rolled and pushed the control stick forward (the correct movements). He forgot, however, to centralise the rudder when he became inverted, and went into an inverted spin with the engine off. He was thrown from his seat, still retaining hold with his left hand. To save himself from being thrown out further, his first effort was to grab hold of the seat with his right hand also. Being confused, he seized hold of the pressure pump handle instead, which partially broke away; in fact he nearly pulled it right out. By this time he was getting very low—about 800ft. from the ground—and with a final effort succeeded in reaching the control stick with his right hand, pulled it back and made a recovery just before striking the ground. He did not proceed further with inverted flying

until he had obtained and fitted a Curtis shoulder strap belt. Since then he had frequently carried out intentional inverted spins.

On joining the R.A.E. he explained to Bulman how the inverted spin was performed, and Bulman was able to go straight up and reproduce the manœuvre perfectly. The normal method of inducing a right-hand inverted spin from the pilot's point of view is to push the control stick fully forward into the left-hand corner of the cockpit and to give full right rudder, which means that rudder is given in the desired sense of rotation and aileron against it. For a left-hand inverted spin, the control movements are reversed. In the "Camels" previously flown by Gerrard the consequent motion of the aeroplane was as follows :—The aeroplane did a flat turn inverted, dropped its nose and commenced to spin on its back, in a somewhat similar way to a normal spin. Gerrard described the recovery as not essentially abnormal, except that the controls had to be used with the greatest gentleness to prevent the aeroplane coming out with a violent jerk, and that the height loss exceeded that in the recovery from the normal spin. It is best to check the spinning, in the case of a right-hand inverted spin, by moving the control stick across from the front left-hand corner of the cockpit towards the centre of the dashboard (taking off aileron), and by centralising the rudder gradually. The aeroplane may then be eased out of the resultant inverted dive by gently pulling the control stick back. In the case of a left-hand inverted spin, the control movements for recovery are made in the opposite sense. In a good recovery the height loss should not exceed that from a normal spin by more than 20 per cent. ; in a bad recovery it may run into thousands of feet (see Fig. VII.).

The "Camel" on which Bulman had been experimenting showed a strong tendency at low speeds to drop the right wing inverted [Part II. (3) (*b*)]. Gerrard flew it and considered that this tendency was more marked than in the "Camels" which he had previously flown. It was this individual peculiarity that seems to have confused Bulman in his initial attempts at inverted spinning. Gerrard found that when the control movements for the inverted spin were made this aeroplane, instead of flat turning on its back and sliding into a spin immediately, tended to drop the right wing and roll round the right way. If the controls were still held over, it rolled over again on to its back and commenced an inverted spin, by which time the nose had dropped to about 45 deg. with the horizontal. In attempting the inverted spin Bulman, finding that the aeroplane rolled round right away, immediately straightened it out in the belief that he had failed, and never guessed that if he persisted an inverted spin would result. Nevertheless when I tried the inverted spin on this aeroplane, in no case did I experience the tendency to roll out before the spin had commenced, which must have been due to a slight difference in handling the controls. Bulman lays emphasis on the extraordinary smoothness of the inverted spin. There is no suggestion of "kicking" round, a frequent feature of the normal spin on the "Camel." He also noticed that by taking off rudder slowly he could decrease the rate of rotation.

Although in my initial attempts at inverted flight on the Sopwith "Camel" I had several times mishandled the controls, it so happened that I had not, as a result, commenced an involuntary inverted spin. As I was anxious to gain experience of the manœuvre I carried out a considerable number of inverted spins. I judged that it would be a matter of considerable difficulty to read instruments in this condition ; in any case I was compelled to concentrate on reading one at a time. In ordinary inverted flying it is possible to read even two or three instruments at once. Having previously shut off the engine and petrol, I commenced inverted spinning by half rolling gently on to my back at 5,000ft., and producing an inverted glide. I steadied the airspeed to 80 m.p.h. and then gently eased the control stick forward and to my left, and gave full right rudder. The aeroplane showed no hesitation in answering. It did a flat turn

to the right, and started spinning inverted with the nose fairly well down, in a roughly similar attitude, except for the inversion, to that of a normal spin.

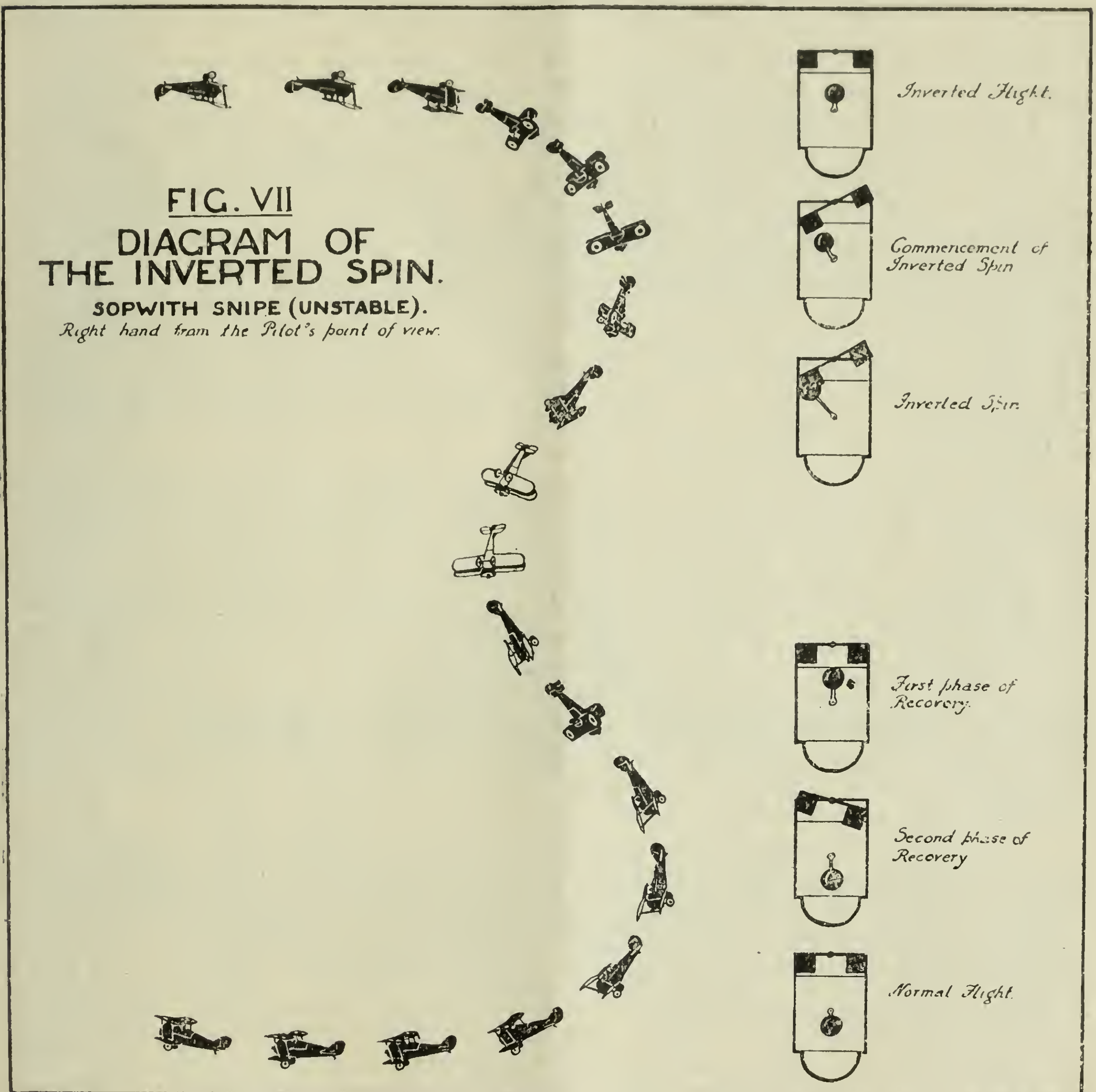
I agree with Bulman that the inverted spin is unlike the characteristic "Camel" spin; it seems both smoother and slower, and there is no "kicking" noticeable. As soon as the spin commenced I felt the negative acceleration increase, and the belt stretched a little. I was able, however, to read the airspeed, which settled down to 90 m.p.h. The pitot head is mounted on the starboard wing strut, which would be the inner one in the spin. Its reading in an inverted spin would probably be subject to a considerable correction.

FIG. VII

DIAGRAM OF THE INVERTED SPIN.

SOPWITH SNIPE (UNSTABLE).

Right hand from the Pilot's point of view.



After spinning for 1,500ft. I gently eased the control stick towards the centre of the dashboard and then backwards, simultaneously centralising the rudder. After what seemed like one to one and a half turns the rotation slowed up, started again momentarily, and then ceased; after which I emerged in an inverted dive. In this dive I attained a considerable speed, over 160 m.p.h., being reluctant to handle the aeroplane roughly and flatten out too violently. This procedure, however, involved the sacrifice of about 1,500ft. of height, and my nose crosses the

horizon at 2,000ft. Just as in the half-looping recovery from the inverted position, the speed in the inverted dive tends to rise very rapidly, and it is essential always to aim at the low speed recovery. In such dives I have more than once attained speeds in the neighbourhood of 200 m.p.h. During the inverted spin proper the pilot, though subject to violent negative acceleration, is quite able to realise where he is and visualise his attitude; during the recovery and consequent reversal of the sign of apparent gravity, he tends to feel dizzy for a few moments.

One of the most important things to discover about the inverted spin was at least an approximate notion of the loads imposed on the wings, and hence the margin of strength. I was therefore anxious if possible to measure the acceleration in the spin, and had a simple type of spring accelerometer (non-recording) fitted between the guns so that it was close to my face and easy to read. This accelerometer was used by Lindeman in his early spinning experiments. I commenced inverted spinning at 5,000ft. in the same way as before. The indicator of the accelerometer appeared to rise steadily but quickly to -2G, and to remain there for as long as I was able to watch it. At about 3,500ft. I had intended to make the control movements for recovery. I was, however, beginning to be confused, and think that while centralising the rudder I may have kept the control stick forward or have failed to pull it back enough. When I next appreciated what was happening I was gliding fast on my back at about 120 m.p.h., side-slipping with my right wing down. It took me a few moments to decide whether I was inverted or not, as, having been subjected to large unusual accelerations, my sense of feel seemed partially paralysed. On realising the position I gently eased the aeroplane into an inverted dive and recovered normal flight at just under 2,000ft. I subsequently read the accelerometer in a straight inverted glide, and its reading was approximately -1G.

Up to that time it had been an open question whether it was possible to pass direct from an inverted spin to inverted flight, the normal recovery being an inverted dive. That I did so unintentionally proves the possibility of it; indeed the manœuvre virtually amounts to the first phase of an inverted loop. It was unfortunate that I was unable to control myself sufficiently during this period to read the accelerometer, as the reading should have been of interest. The fact that I passed direct into the inverted position in trying to recover from an inverted spin, seems to point to the persistence of habit formed during normal spinning. the recovery from which requires the control stick forward. Although I had clearly thought out the whole manœuvre beforehand, yet when the time came for recovery I failed to make the subconscious effort to pull the control stick back.

I carried out another right hand inverted spin and was able to watch the spring accelerometer more carefully. During the spin the indicator oscillated between -1.8 and -2.2 G., the period of the oscillation being approximately in phase with the rotation of the aeroplane. Bulman then carried out two left hand inverted spins and observed a similar oscillation between -1.6 and -2.4 G. Later on a recording accelerometer was fitted up and a record that I obtained in a right hand inverted spin is shown diagrammatically in Fig. VIII. For purposes of comparison a record of a straight inverted glide is shown in Fig. IX. The maximum force on the wings during the inverted spin is seen to be -1.7 G., a figure lower than that obtained from the non-recording spring accelerometer. Taking into account the conditions under which the latter had to be observed by the pilot, great reliance cannot be placed on the readings taken from it.

It was possible to induce an inverted spin on the modified "Camel" by the same class of control movement as that for the standard "Camel." The controls, however, especially the elevator, had to be used more vigorously to prevent the nose from dropping before the rudder came fully into play. The inverted spin felt similar to that on the standard "Camel" except that the rotation seemed

FIG. VIII

INVERTED SPIN

STANDARD SOPWITH CAMEL. ENGINE SWITCHED OFF.

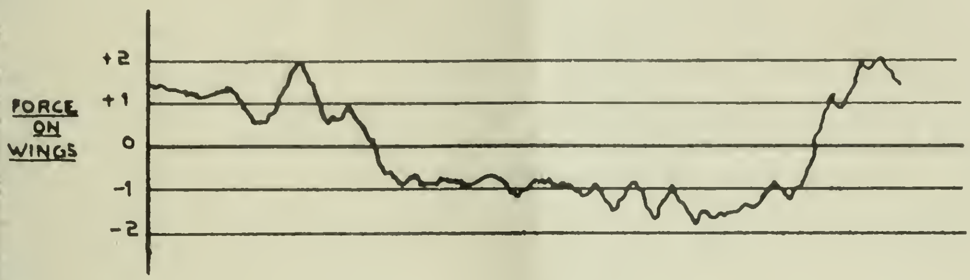
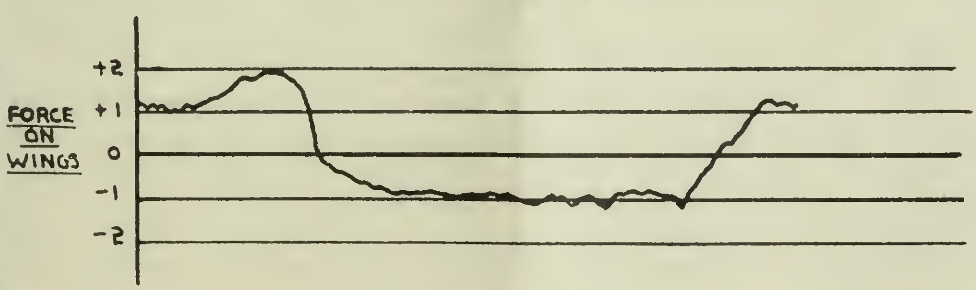


FIG. IX

INVERTED GLIDE.

STANDARD SOPWITH CAMEL. ENGINE SWITCHED OFF



NOTE These diagrams were copied from Accelerometer records

faster and the recovery more certain. When the controls were set for recovery, instead of answering sluggishly, the aeroplane answered immediately.

Gerrard reported that he had carried out inverted spins on the Sopwith "Snipe," and that the smoothness remarked in the standard "Camel" was a feature. The control movements for entry and recovery were similar to those that have been described for the "Camel." It is also known that the "Snipe," if mishandled, shows a tendency to fall into an inverted spin in the same way as the standard "Camel."

Considering the abnormal behaviour of the "Bat Bantam" in an ordinary spin, the greatest caution would have been necessary in attempting the inverted spin. All that was actually established was its reluctance to fall into an inverted spin near the inverted stall, in which respect it behaved in a similar way to the more stable aeroplanes. There is, however, no doubt that it was fully capable of inverted spinning, had the manœuvre been deliberately attempted.

Finally I tried an inverted spin on the S.E.5A. I set the adjustable tail at its maximum incidence, thus producing considerable nose heaviness in normal flight. I made the usual control movements and the aeroplane entered an inverted spin in much the same way as the modified "Camel." The rate of spin was less smooth, with a noticeable "kick." The S.E.5A showed no tendency to fall into the inverted spin; in fact the pilot had to be determined with the controls to produce it at all. Once produced it was, in spite of the "kicking," perfectly definite and consistent. The recovery was more direct than on any other of the aeroplanes examined.

The conditions favourable to the inverted spin required that rudder should be applied when the control stick was in a far forward position, or conversely that the control stick should be pushed forward when the rudder was across. When carried out intentionally, the inverted spin had always been produced with control stick forward, rudder given in the desired sense of rotation and aileron against it, by analogy with the normal spin. In normal spinning, however, it has been found that the ailerons are the least important factor in exciting or governing the motion, and that aeroplanes can readily be spun with ailerons neutral or given in instead of against the desired sense of rotation. It seemed natural that this should be true of the inverted spin. I therefore made tests on the standard "Camel" to investigate the influence of aileron. I first tried to induce an inverted spin by flying inverted, pushing the control stick forward without giving aileron and applying full rudder. The "Camel" did not appear to respond quite as readily as with aileron given against the sense of rotation, but fell into an inverted spin for about a turn and a half, threw itself violently out and began to gather speed. I then tried again by inducing an inverted spin with aileron against the sense of rotation and slowly moving the ailerons during the spin until they were fully over in the same sense as the rotation. The spin slowed up slightly and became less even, but still persisted until I made the movements for recovery. These tests showed that for inverted spinning the position of the elevators and rudder was more important than that of the ailerons. This point has a distinct bearing on the chances of falling into an involuntary inverted spin by a mishandling of the controls.

It will be seen that the pilot, when attaining the inverted position or resuming normal flight must, with aeroplanes showing a natural tendency to spin inverted, be cautious in approximating to the conditions favourable to the inverted spin. In no manœuvre to attain inversion or to recover from it does the pilot need to have the control stick forward with rudder and ailerons "crossed"; but in attaining inversion by the half looping method there is just the possibility of an overlap of full rudder with control stick right forward and ailerons neutral, and in attaining inversion or recovering from it by the half rolling method the control

stick may be right forward with aileron given in the same sense as the rudder. It is clear that although aileron against the rudder is apparently the most favourable to inverted spinning, so long as the control stick is forward with rudder across, the conditions essential to inverted spinning are, in highly unstable aeroplanes, satisfied. As was mentioned in Part II., the more unstable the aeroplane the more careful will the pilot have to be. On the other hand, with aeroplanes that are neutral or stable he will probably experience no difficulty and be able with impunity to push the control stick right forward with rudder full across. If he finds it possible he should use this elevator-rudder combination unhesitatingly, as it is a powerful weapon for inducing good half rolls from or into the inverted position.

The physiological effects of the inverted spin do not seem to be at any one moment as severe as those of the inverted half loop (Part III. (2)), although the pilot is tending to be thrown from his seat and naturally feels the rush of blood to his head. These effects are nevertheless longer sustained and appear to be harder on his physique mainly because, although the force of apparent gravity may be no greater, the reversal of its sign produces a very unnatural sensation. The most violent reaction occurs neither during the inverted spin nor at the period of transition from the inverted spin to the inverted dive; it occurs when the pilot is flattening out from the inverted dive, presumably during an acceleration in which the force of apparent gravity is positive. As the pilot has been subjected to forces of the opposite sign, the reaction due to such an acceleration, even though of moderate intensity, is unusually severe. A frequent effect on the pilot is that for a moment or two everything goes black; but the sensation is accompanied by no pain.

The inverted spinning experiments proved definitely that recovery from an inverted spin is not inhibited by ineffectiveness of the elevator or rudder; and that spinning can be checked by centralising the controls, after which the aeroplane can be gently eased out of the resultant inverted dive by a backward movement of the control stick. This points to the conclusion that pilots who failed to recover either were prevented from making any consistent and reasoned control movements by a physical condition induced by the accelerations of the aeroplane, or fell out of their seats due to inadequate belting arrangements, and were unable to reach the controls. If they could have steadily held the controls anywhere near their central position, spinning would have ceased, and a gentle pull on the control stick would have righted them. The utter confusion of an inexperienced pilot when spinning inverted is not, however, difficult to imagine, especially when, even assuming he were held to his seat, he had no idea of the requisite control movements. Such a pilot, subjected to the onset of violent accelerations, would be unlikely to hold the controls central; unless he had by systematic mental drill been definitely taught what it was necessary to do to recover from such abnormal conditions, he would probably make wild coarse control movements; and, as if this were not enough, the control movement (pushing the control stick right forward) that ensures recovery from a normal spin is the very movement that induces and maintains the inverted spin. Hence the danger.

2 *Possibilities of the Inverted Loop*

Bulman and Sainsbury experimented on the "Bat Bantam" to investigate the possibility of carrying out a complete inverted loop. I believe that Pégoud performed the first half of an inverted loop in 1912 on a Blériot monoplane, but that since then the manœuvre had received no serious attention. For these experiments the pilots strapped themselves in so tightly with shoulder strap belts that the discomfort would have been too severe for a flight of long duration.

Sainsbury also made use of the fairing behind him, under which he was able to wedge his shoulders.

By the term "inverted loop" is meant a loop in which the undercarriage of the aeroplane is inside the loop, that is towards the centre, and in which the loads on the aeroplane are the reverse of a normal loop. When the aeroplane became inverted the engine (a Wasp Radial) ceased to fire almost immediately. Bulman and Sainsbury first tried to estimate the speed that would be required for the performance of an inverted loop. They noted the fluctuation of speed during a normal loop commenced at 100 m.p.h. with engine on, and the speed at the top of the loop was observed to be 55 m.p.h. With a fast loop commencing at 120 m.p.h. a minimum speed of 60 m.p.h. was noted. Secondly, the stalling speed of the aeroplane when inverted was examined and found to be approximately 73 m.p.h. While the lateral control when inverted was quite satisfactory above speeds of 80 m.p.h., below that speed it became sloppy and was soon followed by the longitudinal control. Comparing the stalling speed of the "Bat Bantam" inverted (73 m.p.h.) to its stalling speed in normal flight of approximately 52 m.p.h. the inefficiency of the wings in inverted flight was apparent. This speed of 73 m.p.h. would have to be exceeded during the critical period of the inverted loop.

Bulman and Sainsbury next attempted to perform the second half of the inverted loop. In order to do this it was necessary to start from the inverted position. The "Bat Bantam" was flown inverted at as high a speed as possible without inducing too much of an inverted dive, on parallel lines with the dive sometimes made to gain speed for a normal loop. A favourable commencing speed in inverted flight proved to be 130 m.p.h. When the pilot had attained this speed on his back he pushed the control stick fully forward; the aeroplane recovered a little height, but lost speed rapidly and nearly stalled, the speed having fallen to about 75 m.p.h. This was tried a number of times, but in no case did the aeroplane make a serious attempt at completing the second half of the inverted loop (see Fig. X.). Attempts were then made to perform the complete inverted loop by pushing the control stick forward when the aeroplane was in normal flight. The commencing speed varied from 50 m.p.h. to 130 m.p.h., both with engine throttled down and full out. In each case the aeroplane passed the vertical and completed an inverted half loop; but with the control stick kept fully forward the aeroplane, instead of continuing round, lost speed very rapidly, stalled and the engine ceased to fire. Except for a more rapid loss of speed, its behaviour was exactly similar to that described in the previous experiments in which the second half of the inverted loop only was attempted (see Fig. XI.). The speed at which the inverted loop was commenced did not appear to make much difference, as at high speeds the pilot did not dare to use the controls violently for fear of stressing the aeroplane unduly, and increasing the already large force tending to break his belt and throw him from his seat.

It is possible that had special arrangements been made to enable the engine to run inverted, the aeroplane might have been made to complete the inverted loop. Considering, however, the heavy stressing to which the aeroplane had inevitably to be subjected during this manœuvre, the experiments were not pushed further. Whereas a maximum force of $-1.7G$ was obtained for an inverted spin, it is possible that the force on the wings during an inverted loop might reach a considerably higher figure.

I made one attempt with the spring accelerometer to measure the acceleration during the first half of the inverted loop on a Sopwith "Camel." I produced the manœuvre in the most gentle way possible, by stalling the aeroplane sharply in normal flight, allowing the nose to drop and pushing the control stick quickly forward, thereby hoping to swing the aeroplane round before the speed rose excessively. This I was successful in doing, but was unfortunately unable to

ATTEMPTS AT INVERTED LOOPING
BAT BANTAM AEROPLANE

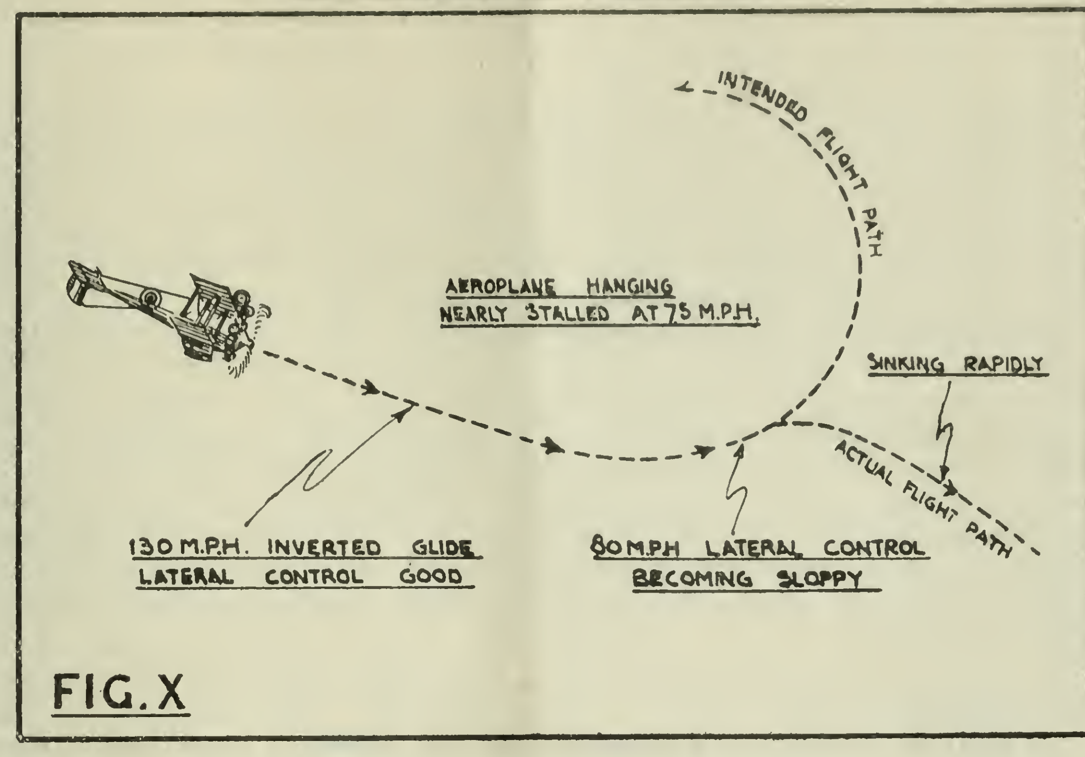


FIG. X

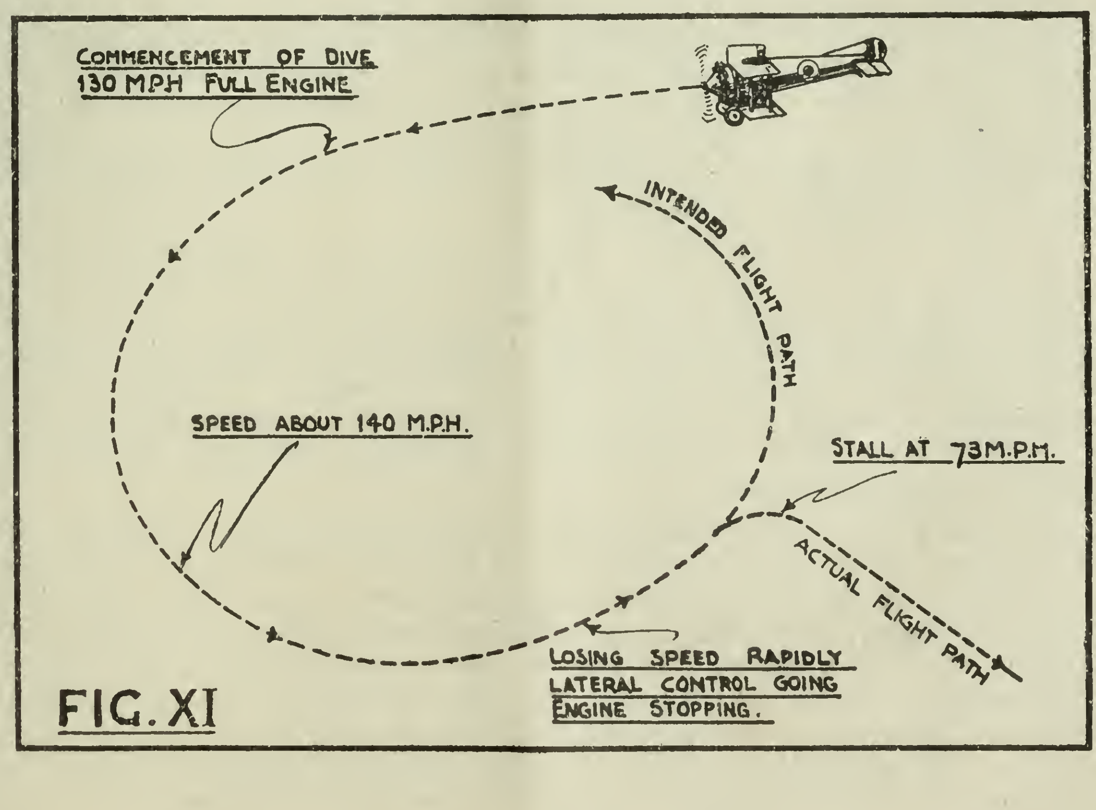


FIG. XI

keep my eyes on the accelerometer, and so failed to obtain a reading. Incidentally I must have applied rudder as I approached the inverted position, for the aeroplane fell sharply into an inverted spin.

The first half of an inverted loop can be regarded as yet another method of attaining the inverted position; as, however, it contains special features of interest, I have treated it separately. Although in the second half of an inverted loop, even could it be performed, it is difficult to see any military value, in the first half there are undoubted possibilities. In combination with a half roll it could be utilised for a rapid change of direction. The pilot would be flying in one direction, perform an inverted half loop, fly for a moment inverted and then half roll out flying in the reverse direction. This manœuvre would actually seem a more rapid way of reversing the direction of flight than that of turning with a vertical bank. In aerial combat, the attack is often made in a steep dive. Almost as often it happens that the pilot of the diving aeroplane is compelled to cease firing because his opponent's aeroplane, flying in the opposite direction, passes underneath him. If the attacking pilot could continue into an inverted dive, still firing, pass into the inverted position and half roll out, he would find himself flying behind his opponent's tail and in the same direction, with further favourable opportunities of attack. The possibilities of the inverted half loop remain to be proved; but it is certain that a fighting scout, capable of performing any given manœuvre and designed for war, will bring with it the pilot determined to explore its value as a weapon of offence. It is therefore essential that the designer, breaking away from present-day tradition, shall foresee future developments in aerobatics, and as far as possible allow for them in the strength of the design.

3 *Conclusions*

(*a*) That the belting arrangements of any fighting scout, more especially an unstable one, are of supreme importance. So far as I know, a belt has not yet been evolved which simultaneously fulfils the three necessary requirements—firstly, that of holding the pilot securely in his seat during inverted manœuvre; secondly, that of giving him the freedom to look round over his tail that the proper management of a single-seater fighting scout requires; and thirdly, that of transmitting the shock of a crash to his body in the manner least calculated, under the circumstances, to injure it.

(*b*) That on all the types of aeroplane open to investigation it was possible to maintain steady inverted flight. The chief difference lay in the absence of self-righting properties in the longitudinally unstable type, and the presence of powerful self-righting properties in the longitudinally stable type. To maintain inverted flight on the different types a marked difference in control stick position was evident. Though if anything more longitudinally stable than the modified "Camel," the S.E.5A was the more controllable aeroplane in inverted flight because of its effective tail.

(*c*) That there is no tendency for an unstable aeroplane to remain inverted in the sense that an experienced pilot has any real difficulty in recovering from the inversion. Recovery is prevented by inability to reach the controls due to inadequate belting, by confusion in handling them, or by an insufficient margin of height. With a knowledge of how to use it the pilot has control sufficient for ultimate recovery from all attitudes of flight. The least favourable condition is that of the high-speed inverted dive. Time and height are then necessary for recovery. Though it is suggested that the standard Sopwith "Camel" may be stable inverted, it is a fact that no control forces of any magnitude are requisite to overcome the effect of this stability.

(*d*) That whereas on all types of small scout can inverted spinning be performed intentionally, the longitudinally unstable type only is liable, if mishandled

during inversion or recovery from inversion, to fall into an involuntary inverted spin; that provided the pilot appreciates the correct control movements to make, recovery from the inverted spin presents no insurmountable difficulties; that an error in making the correct control movements for recovery leads to the loss of a far greater interval of height than in the case of a normal spin; that a minimum of 1,500ft. should be allowed for recovery, instead of the usual 400 to 600ft. for a normal spin; and that even 1,500ft. is a narrow margin if the possible physiological effects on the pilot are taken into consideration.

(*e*) That so long as there existed a school of thought which held that longitudinal instability was an inalienable accompaniment of good longitudinal control it was natural that this instability should be tolerated as a necessary evil; that the foregoing experiments indicate that all the manœuvres of inverted flight, excepting perhaps the inverted half loop, can be satisfactorily performed on a highly stable aeroplane, provided that its control organs are well designed; that the best compromise between safety and extreme manœuvrability is to be found in an aeroplane which, though preferably stable throughout the major part of its range of flying speeds with elevators free, must definitely be stable with them fixed.

(*f*) That judging by the steady rise in the standard of aerobatic performance required of the scout pilot, it is evident that this standard will not only be maintained, but rise yet higher; at least higher in the sense that what the experienced and matured pilot does to-day, the novice does to-morrow; that the manœuvres of inverted flight will take a prominent place in the curriculum of every flying training school; that because in peace time they train the pilot's judgment, make his hand sensitive, stiffen his nerve, they will adequately prepare him for the more serious issues that every service pilot may one day have to face; and that the above contentions are a wholly reasonable answer to the question, if it should be asked: "What is the use of inverted flying?"

(*g*) That in the design of a fighting single-seater aeroplane, specific account should be taken of inverted manœuvre. Because in the manœuvres contemplated when the aeroplanes that exist to-day were designed, this was hardly more than an incidental possibility, there is a widespread lack of confidence in their margin of strength against the loads imposed during inversion. Even were such anxiety ultimately to be proved ill-founded, its very existence justifies an adequate study of the manœuvres of which in time to come a fighting aeroplane may be capable, and its practical application to stress calculation of the future.

The CHAIRMAN, before opening the discussion, said he could not help thinking that everybody present must wonder which of the three or four outstanding qualities of Squadron Leader Hill was the most remarkable—as a pilot, as an observer of flying manœuvres, as a writer or as an orator. He believed that Squadron Leader Hill would gain most satisfaction from his capacity as an observer than from either of the others, but there was no doubt that anyone who heard him speak on this extremely interesting subject—one which was not exactly incomprehensible, but was very difficult to follow without a lot of experience—would wonder at his clear mind. Generally speaking, pilots were extremely good as observers, and it was only when they plunged into design and tried to alter their aeroplanes themselves that they broke down. As observers, it was perfectly extraordinary how pilots could follow out what they did and what happened in an aeroplane during the most complicated manœuvres. As far as he himself was concerned, he had never attempted any such feats and manœuvres as those so simply talked about by Squadron Leader Hill. On one occasion he had gone up in order to observe some particular phenomenon which had been bothering himself and others, and which involved the machine in a spin, but he was sorry to say that he was unable to give any clear account of what did happen. (Laughter.)

Continuing, he said he did not know whether everybody was aware of the origin of the paper which Squadron Leader Hill had presented. It was written, in the first place, for the Aeronautical Research Committee, but it had struck everybody on the Committee as being so excellent for general publication that permission was obtained to have it presented to the Royal Aeronautical Society. There could be no possible doubt that publication of the story of this work could do nothing but great good, by the simplification, for the benefit of pilots, of all the various manœuvres and difficulties which were still standing in the way of flying. He could remember quite well the time when "stalling" was entirely misunderstood; all that was known was that the machine would fall to the ground, as people said, without any apparent reason at all. Gradually these difficulties that pilots experienced were being cleared away and simplified. It was by the experiments made by Squadron Leader Hill and his staff of experimental pilots that almost the last obstacle was cleared. A very small amount of public thanks or reward came to these men, but he could not help thinking that there was amongst themselves a feeling of great satisfaction in the knowledge that what they had done would be of great use in the future. Finally, the Chairman asked anyone who had any remarks to make—and he saw one, if not two, of the pilots who had taken part in the work, present—to say something about his experiences.

Discussion

Wing-Commander T. R. Cave-Browne-Cave, in opening the discussion, said that, unfortunately, he could not contribute anything to it from his own personal experience. This investigation was a remarkably complete one of a very difficult problem, and the solution which Squadron Leader Hill had presented showed exactly in what the difficulty lay. There was no serious trouble in getting out of the inverted position, or in controling one's machine whilst in the inverted position, provided one knew what to do and could also think clearly. The value of the investigation was that the way in which the result was presented was so clear that it should be possible for pilots to keep their minds clear and to remember exactly what they had to do if they got into the inverted position unexpectedly. He admitted that it was difficult to think clearly under these conditions, but if it could be done, then, apparently, there was no real difficulty. The diagrams which Squadron Leader Hill had shown, he considered excellent. They were most important, and he considered that the value of the lecture would be prejudiced seriously if they were not reproduced in the journal.

The lecturer had referred to "left" and "right" wings, and "left" and "right" rudder, as distinct from "port" and "starboard." "Port" and "starboard" were accepted terms, both in the Air Force and in the "Engineering Standards Glossary." An advantage of using them in this respect was that if one spoke of "port" and "starboard" they obviously referred to terms as appreciated by the pilot. If one saw a ship in the distance turning to port, one always described it as turning to port, as seen by the Captain of the ship, however it might appear to the speaker. The terms "port" and "starboard," being somewhat more formal than left and right, would make it clear that one was using what was obviously the only logical system of terms, *i.e.*, the terms as appreciated by the pilot himself. "Up" and "down," he admitted, were a little more difficult, but it would be wise to adopt the terms "port" and "starboard."

There was one point mentioned by the Chairman, which he wished strongly to support, namely, the thoroughness of this investigation. The actual flying work, he believed everyone would agree, was "a brave business," but Pegoud, for instance, had done that when aeroplanes were in a very much more primitive form than they are to-day What had struck him about the present investigation was that it was scientifically carried out and very carefully observed. The deductions were correctly drawn from the observed facts, and had been presented by

Squadron Leader Hill in a very full and extremely clear paper, which finished up with the conclusions which he and the pilots who had worked with him had drawn from them—a very important feature—and a guide as to what work should be done next. As a complete investigation of a very difficult problem, this must be taken as a model.

Flight Lieutenant T. E. B. Howe, referring to the lecturer's statement that, in the design of the new fighters, specific account would be taken of stresses due to inverted manœuvre, asked whether the present machines, such as the Sopwith "Snipe," were suitable for practising inverted flying which many of those present would be encouraged to do as the result of the lecture.

The Chairman said that he did not think there was much doubt that the Sopwith "Snipe" had plenty of strength for ordinary upside-down flying.

Flight Lieutenant Howe said that he had noticed that the landing wires were not so strong as the flying wires; and that, in the accelerometer diagram shown by the lecturer, the stresses of an inverted spin were shown as —2.

The Chairman replied that the safety factor of a "Snipe" was 5—6, and he should think, when flying upside down, it would be about 4.

Flight Lieutenant Howe said he would like to know whether the present fighters were suitable for inexperienced pilots to practise inverted flying.

This question the Chairman was not prepared to answer. But in his closing remarks the Lecturer made it clear that he thought it better to try these manœuvres intentionally rather than to be unprepared for a possible unexpected experience of that sort.

Flight Lieutenant Bulman said he was rather of the opinion that Squadron Leader Hill would come in for severe censure by that large and influential body, the Pilots' Union; all their stock in trade had been given away, and nothing was left secret. (Laughter.) Coming to more serious things, however, he asked if one realised the difficulties which Squadron Leader Hill had in co-relating the various reports from the pilots who had tried some of these inverted flights. Nearly all pilots had different ideas, and expressed them in different ways, and it was not until some of them tried to practise some of these manœuvres in formation that they really understood the difference in their methods of performing what was apparently the same manœuvre. Taking, for example, the apparently simple half roll to attain the inverted position, he remembered that the first time they tried this together they were in "line ahead," and thought it would be quite easy, because the man behind could just follow the man in front of him. However, the slowing up of the first machine as soon as it started the manœuvre was extraordinary, so much so that the second man simply dare not attempt to perform the turning over, for fear of running into the man ahead. That was quite a fallacy, however, because he in turn also slowed up, but it certainly seemed almost impossible to avoid hitting the man in front. Then there was the question of the lateral displacement during the half roll. From the ground observer's point of view the wings appeared to turn about the longitudinal axis of the machine. Actually the aeroplane rotated about a point outside its wing tip and finished the manœuvre on a course parallel to its original flight path, but some 20 or 30 yards to one side of it. The inefficiency of the aeroplane when inverted was also very marked when flying in formation. A pilot, fearing he was getting below the machine in front of him, would push his stick forward. Instead of the aeroplane rising it actually descended more rapidly unless the air speed were 20 m.p.h. or so above the stalling speed inverted.

Captain G. T. R. Hill said he could not speak from any extended personal experience of upside-down flying. The only occasions on which he had indulged in it were when his machine had become inverted by mistake, and then it only lasted

for a very few seconds, because he had always rectified matters as speedily as possible.

What had struck him in the paper, apart from the sections dealing with the technique of flying upside-down, was the relation, which was insisted upon again and again, of the power of the controls to the stability of the machine. They had heard described the case of one aeroplane that was stable, and the controls had a certain effect, while another aeroplane, which was unstable, was contrasted with it, and the controls had an entirely different effect; this difference was much more than one might have expected, judging by the relative sizes and areas of the control surfaces. That seemed to bring out one of the great difficulties in trying to judge the effectiveness of the control by examining the data which is available in the greatest quantity, namely, that from the wind channel. In most cases these data were obtained from happenings in an aeroplane situated at one point, at a steady speed, whereas in an actual manœuvre the conditions were changing continuously, and at present, when we wanted to think about control, we had only very limited data with a direct bearing on the problem at our disposal. Even these data did not lead us very far, and this paper by Squadron Leader Hill had made a great addition to our knowledge in this direction.

He had been carrying out some preliminary experiments on the measurement of control, and, although he had only just started, some of his results had confirmed by actual measurement one of the statements made by Squadron Leader Hill—which is now really a generally accepted phenomenon—that in the stalling of an unstable aeroplane when the nose was pulled slowly up, the speed would drop off, and it would hang for quite a long time before putting its nose down, in contradistinction to the behaviour of a stable aeroplane, which stalled and put its nose down comparatively quickly. Some of the records he had obtained on his Bristol Fighter, which was unstable, showed that when the aeroplane was stalled quite slowly it would hang for several seconds, the nose, perhaps, falling by only two or three degrees, which was hardly noticeable, before it really puts its nose right down, and that behaviour, he saw from the paper, was attributed to the S.E.5 when flying upside-down. When the S.E.5 was right way up it was stable and would stall suddenly, but when upside-down it was unstable and stalled gradually. Although two aeroplanes of the same size of controlling surfaces, to all appearances, might at first sight be expected to behave in the same way, the question of the stability did influence the behaviour a great deal. The paper emphasised that again and again, and he considered that it should be thoroughly absorbed by all people who had to deal with the problem of control.

The overpowering effect of the stability, or the "aeroplane's own will," over the control by the pilot led, obviously, to a state of danger. Supposing we could ensure that the control was so effective that the pilot could always overcome whatever the aeroplane wanted to do, we should be in a much happier state than we are now. At the present time, a pilot of an ordinary machine, going up to try these curious and harassing manœuvres, might be likened to a child leading out a fierce wolf-dog for a walk. Things perhaps, one day, went wrong, the dog pulled the child over, and very likely turned and gobbled him up. What was wanted was the aeronautical equivalent, if he might say so without disrespect, of their Chairman exercising his pug dog.

Squadron Leader Hill, replying to the discussion, said that all the speakers had been very kind to him, and there was very little for him to do except to thank them. In reference to the remarks of Wing-Commander Cave-Browne-Cave, he would certainly try to get the diagrams illustrating the lecture; he believed that they were at the R.A.E. He was inclined to agree with the advisability of using the terms "port" and "starboard," and considered that it would possibly bring the terminology into better line. He wanted to make it clear that the pilot must

refer to control either from his own point of view or, at any rate, from some consistent point of view.

Wing Commander Cave-Browne-Cave said it was necessary that the terms used should be in reference to the position of the pilot.

Squadron Leader Hill suggested that difficulty as to terms of reference might arise if two aeroplanes were flying together and one was inverted while the other was right way up.

Wing Commander Cave-Browne-Cave considered that, when speaking of what a pilot was doing, it was very difficult to make a case for using anything but the terms that he appreciated.

Squadron Leader Hill said he thought the suggestion was a right and proper one. With regard to the stresses in inverted manœuvres, perhaps he had not made himself quite clear. He had not suggested that the present aeroplanes were not strong enough for inverted flight, but merely that the extent of their margin of safety was less certain than that for normal flight. The point he wished to emphasise was that it would be a good thing, now that we have some data on inverted flight, if the stressing were looked at from the point of view of inverted manœuvre, to see if it could be improved. As we got to know more and more about aeronautical engineering, aeroplanes constantly improved; but there was never a time, even in the early days, when a pilot was not prepared to try any manœuvre if he thought it could be done. He did not mean to say they did absolutely mad things; they flew to the best of their ability, but did not allow possible defects in the aeroplane to thwart them. He held that over-cautiousness was a policy that was actually less safe than pushing ahead keenly; for example, there were fewer accidents among people who were flying what he called vigorously, because by this means they came to have greater knowledge of their aeroplanes and were less likely to succumb to difficulties in flying. He would certainly suggest, therefore, in reference to the Service scouts, that they should be flown inverted, with the proviso that flying was carried out with the gentleness of hand which was taught to every pilot when he learnt to fly. On the one side we witnessed the engineer and scientist steadily improving the aeroplane; on the other the pilot exploring its possibilities and not wasting his talents. That was the way that progress had been made, and it was the only way.

SOME ASPECTS OF AERONAUTICAL PROGRESS

BY MAJOR-GENERAL SIR F. H. SYKES, G.B.E., K.C.B., C.M.G., M.P., F.R.AE.S.

A Paper read before the Royal Aeronautical Society (Scottish Branch).

It is very kind of you to invite me again to address the Scottish Branch of the Royal Aeronautical Society, and I am glad to do so at this the opening meeting of what promises to be a highly valuable session.

I will not excuse my temerity in coming here to-night for two reasons. First, since I am no longer in direct touch with aviation I do not claim first hand knowledge of detail, my interest in what we all believe to be one of the greatest of modern developments is still unabated. And secondly, because aeronautical progress must be assisted by open discussion of its difficulties, and it may be of value, whether you agree with them or not, to express some personal ideas from the outside angle of a year's private membership of the House of Commons as a preface to your more technical meetings.

The present Government has had a difficult air heritage to handle. Aeronautics, like many questions such as trade and unemployment, Empire consolidation and development, European conditions, reparations and the Ruhr, agriculture and the like, has reached an important milestone. Not least amongst such problems are those of communications and defence which, from now onwards, must largely depend upon the air. The issue requires great balance of judgment. A narrow-minded policy at this stage may lead to future disintegration; a broad, far-seeing one to the development and close linking up of the Empire.

The air, as we know it, has been evolved in the short space of fifteen years. Under right guidance it holds the developments of the future. As an instrument of war it will enforce operations in three dimensions. Air power may destroy the civilised world or at best render a country a menace to its neighbours. Air transport, as an agent of peace, can be a strong factor in the welding of civilisation. As the fastest locomotion ever known, independent alike of sea and land frontiers, it is bound to have a far-spread beneficial effect.

The civil and military, each with their independent *rôles*, are yet indissolubly intertwined, and any apparent stressing of the military side is only because the war, its requirements, and the fact that money was then of no consequence, played such a great part in bringing aeronautical progress to the position in which we know it.

There is not time to-night to deal in more than the barest outline with some aspects of the subject. But it is worth while to try to see something of what is or is not going on and, as far as possible, how we stand.

At the outset it is clear that all is not well with British air power. The experience of the past five years emphasises the fact that it is by research and operation that progress can best be maintained. Real aeronautical progress has been practically at a standstill since 1918.

Wide questions of politics have a direct effect. In 1919 there were two groups of problems confronting the air. First, the responsibilities for Imperial Defence and the inter-relation of the Navy, Army and Air. And second, how best to retain and utilise the necessary service portion of the instrument which the war had produced and, whilst looking upon the balance as a reserve, to turn as much of it as possible into an organisation of the greatest value to peace. That is to say, to hold the scales and to develop the two branches in their correct functions and proportions.

So far as the first group is concerned, Britain and the Empire are no longer protected by the sea, and air armaments have added greatly to the complications of Imperial Defence. In European warfare it will be impossible, until air superiority has been gained, for fleets to move, armies to mobilise and operate or the organisation of reserve resources of material and man-power to make headway. There are, in addition, the difficulties of defining the principles to which the services should direct their efforts; the extension of the areas of possible theatres of war; the increase of the vulnerability of the United Kingdom; and the necessity to secure the Empire and its communications as a whole.

Air power opens a new phase and sphere of war. Its radius, approaching 1,000 miles at 150 miles per hour, will increase and its action will be countered by nothing short of superiority in the air. An uncountered offensive will compel the removal of seats of government and naval, military and air bases beyond its radius of action. For these reasons, our first-line air strength must be equal at home to that of any other Power.

In this connection you will have seen an increasing number of articles supporting or denying the necessity for the Navy to have its own air arm and an announcement of the compromise recently approved by the Cabinet. There has been much controversy on this subject for the past three years. Personally, I think the compromise will be impossible of satisfactory working. A sound system is mainly dependent upon good organisation and correctly placed authority. It cannot rely entirely on goodwill. There is no doubt that the Air, the Navy and, in a lesser degree, the Army, have each a strong case in this matter. The real trouble is, of course, that the Air was allowed after the war to sink to an inadequate strength and to be ill-organised for its essential functions. Attack is the best defence and the primary requirement in war is an independent long-range air fleet supported by home defence units. Any factor which detracts from this must give way until reasonably adequate independent strength is assured. Theoretically, the arguments are in favour of all air being organised as separately as all sea or land forces, and it would obviously be unsound to revert to the old arrangement under which the Navy and Army had their own competing air services; but practically, on the other hand, their efforts will be ineffective unless they are allowed a more suitable system of control over the air units required for their particular purposes than has been the case. A generally satisfactory solution is almost impossible under any financial conditions that are likely to apply in this country, but I think it will be found necessary, sooner or later, to permit the Navy to have at all events a trial test of its scheme to run the personnel of its own air units; the control of the independent air force, of research, experiment and supply remaining with the Air Ministry.

A serious factor is that difficulties between the fleet and the air will tend to increase rather than diminish if matters are allowed to drift owing to gradual loss of touch by the air and the disappearance of those ex-naval officers now with the R.A.F., who have been mainly responsible for whatever has been effected by the R.A.F. naval air units. Already the last ex-sailor on the Air Council is being replaced by an ex-army officer, while three at least of the comparatively small number of senior ex-naval R.A.F. officers are on the half pay list.

As far as work with the Army is concerned, co-operation with the artillery is said to be good, but close reconnaissance poor owing to lack of knowledge by the observers of ground tactics and troop operations.

Let us turn to the second group of problems—the internal re-organisation of the air on a systematic, proportional, progressive policy.

The war left us with the finest flying service in the world; we are now aerially defenceless against at least one Power.

In war the Air must strike before the Navy and Army and will immediately suffer heavy casualties; the percentage of fighting men in the R.A.F. is very

small; only a fraction of the total air establishment do any day-by-day work in the air, and we have no reserve of trained personnel to fill the gaps.

Vast supplies of aircraft material must promptly be forthcoming; the British industry, the greatest and best equipped in 1918, has shrunk to attenuated, ill-nourished proportions, whilst that of France has been retained at a considerable size.

In the event of war there will only be time to carry on in production with designs which have been produced and tried out in peace; comparatively little has been done in this important aspect, whilst operational experiment also is stagnant. America has taken the lead in research and experiment—witness her recent success in the Schneider Cup contest at Cowes—and France in operation.

Air power, like sea power, and in this we have no less an authority than Captain Mahan, must intrinsically be based on a broad reserve of commercial effort and achievement. Expenditure on a purely military basis involves an ever-increasing deadweight as defence necessities increase; expenditure on a military spear-head and civil-shaft basis promises an eventual provision of air defence force at a minimum State outlay. Air transport, quite apart from its value as a reserve, has the strongest of claims, and is not camouflaged militarism; the utility of a self-supporting mercantile air fleet is obvious, but the development of British commercial aviation by lighter-than-air craft has hitherto been disregarded and by heavier-than-air brought to a quite diminutive result.

Technically, aviation has a broad trinity of inter-dependent aspects—design, new construction and experimental and normal operation—no one of which in peace can progress without the help of the other. And service aeronautics must be dependent upon this trinity, unless it is merely to rely upon a crude effort to achieve its results by the unnecessary expenditure of vast sums of money. From the service point of view, as I see it, the principle underlying all these matters should be the maintenance of the smallest possible regular forces compatible with safety, and the largest and most easily obtainable reserves of personnel and material; the greatest allocation of mental and financial resources to research and experimental work; the carrying out of all possible duties on the most economical and efficient procedure—that is on commercial lines—thus allowing the net to be most widely cast for the necessary reserve power. This principle has been disregarded and war ratios have been maintained. It is difficult to arrive at a precise figure, but in the Budget for the last completed financial year the funds allocated for the fighting branch were in the order of £14,000,000 and those for research and operational development were £2,000,000, or say seven to one, and even of this sum only a small fraction was devoted to true research. The proportion adopted by France, on the other hand, is about three to one.

In this connection it is, I think, regrettable that, as a matter of administration, the division hitherto made in Air Ministry estimates between expenditure on research and expenditure on service construction has been eliminated with the possibility of a departmental transference to meet excess expenditure on service standard equipment of funds intended for research.

The first question of policy to be decided is whether Britain really believes aviation to be worth while or not. The main problem is, of course, largely one of finance. Progress in any of the applied sciences is almost entirely governed by the amount of funds available to meet the cost involved. But even if the total sum granted is too small there is always a right and a wrong policy for the allocation of whatever is available. As a rule, if Great Britain does a thing at all she does it better than any other country. But in this matter the comparative results are that in 1922-1923 with a budget of about £18,000,000 we had thirty-two squadrons and, in size, a negligible "trinity," and France with an expenditure of 600,000,000 francs, or £12,000,000 at 50 francs to the pound, had some 200

squadrons and, as a "trinity," a strongly supported technical and operational industry and reserve of personnel, material and design and manufacturing resources. Again, even within the number of our service squadrons, it would appear that those in India—not perhaps technically, yet undoubtedly morally, largely the responsibility of the Air Ministry—are still unsuitably equipped for their duties. Indeed, the whole of the Royal Air Force itself—for which the Air Ministry is responsible—is still equipped with the identical scouts, two-seater fighters, day bombers and night bombers which were in use five years ago. Furthermore, it is very doubtful whether any even single particular machine in the possession of the Royal Air Force could come within 50 miles per hour of the speed of 266 miles per hour recently attained by the American Curtis Navy Racer. The Americans are rightly looking for quality as against quantity.

The present position then can hardly be described as a happy outcome of five years' policy. The first political régime asked for time in which they could, as they said, "cut away the rank growth of war and well and truly lay the foundations" of British air power. At all events the policy had a nearly four years' run. Action on erroneous principles once started takes a lot of undoing, and whilst I think the present Secretary of State, faced with a most difficult task, has tried hard and done many things of considerable value in lesser ways, very little advance if any has yet been effected in major policy. He appears, indeed, as far as fundamental matters are concerned, to have accepted the situation and, with the same weight of military advisers, the country is still reaping the same results.

The methods adopted since the war, whilst entailing very heavy expenditure, have ignored the basic necessities of progress.

We are now, it was announced in the House of Commons on 26th June last, to have an additional 35 squadrons for home defence at an immediate annual cost of £500,000, rising in three years to an annual cost of £5,500,000, and since this date a further increase up to 52 squadrons has been promised, but the requisite financial outlay has not yet been mentioned. No statement has been made as to any consequent increase in research while the unfortunate elimination of a separate research vote effected at the same time, lends itself to a raid on the pre-existing research mite to assist in meeting the cost of equipping these squadrons. I agree entirely in the necessity for additional squadrons, but further to reduce the proportion of air funds devoted to research or to take away the little which research hath, whatever holy writ may say on this score, is a continuance on even worse lines of the policy adopted since the war.

As taxpayers, we want the least amount compatible with safety spent on defence, but also as taxpayers we have a right that what is spent is utilised to the best advantage. Financial stringency has properly reduced the air vote to the lowest possible level, but the bulk being allocated to first-line defence, first-line defence wrongly and detrimentally has determined the general rate of aeronautical progress.

These being the results it would seem that the allocation is unsuitable; that though the basis of air work is flying, the greater proportion of those in the Air Force do no regular flying; that insufficient importance has been attached to purely aeronautical development; that insufficient funds have been expended in research, and that, in the absence of material orders for new equipment for Air Force units, constructors have been unable to finance experimental work and progress has been checked at its source.

Since the war the progress of service aeronautics has amounted to little more than the production in quantity of types which were in existence in an experimental stage in 1918.

The only material additions are the development into troop-carriers of the earlier large commercial passenger machines; the development for service uses of

the Amphibian which was first brought into being at the Civil Aviation Competition in 1920; and, in the case of lighter-than-air craft, the development of the mooring-mast during the short period in which airships were under civil control.

Efforts have also been addressed to producing efficient fleet co-operation machines in which we were undoubtedly very short at the end of the war, owing to the requirements of the Army. In the majority of cases these machines are fitted with engines which—in their early stages in 1918—have recently been sufficiently developed to render them suitable for service equipment.

Design, Research, Experiment and Operation

Let us now turn to the many fields of great potentiality which await us, but which can only be furthered by work and money laid out on the soundest basis of development, that is, upon design, research and development derived from experimental and operational use.

Research brought aeronautics into being and its progress much depends on full-scale and model experiment. A great deal has been and is being done by the Aeronautical Research Committee, the N.P.L. and R.A.E. within their financial limitations, but more and more research is necessary merely to maintain, let alone advance, the present rate of progress. Expenditure on research is one of the best of economies in life and material. It would repay us considerably to increase rather than to "axe" its scope. It and its operational trying-out, day in and day out, by civil flying are the seed, root and trunk of positive as against negative direction.

But while I think the proportion and net sum allowed for research have been too small, additional mistakes have been made. For instance, disregarding the necessity that the work of the designer, constructor, experimenter and operator should be sympathetically co-ordinated by an independent sound homogeneous governmental organisation of good technical and administrative calibre, the existing government department responsible for research is subservient to the fighting department of the air staff of the Royal Air Force. Then, too, instead of concentrating on aerodynamic and engine essentials and an increase of purely experimental flying, much of the small sum of money research has received has been wasted in the production of military fitments, the provision of which is of secondary importance. The production of new experimental types of aircraft has been delayed and their cost increased by the insistence that such machines should be fully fitted with the latest military equipment before trial is made of their aerodynamical efficiency. Covered with gadgets, machines take the air for their trials long after such tests should have been completed, and the expenditure and trouble devoted to their military equipment is wasted as the aircraft are not fit for issue to the service until the normal process of elimination of defects is completed by the construction of modified machines.

Two reforms in organisation are desirable if research is to receive the attention it deserves. First it should be under the immediate control of a technical official of high scientific attainments employed on a civil basis, and equally important it should be administered by a civil staff of technically qualified officials promoted on merit within their own department. Co-ordination of service interests with the progress of research could be adequately secured by attaching a succession of serving officers to this department.

The greatest technical requirements are those of the rapid development of the aeroplane as a safe, reliable and economic vehicle; greatly improved control; the improvement of useful load and range; increase of speed in getting off; reduction of head resistance; reduction of landing speed, elimination of stalling and nose-diving; increased reliability and durability of engine; reduction of fuel and oil consumption.

Then in the important matter of engines, effort has been solely concentrated on the production of high performance engines of military type which were in the experimental stage at the end of the war. The results, if slow, have certainly been good, as witness the high-powered 1,000 Napier "Cub," the 600 Rolls-Royce "Condor" and the air-cooled radial engines, the 350 to 400 Bristol "Jupiter" and the 350 Siddeley "Jaguar."

The only addition which occurs to me is the Beardmore heavy oil engine, but this is not yet in production, and in view of its very high importance for civil development in a probable large reduction of running costs should, I suggest, have received governmental assistance on a bolder policy.

The demand for a really commercial engine has been consistently side-tracked. The numbers of such an engine required are too small as yet to permit a manufacturer to recoup himself for the heavy initial outlay. An aero engine is necessarily a very costly article, and it is in just such a case that assistance can very usefully be given.

All existing engines, excellent in their way, were designed for military purposes, and lasting qualities were therefore sacrificed to light weight and high efficiency.

Safety depends primarily upon keeping the air. Endurance, simplicity and accessibility are qualities for which air transport firms would willingly pay in quite considerable increase of engine weight and consequent reduction of paying load. When it is realised that the 3s. per ton mile, which may be taken as the present minimum cost of air transport, includes something over sixpence per mile as the cost of engine maintenance, inspection and overhaul, and that, in addition, a large percentage of the comparatively high insurance rates charged are due to lack of confidence in present engines, the importance of suitable engines is apparent.

There are fortunately some signs that the importance of this matter for the development of commercial aviation is now being realised at the Air Ministry. But it requires bold handling to achieve the necessary results.

Taking it all round and considering the lack of support given, it is remarkable how much, though greatly less than should have been possible, has been achieved.

Two lines of research, for instance, in which progress has been made are those of the slotted wing by Mr. Handley Page, which gives improved performance and control, and that of metal construction, of which more than one type of aircraft is now undergoing prolonged trials, and of which there is no need to stress the value for use in the tropics or under conditions of widely varying temperature and humidity.

As regards the development of lighter-than-air craft events have, it would seem, compelled a reversal of the official attitude, and the Government have announced their intention to try out the modern airship, with commercial co-operation —under commercial control—on the long distance oversea routes. Technical details of the proposals have not yet been announced, but I would sound a note of warning that where the unit of operation is so large, costly and experimental, it is vital that however slowly development takes place, whatever allocation of funds is necessary, only the best equipment that research and science can give, whether on the ground or in the air, should be employed if the undoubted potentialities of the airship are to be fairly explored.

Our experiments have been carried out in the past with ships designed to out-manœuvre an enemy whose constructional and operational experience was greater than ours. The Pulham experiments, though clearly demonstrating the efficiency of the mooring-mast, which is essential to commercial running, were hampered by the opportunist nature of the equipment which was employed. With plant designed with every care for the purpose for which it is to be employed,

and provided that all concerned in the operation of the ships face from the outset, the fact that minor set-backs must be expected and be overcome, progress as in the case of heavier-than-aircraft should be definite and cumulative. The recent five-day flight of the French (late German) Dixmude is evidence of the potentialities of the airship, and progress is also marked by the new 400 horse-power Maybach airship engine which recently completed a continuous 29-day bench run at full power, at a fuel consumption of .403lb. per horse-power hour.

In America unlimited expenditure on a wide field of research and development has supported the efforts of designers and there has, in consequence, been a great stride in general aeronautical progress in the United States, where the performances of latest types far surpass those achieved in Europe. There are many examples; as a recent instance, America has secured first and second place in the Schneider Cup race. She holds the world's records for speed, altitude and endurance. This is entirely due to the way that she has concentrated on research. She is, incidentally, the only country equipped with a compressed air wind tunnel in which results can be obtained directly comparable with full-scale without the need for "scale effect" correction.

It is in the U.S. also that the development of the airship is being seriously pushed forward. Whether or no there is truth in the Press reports of an immediate trans-Atlantic service, it is clear that America is prepared radically to tackle the various problems concerned and generally to utilise the experience of the world.

The progress of air operation in this country since the war has been hampered, equally if not even more so than research, by an inadequate governmental and official length and breadth of view and realisation of the importance of the early development of a healthy civil transport industry.

There is need for more thorough full-scale tests of progressive types by commercial operation. Military operation affords a comparison with the last best. Civil operation provides a striving towards the next best.

The reason that air transport has not succeeded more rapidly and extensively is insufficient steady support (*a*) by the State and (*b*) by the public.

If the State believe in the necessity for aviation they must bear to a greater extent (whether by increased orders to firms or by indirectly assisting firms to meet costs) the expenditure involved by firms in experimental and research work.

Orders should be boldly and freely given to the construction industry to assist in the evolution of progressive types and these should be lent to the transport industry to try out in daily operation.

The little that has been done has, however, been well done. The existing routes to the Continent show a steadily progressive trend in regularity and in traffic. The recent tragic accident on the Manchester route emphasises by contrast the remarkable degree of safety that has been reached in the operation of the services. No such accident had occurred for over two and a half years during which over 1,500,000 miles were flown by British air transport firms.

The service to Berlin, in spite of international difficulties preventing it from becoming a regular daily service, has already shown by its results that it fills a commercial need, and the attraction of a seven-hour journey against one of 24 hours by rail and sea is meeting with response from the travelling public.

A beginning has also been made with a very important line of development; that of an oversea service by flying boats. This development—the Southampton-Channel Islands experimental service—is carried out in a zone subject to some of the worst flying weather in the world. The equipment used on the service is standing up to its work, and the experience gained in surmounting the difficulties, whatever its immediate financial results, will be of great value on more distant Imperial routes.

The service from Manchester has been disappointing owing, I think, primarily to conditions of bad visibility to which Manchester is subject.

The experimental mail service between Plymouth, Manchester and Belfast is one of promise, and definitely corrects the disadvantage under which these northern cities suffer in receipt of the American mail as compared with Continental competitors.

I gather from the Press that this experimental service has re-raised the question of communication between Belfast and Glasgow, and the establishment of a service taking one hour to one and a half hours should not present insuperable difficulties.

On the oversea routes the R.A.F. service between Cairo and Baghdad continues. It carries out a useful function, but having military considerations primarily in view and being operated under military control, it is naturally costly and does not tend towards development to a commercial basis.

Development elsewhere on the Imperial routes appears to have been held up pending decision in regard to the airship scheme. Malta aerodrome has, however, been completed and the cross Mediterranean facilities it offers will tend to induce air traffic along the Malta route to the East and assist Malta to attain in the air the position she holds on the British sea chain. Similar commercial air ports should be organised on the Imperial routes if British air power is to be free to operate in the defence of Greater Britain.

Here I would stress the importance of development of the Southern rather than the Central European route to the East.

The Southern route is the route to fine weather and stable flying conditions; it is the route upon which we can utilise British resources at British air ports and avoid the interference and complications which arise from unsettled international conditions now or in the future.

To reach the Near East by the Central European route, on the other hand, involves flight over France, Belgium, the Rhineland, Germany (or Austria), Czecho Slovakia, Hungary, Roumania, Bulgaria and Turkey, and leaving aside altogether present conditions which we hope are only temporary, the risk of future complication caused by international difficulties to an air service established on such a route as this is very considerable and very serious when such a service depends entirely on smoothly effected connection throughout its length for its efficiency.

Incidentally, in my opinion, the established mobile squadron organisation for home defence should be overhauled; non-flying personnel should largely be found by an auxiliary civilian reserve, facilities should be given for the training of pilots for and in civil transport and as many aerodromes as possible, other than experimental grounds, organised on a civil basis.

Turning to another aspect, air transport has gained considerable experience on the business side and in the improvement of the technical details of organisation. Progress in efficiency of administration is a development of the utmost ultimate importance. As regards the machines employed, the main progress following the adaptation of war designs to commercial work has been the production and try-out of the first, and as such, the first experimental commercial aircraft; useful load per horse-power has been increased, comfort bettered and noise to some extent reduced; while many of the causes of engine failure have been eliminated by the redesigning of fuel and cooling systems on operational experience.

A great deal, however, remains to be done to make machines more fool-proof and reduce the effect of possible human error, present in every form of transport, to a minimum.

As far as personnel is concerned, routine medical examinations of civil pilots are carried out every six months, and as the statistics so obtained increase, it becomes more evident that it is possible to look forward to a long flying life for the majority of pilots who take up this work as a profession.

One of the most serious results of the so-called policy of "cutting away the rank growth," however, has been the disintegration of the nation's pilot strength.

Refresher courses for ex-pilots on the Reserve have now been started at civil schools, but because of the absence of flying facilities which has occurred in the meantime for those pilots who did not remain in the R.A.F., the air industry, small as it is, is already hard put to it to carry out its services. And, while reserves of marine craft pilots have been allowed practically to disappear, no action has yet been taken to provide marine flying courses under the Reserve.

There is a great deal yet to be done, but the reliability of the air services shows steady improvement, and although the best of the existing aero engines still leave occasion for very considerable reduction in running and maintenance cost, weather or rather low visibility is the chief obstacle to regularity.

Improvement in material securing a high degree of inherent stability and controllability and a sense of confidence against breakdown will go far to counter bad visibility, but so long as the human element is employed progress in mist penetration is necessary if regularity equalling that of rail transport is to be attained.

Night flying tests have been carried out and have shown that the problem of night flight is the problem of daylight flying with the fog, mist or low cloud difficulty intensified. The ground organisation laid down after the war on the London-Paris route has proved, in the absence of conditions of low visibility, sufficient for regular operation of night services.

The importance of overcoming this remaining difficulty, caused by conditions of bad visibility, cannot be over-estimated, for until night flying is a regular practicable proposition the hours gained in the day are largely lost during the night, and the advantages of the air's speed are, with few exceptions, negatived in the transmission of mails which are the heart of commerce and for which the organisation of commerce utilises the night.

Whether, therefore, from the point of view of securing complete regularity in daylight air transport or from that of bringing into being a 24-hour flying day, there is ample scope for research and development in solving this problem.

Perhaps the most noticeable progress in commercial aviation is shown by the figures of costs. Two or three years ago exceptionally favourable conditions alone would permit running cost to be brought as low as 10s. to 11s. a ton mile; during this last summer the over-all cost under the best conditions in at least one instance, I believe, averaged for a month under 3s. per ton mile. This result demonstrates one of the advantages of the principle of "no monopoly." It is true that duplication of overhead charges involves expenditure which has to be met as a cost of competition. But, on the other hand, reduction in running costs is largely due to the system of intensive use of plant introduced into the industry by competition. Any tendency to carry to excess that intensive use must be carefully watched, but the fact remains that the principle was introduced by the competitive system, has been adopted in principle by all operating firms and is responsible, together with design developments, for a large proportion of the reduction in running costs which has been effected. That is not a result which would have been compelled by any foreign competition we have been called upon to meet.

Personally I am against the monopoly in principle. If the problem were merely one of purchasing the greatest possible number of flight miles in the current year, then support to a monopoly undertaking should give the greatest

results for available expenditure. If, on the other hand, the industry can still show advance in economic running, then the competitive system, though it involves a more patient waiting for mileage achievement, will more speedily attain to running on the most efficient and economic footing. And when the stage is reached of carrying out services on a self-supporting basis, then expansion of mileage and utility is automatic and almost limitless. Such expansion and the opportunity of becoming the world's air carrier will be the reward of the nation which first achieves economic air transport.

However, the Air Ministry have recently decided that the advance so far made should be consolidated and they are, therefore, about to place one firm in a monopolistic position by subsidising it alone. It is claimed that, whilst the immediate effect will be to eliminate British competition, in a short time the subsidised firm, if efficiently managed, should be on a paying basis without a subsidy, and that other British firms will then face it as unsubsidised competitors.

Whether State assistance designed to set the industry on its feet is given to a monopoly company or to two or more competitive firms, the manner of its allocation remains open.

The frankly military subsidy based on the quantity of aircraft maintained and of personnel on the pay roll can be left out of account as offering no hope of eventual discontinuance.

The remaining alternatives, payment on revenue earned and payment on mileage flown, have both been in operation in this country.

The former clearly offers the greatest incentive to operating firms. The difficulty in applying it is that neither the State nor firms have reliable traffic data upon which a fair percentage rate can be assessed. The result therefore must be that a rate too low involving unavoidable loss to the operating firm or one too high involving excessive payment of State funds is likely to be fixed. If the principle of subsequently revising the rate in the operating firm's favour is admitted, the firm's incentive to make economic progress is destroyed.

On the other hand, payment on a mileage basis, besides encouraging irregular and purposeless flying, suffers to some extent from the same difficulty in assessing rates.

The solution would appear to lie in a long dated guarantee of support on a scale decreasing annually, with a proviso that profits in excess of a reasonable return on capital should be returned, not to the Treasury, but should be devoted to increased research.

Consideration might also well be given to the possibilities of subsidising two groups, each with a zone, but with these zones overlapping and the overlap increasing annually until any monopolistic feature is eliminated.

In either event a factor of the greatest possible importance is the personnel of the Board.

As an instance of the progress of civil aviation in another direction the use of aircraft as a speedy and economical aid in fighting forest fires in Canada should be mentioned. In one case, selected from many similar instances, a serious fire was reported, the scene of which was distant two days' journey by ordinary methods. Using an F3 flying boat, which was operated to and from a lake close by, the fire was got under control by the morning after the first report was received.

On one journey this machine carried nearly 5,000lbs. useful load, and on its last journey from the fire carried twelve passengers in addition to its crew.

In the provinces of Ontario and Quebec also during last year over 19,000 square miles were surveyed, and nearly 800 square miles photographed, from the air.

A letter which I have recently received says:—

> "If every Government in the Empire were to adopt the policy of using aircraft in every service under their jurisdiction, where they could be efficiently and in the broadest sense of the word economically employed, our problem in regard to air power would be largely solved. We have only touched the fringe of the subject in Canada, but the results obtained in the useful forms of flying show what can be done."

Undoubtedly much could usefully be done on this principle, especially where there is no military air force, but if development to a commercial basis is to take place, it should be on commercial lines and in commercial hands. Civil aviation properly developed offers the prospect of a commercial undertaking of national earning benefit. To allow it to remain under conditions where it merely carries out services, even services of great utility, without prospect other than that of indefinite continued cost to the State, is to risk its existence as an industry.

Of development other than that of commercial air transport an important one has been that produced by the recent reversion to the use of gliders and low-powered aeroplanes. When the 50 h.p. or more engine first came into use some thirteen years ago a cry went up from some that improvement in aerofoils would largely be stifled by the production and use of high-powered engines. Their voices were drowned by others who delighted in the prospect of flying projectiles hurled through the air by the brute-force of many cylinders. But the result of the restrictions placed on German aeronautics after the war was to turn the minds of her scientists back to the days of Pilcher, Lilienthal and the Wright brothers. With all the knowledge of piloting since gained and which those pioneers lacked, the Germans took up again the study of gliding and their example was quickly followed in other countries. As in the case of the early gliders, the next step has been to fit low-powered engines to the aircraft so produced.

Provided that the engines so used are strictly limited in power, and progress is obtained by improvement of the aircraft, exceedingly useful development may be expected, the results of which can then be applied in the case of commercial and military aircraft. Indeed, what may be termed amazing results have recently been obtained at Lympne.

At the same time the development of the cheap light aeroplane, whilst of great use in many ways, and especially in improving upon wind tunnel tests and in supplementing full-scale aerodynamic experiment, must not be relied upon to take the place of full-scale experiments which, though they entail great difficulty, should be much increased.

Some evidence that the development of civil aircraft is beginning to be realised to be important is given by the recent decision of the Air Ministry to purchase new British aircraft which will win the Aerial Derby or the Schneider Cup.

True, the aircraft in question would be of a racing type purely and simply. But for these races will be developed the latest ideas of designers for producing high speed aircraft, unfettered by military restrictions or any but a minimum of safety requirements.

Best methods of attaining speed will not be obscured by other factors, and from these special forms of aircraft will be bred the high speed fighting scouts of the future.

Without such an opportunity of recovering at least a portion of their outlay, few constructors can afford the expenditure necessary for entering for such events as serious competitors. Success in such widely advertised races, on the other hand, brings in its train foreign orders for the industry of the winning nation.

An example of this was the wide demand for Rolls-Royce engines after the series of long-distance performances for which they were used in 1919 and 1920; while the more recent fine performances of American aircraft had their repercus-

sion in a steady flow of orders for the Liberty engines held for disposal in England. The Air Ministry assistance, which enabled the British aircraft industry to provide their excellent exhibits at the recent Gothenburg Exhibition, will tend to attract more orders for British firms and thus provide such firms with further resources for carrying on their own lines of development.

This policy which has achieved success in Japan and in Sweden is capable of considerable expansion. A greater readiness to assist and encourage the training of foreign pilots in this country with British material, a greater State assistance, not merely in funds, to British training missions abroad and a broader view of the value of achieving success for British equipment in international events and international records are services which in their indirect results would amply repay the State. Civil missions such as that which went to Japan should be fostered, and expeditions, demonstrations and record-breaking assisted.

This is an aspect of development in which our French trade competitors have received far greater assistance than has been given to the British industry.

Air attachés abroad, foreign missions assisting sales and carrying out training with French equipment have their part in the development of the French air industry and at home State encouragement is still given to eliminating competitions for civil aircraft.

Speaking as I have been to the converted, I have not thought it necessary to postulate the utilities of the conquest of the air. I should like, however, to emphasise its importance in one direction. Year by year, almost day by day, events are forcing the conclusion that it is to the British Empire that we must look for our continued existence and for the well-being of the State and its citizens. Geographical conditions are the outstanding factor which tends to prevent fuller utilisation throughout the Empire of its resources. Air development can greatly help to surmount these difficulties.

I am as convinced as ever of the great future of aeronautics. Nothing can stop its growing importance to the nation and the world. No one who has the faintest understanding of the difficulties expects it to come in a flash. Its advance, though unparalleled, has been borne on the track of sacrifice and toil, and an immense amount of patient thought, courage and work lie before those engaged in it. It wants wise and adequate assistance. Too much is involved for it to be allowed "to worry through somehow." We can only hope that in honour to those who have gone, to assist those now in it and to come, and bring lasting benefit as we believe to the world, that the Government, assisted by the Imperial Conference now sitting in London, will give sound redirection, reinforced impetus and increased breadth of vision to its guidance.

ASSOCIATE FELLOWSHIP EXAMINATION,

September 24th-25th, 1923

Aerodynamics

(Time allowed, three hours.)

Six questions only to be answered.

1. Explain, with the help of diagrams, a method of estimating the lift of a wing from the surface pressure distribution. If the method were extended to the question of drag, what discrepancies would you expect and how would these arise? Briefly describe a scheme for obtaining the measurements in actual flight.

2. Define "Drag coefficient." Go carefully into the question of whether this quantity is a constant, establishing any formula you may use and illustrating your answer by means of an example.

   In comparing the drags of airship envelopes a different coefficient is commonly employed. Define this and explain its advantages.

3. State Bernoulli's theorem.

   The static pressure at a certain point above an aerofoil held in a wind channel working at 60ft. per sec. is observed to be less than that of the undisturbed stream by 1.2in. of water. Calculate the velocity at this point, stating the assumption you make in applying the theorem. Explain why a similar calculation applied to a position immediately behind (*a*) the disc of rotation of an airscrew, (*b*) a bluff obstruction in the airstream, would lead to error.

4. Sketch the section in general use for streamline wires. Discuss the considerations leading to the choice of this section. What other factors should be taken into account in choosing a section for exposed struts?

5. An aeroplane weighing 3,200lbs. has a landing speed of 40 m.p.h. The wing system gives the following results on test :—

Lift coef.	.093	.171	.243	.299	.465	.505 (max.)
Lift/drag	7.0	11.6	13.6	12.7	9.3	5.5

   A 1/10th scale model of the remainder of the aeroplane is found to have a drag of 0.85lb. at 60ft. per sec.

   Assuming the thrust h.p. developed to be 160, estimate the maximum horizontal and climbing speeds at low altitude.

6. The thrust diagram of a two-bladed airscrew, 9ft. diameter, may be taken as triangular; the thrust being zero as far as 9in. radius and at the tip, and having a maximum value at a radius of 4ft. The airscrew is fitted to an aeroplane weighing 3,000lb. whose overall L/D ratio at 120 m.p.h. is 6.7; 200 B.H.P. is required at this speed, the airscrew turning at 1,400 r.p.m.

   Find (*a*) the thrust per ft. run at a radius of 3ft. 6in.

   (*b*) The apparent increase of angle of incidence required at this radius to allow for inflow.

   Take the ratio of the inflow factor to the outflow factor as 0.35.

7. A monoplane, 36ft. span, weighing 1,450lb., has a speed range of 45 to 80 m.p.h. The lift coefficient varies uniformly from zero at 3° to 0.52 at + 10° and its maximum value is 0.65. While at full speed the machine is given an angular velocity of 0.3 radians per sec. about its longitudinal axis. Calculate approximately the instantaneous value of the rolling moment damping out the disturbance.

8. Briefly discuss what is implied when an aeroplane is said to be "inherently stable." Explain the function of a dihedral angle on the wings in contributing towards stability.

9. Describe fully a method of testing the maximum speed and climb of an aeroplane. Your answer should be as complete as possible and include notes on the reductions you would make to standard conditions.

Mathematics

(Time, three hours.)

Full marks will be obtained by answering correctly *five* questions or their equivalent.

1. A simple pendulum is allowed to oscillate under gravity from an initial position of rest and angular displacement a. If the medium exerts a resistance on the pendulum bob proportional to the velocity, determine how the successive amplitudes of vibration diminish.

2. A motor car, left unattended on a long incline of 1 in n, starts to descend under its own weight. If the combined road and air resistance expressed in lbs. per unit mass of car is found to be $R=a+bv^2$ where v is the speed in ft./sec., find the speed attained after any time t, and the limiting speed.

3. Define:—Moments and Products of Inertia; Principal Axes of Inertia.
Given one Principal Axis at a point of a rigid body, show how the other two may be found.

4. State precisely what you understand by the terms:—Equilibrium; "stability or instability to small disturbances," describing the process and principles you would utilise to investigate these conditions.

5. Explain the dynamics of gyroscopic action, treating a simple case in detail.

6. Explain what is meant by a determinant and show by an illustration that the product of two determinants of equal order is itself one of the same order.
Prove

$$\begin{vmatrix} x^3 & x^2 & x & 1 \\ a^3 & a^2 & a & 1 \\ \beta^3 & \beta^2 & \beta & 1 \\ \gamma^3 & \gamma^2 & \gamma & 1 \end{vmatrix} \equiv (x-a)(x-\beta)(x-\gamma)(\beta-a)(\gamma-\beta)(a-\gamma)$$

7. At noon a person standing on a cliff h feet above sea level observes the altitude of an airship in the plane of the meridian to be a, and the angle of depression of its shadow on the surface of the water to be β, the sun being behind the observer. Show that if γ be the sun's altitude at the time of the observation, the height of the airship above the surface of the water will be

$$h \sin\gamma \sin(a+\beta)/\sin\beta \sin(\gamma+a)$$

8. If $x=a$ is a first approximation to a root of the equation $f(x)=0$ show that a second approximation is $a-f(a)/f'(a)$. Hence, or otherwise, find the roots of the equation

$$5x^3+2x^2-3x+1=0$$

correct to two significant figures.

9. Describe the method or methods you would adopt to determine the value of a definite integral of a function you could not integrate formally.

Illustrate with

$$\int_0^{\frac{1}{2}} dx/\sqrt{(1-x^4)}$$

10. Evaluate

$$\int_0^a dx\sqrt{(x^2+a^2)}$$

$$\int_0^{\pi/2} \sin^n x dx\ .\ [n=+ve \text{ integer}]$$

11. Solve the equations :—

$$d^2x/dt^2-2a\,(dy/dt)+b^2x=0$$
$$d^2y/dt^2+2a\,(dx/dt)+b^2y=0$$

12. Show that if any function of the complex quantity $x+iy$ be arranged in the form

$$U(x, y)+iV(x, y)$$

where U and V are real functions of the variables x and y then U and V are separately solutions of the equation :—

$$\partial^2u/\partial x^2+\partial^2u/\partial y^2=0$$

Deduce that $U=$constant and $V=$constant are systems of orthogonal curves.

Interpret these equations and properties in terms of any type of physical problem to which they apply.

ASSOCIATE FELLOWSHIP EXAMINATION,

September 25th-26th, 1923

Strength and Elasticity of Materials and Theory of Structures

Eight questions only to be attempted.

1. Explain the terms "limit of proportionality," "resilience," "hysteresis," "proof stress," "permanent set."

2. Describe a method of determining Young's modulus by a beam test in such a way that the deflection due to shearing forces is eliminated. Deduce a formula for determining E in terms of the deflection of the beam and the known properties of the beam.

3. Sketch and describe the apparatus used for applying the Izod impact test. Discuss the value of this test in connection with wood for aircraft construction.

4. An encastre beam 10ft. 0in. long is symmetrically loaded by two concentrated loads each of three tons placed at distances of two feet from the supports. Sketch the diagrams of bending moments and shearing force and give the maximum positive and negative values in each instance.

5. Deduce a formula for the maximum fibre stress in a slender strut when the load is applied eccentrically.

6. Obtain an expression for the maximum bending moment in pin-jointed member which carries an axial thrust and a concentrated lateral load at the centre of the span.

7. Describe the conditions of flight for which it is necessary to design the structure of an aeroplane.

   State the nature of the forces in the following members for each condition :—

   (*a*) Top front spar.
   (*b*) Bottom front spar.
   (*c*) Flying wire.
   (*d*) Anti-flying wire.

   Discuss the effect of positive and negative stagger on the nature and magnitude of the loads in the wing structure.

8. Determine an expression for the maximum shear stress in a box spar subjected to a given shearing force.

9. The fuselage structure shown in Fig. 1 is erected with no initial tensions in the bracing wires. Obtain by calculation the forces in the members AB, BC and AC when a load W is applied as shown.

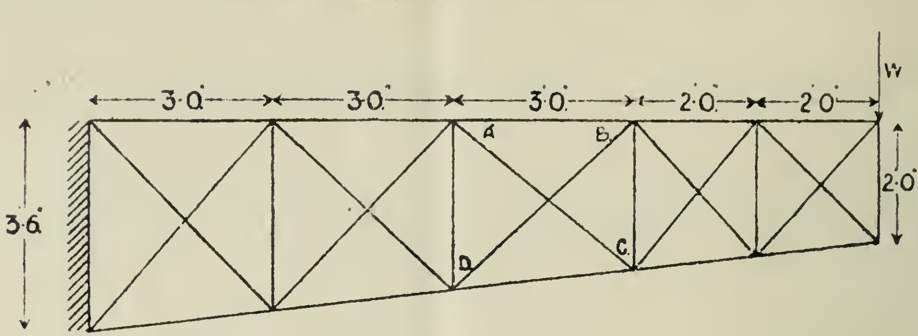


Fig. 1.

10. Obtain expressions for the minimum number of members required to brace a plane and a space frame respectively. If these conditions are fulfilled is this sufficient to guarantee complete bracing? Illustrate your answer by examples.

11. Describe in detail any method for determining the forces in a pin-jointed structure which contains redundant bars.

REVIEW

Over the Balkans and South Russia: Being a History of No. 47 Squadron, R.A.F.

By H. A. Jones, M.C. (Messrs. Edwin Arnold and Co.)

When the Expeditionary Force first crossed to France it was accompanied by four squadrons of the Royal Flying Corps.

At the end of the war there were 132 service squadrons on the various fronts, 16 home defence squadrons and some 200 training squadrons. In addition to the Western Front, where there were 100 squadrons at the date of the Armistice, the operations of the Royal Air Force ranged from the frozen forests of North Russia to the tropical swamps of the equator. The majority of these squadrons are now perforce disbanded and there is a real danger of their records as units being lost.

When the time came to decide which squadrons were to remain permanent a very difficult problem was presented which could never be satisfactorily solved. It is to be hoped that when the Air Force comes into its own all the war squadrons of the R.A.F. will be represented by worthy successors whose achievements can emulate but can never surpass those of the war-tried progenitors.

Unfortunately, the country's financial stringency has brought the production of squadron histories almost to a standstill, for no funds are available to assist in the cost of production. Publishers cannot be expected to take the risk of financial loss such publications may involve, much less the individual who may be keen enough to devote his spare time in writing the history of the squadron in which he was proud to serve during the war.

It is thus not surprising that only seven squadron histories have as yet seen the light of day, and their authors deserve credit for their courageous efforts.

Under the title "Over the Balkans and South Russia," Mr. Jones has written the history of No. 47 Squadron, a squadron which has a distinguished record of service in Macedonia and, after the Armistice, in South Russia. This squadron played no little part in establishing British air prestige in the Balkans. With Nos. 17 and 150 Squadrons it established an ascendancy over the enemy air services from the Gulf of Orphano to the banks of the Vardar. Equipped with machines which were always second rate owing to the demands of the Western Front, these squadrons displayed that indomitable persistence which earned them the profound respect of their foes.

The final offensive on the Macedonian Front, which began on the 14th September, 1918, and which resulted in the complete rout of the Bulgar, was the beginning of the collapse of the whole of the far-flung German forces. Next to Palestine this campaign affords a classic example of the influence of air power on a retreating army.

On the 21st September, the day when the Turkish Seventh Army was being shattered in Palestine, a reconnaissance by No. 47 Squadron established without doubt the fact of the Bulgar retreat. According to Mr. Jones

> "Our machines flew over in relays, and from low heights exhausted the ammunition from the machine guns against the unhappy Bulgars and bombed the columns of his transport, causing endless confusion. In many places the roads run alongside ravines and whole masses of transport were blown down these ravines, to crash in the gully below. At other points the roads cut through the mountain side, and bombs dropped here piled twisted lorries

> and dead animals and men high on one another, blocking the way to the oncoming columns which, ever pressing on from the rear, blocked the line backwards. These congested, panic-stricken masses offered still better targets to our machines, of which every advantage was taken."

The position is well summed up in a telegram forwarded on the 26th September by the XVI. Corps to G.H.Q. :—

> "The routes from Cestovo Valley to Kosturino show signs of indescribable confusion that must have existed in the retreat of the Bulgar Army. Guns of all kinds, motor cars, machine guns, rifles, and every kind of war material abandoned. Indicating that our R.A.F. must have contributed largely to bring about this state of things."

The book is well illustrated with maps and photographs and contains an introduction by Air Vice-Marshal Sir W. G. H. Salmond, K.C.M.G., C.B., D.S.O. Unfortunately a large number of places mentioned in the text are not on the maps and vice versa. Further, where names in the text do occur in the maps frequently the spelling is different. For example, Lake Langaza in the text occurs as Lake Langada on the map; Stojanova on the map is apparently Stojakovo in the text, and so on. This impairs the value of the book, making it difficult to follow the operations on the maps.

The book having been written from excellent official records appears to be free from errors of fact.

Mr. Jones served with distinction in No. 47 Squadron. On page 77 we find in reference to the work of the observers during the summer of 1917, "This reflected great credit on the squadron observers, of whom Lieutenants Boyd-Harvey, Harman and H. A. Jones were most active."

Mr. Jones deserves congratulations for having surmounted all the difficulties inherent in the production of a squadron history. His book is of an intimate nature, having the dash of a novel in parts.

"Over the Balkans and South Russia" will be read with pride not only by those members who served in the squadron but by the present members of No. 47 Squadron, in whose keeping the tradition is safe.